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THE CHEMIST;

OR,

REPORTER OF CHEMICAL DISCOVERIES AND IMPROVEMENTS,

AND

PROTECTOR OF THE RIGHTS OF THE CHEMIST AND CHEMICAL MANUFACTURER.

EDITED BY

CHARLES WATT,

AND

JOHN WATT.

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INTRODUCTION.

The completion of the Third Volume of The Chemist, naturally leads us to a retrospect and review of such events as have occurred during so important a revolution of time as a year; and when we consider the position in which we are placed with relation to our readers, we cannot but experience the highest degree of pleasure and satisfaction;—of pleasure, because our efforts and exertions, since the last annual occasion, have been followed by a success which evinces that our duties have been performed with a care and fidelity, that have met with unprecedented public approbation, and that, if we have not done all that we could have wished, still have we left no chasm to be filled up, nor omitted anything to the extent of our space, to deserve censure or remark: that, in fact, we have preserved to the work—that which, from the first, it has ever possessed, as a record of science in its rapid, continued progress and advances, here and abroad—the character of an accurate and faithful depository of all that is valuable and interesting in Chemistry, Chemical Manufactures, and Pharmacy; and that, whether we view the natural order and arrangement of the subjects in each number, the value of the articles, the importance and amplitude of the selection, the immense quantity of matter, or the numerous and original discoveries with which the Journal abounds in every number it is at once manifest that it has as justly assumed, as it has ably supported, the title of The Chemist;—of satisfaction, because we have added much to the treasures of chemical science, not to be found in any other work; have given to the public, in a compact form, all that has been done worthy of notice during the year, by British and Foreign Chemists; and, lastly, have received the fullest assurances, from every quarter, of the value of our labors with regard to the choice of matter, and to the independent and liberal character of the work, which, as a medium of scientific communication, or as the protector of all engaged in the subjects of its contents, stands without a rival.

On its first establishment, it was our determination to present to the scientific reader, all that is worthy of diffusion, and to form a medium for the promulgation of science as elaborate in quantity, as important in character, and at a price bringing it within the reach of every one; and, lastly, to seek by merit alone, for the approbation and satisfaction of men of science.

Three years' experience has indeed proved to us the value of an adherence to these principles, and its existence for that time has served to evince the high standing which the work has reached, and to satisfy all experienced in science, that it now rests secure in a character earned, confirmed and established, and is not necessitated to hope or seek for one. Any protestations or assurances for the future, are therefore scarcely necessary. We shall continue our labors as heretofore, with the strictest attention and increased vigor, to obtain the best matter, and by original communications, to add much to the amount of scientific knowledge, on all the subjects of this work.

We cannot but avail ourselves of the present opportunity of speaking of the works of L'Heritier, Dumas, and particularly of Liebig, as tending chiefly, and in a pre-eminent degree, to the elucidation of the dark and hitherto unexplored paths of Physiology, by the aid of Organic Chemistry, the importance of which, and the results which must arise from it, we had long ago felt, and frequently expressed. The last enlightened Chemist has indeed brought much that is new and valuable to light; and although he may, in some points, be feared to have arrived somewhat hastily at conclusions, yet has he conferred on mankind the greatest benefits, by the development of many new and important facts, illustrative of much that was long considered hopelessly hidden, and, more particularly, by bringing the minds of men of science to the consideration and study of these subjects. It is indeed difficult to suppose, that the connection of Organic Chemistry with Physiology, can altogether have escaped the observation of Physicians. Any alteration or change in the chemical relations of Organic bodies, must be accompanied by corresponding ones in their functions and actions, which it is the duty of Medical Men to note and ascertain, in order to trace their causes, and thereby prevent or relieve the diseases which they are calculated to induce. Neglect of these subjects can now no longer be excused, since ignorance of their importance, and immediate connection with the states and actions of health and disease, cannot be pleaded; it has, however, been reserved to this eminent individual, to show them its necessity, as a branch of Medical Science; and we trust, that although they cannot lay claim to the credit of any researches or discoveries of their own, they will not be guilty of slighting them, and the road which leads to further and more valuable results, will be made clear and more accessible to them, if they use diligence and application in their pursuits.

According to the plan adopted in the two first volumes, and to which we shall on all future occasions adhere, we now proceed to lay before our readers a *resumé* of the more important contents of the present volume.

The Section of "Chemistry" consists of a very valuable collection

of papers translated from foreign scientific Journals, and some original ones, communicated by talented contributors, or gleaned from the contemporary press, and reviews of many of the most valuable chemical works that have appeared during the year. Among the translations, we may mention,—some excellent Memoirs on the Fatty Acids, by Redtenbacher, Varrentrapp, Bromeis, and Meyer—translated from the *Annales de Chimie et de Physique;* M. Pelouze's Memoir "On the Combinations of Lead;" "On Lithofellic Acid, by Dr. Göbel; Brunner's "Description of some Processes for Analysing Atmospheric Air;" Erdmann's "Investigations concerning Indigo;" Erdmann and Marchand's repetition of Dumas' "Experiments relative to the Atomic Weight of Carbon," verifying the accuracy of the new one adopted by that Chemist; a valuable paper by M. Soubeiran, "On the Molecular Changes which Sugar undergoes under the influence of Water and Heat;" M. Frémy's "Investigations concerning the Metallic Acids;" M. Dumas, "On the Composition of the Air;" Professor Liebig, "On the Preparation and Employment of Cyanuret of Potassium;" "Investigations concerning Nitric Acid," by M. E. Millon; M. Bouchardat, "On the Proximate Composition of Fibrin; on Gluten, Albumen, and Caseum;" M. Longchamp, "On the Composition of Phosphoric Acid and the Phosphates;" M. Dumas' "Investigations concerning the Composition of Water," correcting the density of hydrogen established by Dulong and Berzelius; Guy-Lussac's important Memoir "On the Combinations of Chlorine with Bases;" M. Pelouze's "On a New Compound of Arsenic Acid and Bitartrate of Potassa, on Urea and Allantoïn;" H. Rose, "On the Action of Water on the Combinations of Sulphur with the Metals of the Alkaline Earths;" Prince Louis-Lucien Bonaparte, "On Valerianic Acid and the Valerianates;" besides many other excellent articles too numerous to be enumerated here, by the most eminent Continental Philosophers, among whom, we may mention AIME, WÖHLER, SOUBEIRAN, H. ROSE, BOURSON, DEVILLE, O. HENRY, FRITZSCHE, FORCHAMMER, MITSCHERLICH, REMIGIUS FRESENIUS, TESSAN, ORTIGOSA, FELHING, BARRAL, DUPASQUIER, POGGENDORFF, GAULTIER DE CLAUBRY, JACQUELAIN, RAMMELSBERG, NATIVELLE, VOGEL, WIGGERS, ELSNER, OTTO, and LAURENT.

The principal English Contributors to this Section are our respected correspondent, Mr. HILEY, of Elland, to whom we return our thanks, and Mr. REUBEN PHILLIPS, to whom we are indebted for many clever articles evincing considerable ability, chemical knowledge, and reasoning powers, and which reflect considerable credit on his industry and research. We hope that these talented gentlemen will continue to give the world the benefit of their scientific labors through the medium of this Journal.

In this division of our work, we have also given reviews of the most important chemical books published during the year; these need not to be enumerated.

In the department of "CHEMICAL MANUFACTURES," we have, with considerable effect, exposed some most important abuses and adulterations. The articles entitled "Abuses of the Alkali Trade," were directed to the exposure of a most infamous system of fraud, which received a considerable shock from our uncompromising, unqualified denunciation of it.* We also have had occasion to notice the adulteration of Tobacco, Nitrate of Soda, and Chromate of Potassa. We have likewise given in this section, papers on "Improvements in the Process of Tanning," by M. VAUQUELIN; "New Mode of Gilding, by the Humid Way and the Voltaïc Current," by M. LOUYET, being a translation of a pamphlet sent to us by that gentleman; M. VOGEL's "Memoir on Curcumine;" "Means of extinguishing Fires on board Ships," by Mr. HANCORN; with many interesting papers by DUMAS, LEVOL, NAYLOR, CHARBONNIER, LEMBERT, C. WATT, jun., KRANTZ, OSBORN, PAYEN, VOGEL, WISLIN, &c. &c.

We have to deplore the fact, that in "CHEMICAL MANUFACTURES" we have been able to record but little; but we intend to considerably increase this department next year, and are now making extensive preparations with that view. We shall be most happy to receive communications on all branches of manufacture from practical men. Much benefit must result to manufactures from laying before our scientific readers the manipulations of their various processes, as they will thus be submitting them to the consideration of a highly intelligent portion of the community, who will immediately perceive the improvements of which they are susceptible, and will apply their talents to that object. It is quite a mistake for manufacturers to suppose, that by publishing a description of their processes they are doing themselves injury. We solicit such communications from persons connected with manufactures, who need feel no diffidence on account of any deficiency, real or supposed, in their literary or scientific attainments, as we will cheerfully afford them our assistance in these respects, for so good a purpose. Nor need they fear taking up too much of our space; we wish all articles to be sufficiently long, and to go as much as possible into detail.

In "PHARMACY" we have given a great number of articles of considerable interest to the medical practitioner and druggist, to whom it is more particularly addressed. We have exposed the fallacies of that ill constituted body, the Pharmaceutical Society, which has more than once winced under our lash; which has sunk, and still is sinking, lower in the estimation of

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* This subject will be returned to on an early occasion.

the public, and a very large majority of the druggists of this country, and which is an object of compassion to the Continental Pharmaceutical Societies, to whose members that of Great Britain is indebted for any interesting papers that may occasionally adorn its "Transactions," appearing among the promiscuous trash with which they are encompassed, like so many gleams of sunshine illumining the surrounding darkness, or rather rendering it the more visible.

Among the papers in this section most worthy of consideration, we may enumerate,—"On the Iodides of Gold, with Remarks on the proportional Number of that Metal, and on the Medicinal Employment of the Protiodide," by M. Fordos; "Case of Poisoning by Arseniuretted Hydrogen," in which M. Brittan met his death by inhaling 150 cubic inches of that gas—communicated by Dr. O'Reilly, of Dublin; "Abuses of the Drug Trade," in five letters, from a very intelligent Correspondent, Pharmacopola; "On the Preparation of Medicinal Waters with Magnesia; Dr. Fife, "On the Use of the Ergot of Rye;" M. Guerard, "On the Effects of Cold Drinks on the Body during Perspiration;" "On Galbanum," by Ludwig; "On 'Clinkers,' a new Remedial Agent," by Mr. C. J. Edwards; "On Kusic Powder," by M. Habert; "On Nitrate of Mercury Ointment," by Mr. Cooley; "On the Preparation of Syrup of Saponaria;" "On the State of Mercury in Hydrargyrum cum Creta," by C. Watt, jun.; "On the Ferri Potassio-Tartras of the London Pharmacopœia," by Mr. Mowbray;" Dr. Holt's "Remarks on Pharmaceutical Nomenclature;" M. Guislain, "On the Influence of Impressions made on the Mother during Gestation, over the Conformation of the Child;" Professor Giacomini, "On the Effects of Sulphate of Quinine on Animals;" "New Mode of Collecting Lactucarium," by M. Arnaud; Cases of Poisoning, with Means of Cure in some instances, and *post mortem* Examination in others, by Aconite, Arsenic, Hydrocyanic Acid, Cyder containing Lead, Flesh of poisoned Animals, Copper, Digitalis, Binoxalate of Potassa, Laudanum, spoiled Meat, Squills, Quinine, Strychnia, and Cheese; Preparation and Therapeutical Employment of many new Medicines, and new Application of those already known. There are also many articles by Arnoldi, Osborn, Beasley, Pallaria Sallamea, Dr. Fife, Nauche, Boudin, Buchner, Tavignot, Menier, Meillat, Burdech, Berard, Deschamps, Cooley, Guastamacchia, Bouchardat, Prince Louis-Lucien Bonaparte, Markwick, Reinsch, Marchand, Ludicke Pollius, Lepage, &c. &c. &c. We have also reviewed some important works on Pharmaceutical and Medical subjects.

It would afford us much pleasure, in summing up the contents of The Chemist for the year 1842, could we pay to the scientific Chemists of this country as high a tribute of admiration as to those on the Continent. How little have the former added to our knowledge? How much the

latter? The columns of THE CHEMIST teem with the writings of foreigners; few English Chemists' names occur; are they ashamed of the comparison? They are bringing discredit on the country of Davy. Nothing can more clearly prove that Chemistry is making less rapid advances in England than on the Continent, than the fact that THE CHEMIST—the highest, the only, chemical work in existence—derives its supplies entirely from abroad. Let us hope for better days. The rising generation will, we have no doubt, make amends for the deficiencies of that which is passing away.

These complaints are made "more in sorrow than in anger," and we hope that those who value the scientific reputation of their country will endeavor to remedy the present lamentable position of matters.

It is now our duty to announce that, in order to render THE CHEMIST still more worthy of the extensive encouragement it has received, to introduce into it many valuable Scientific Memoirs by British, Continental, and Transatlantic Chemists, and with the view of still further elevating its rank and raising its character, already very high, we find it necessary to add another sheet to the work. THE CHEMIST, being very closely printed, and in double columns, contains exactly the same quantity of matter as *The Pharmaceutical Transactions*, which consist of four sheets, loosely printed in single columns, the former being charged Eightpence, and the latter, One Shilling. It is our intention to charge One Shilling for each number of the ENLARGED SERIES, which will contain one half more matter than the organ of the Pharmaceutical Society. In making this increase of price and size, we pledge ourselves that the purchasers of our Journal shall be supplied with all that is new and interesting in the various departments of CHEMISTRY, CHEMICAL MANUFACTURES, and PHARMACY, and that they shall not be under the necessity of resorting to any other work for information. The fidelity with which we have fulfilled our pledges for the three years of the existence of THE CHEMIST, will, we feel assured, be considered as sufficient to entitle us to confidence for the future.

So vast is the increase of scientific matter on the Continent, that it cannot be comprised within our present limits; the addition, however, will enable us to present to our readers such a mass of information, and of such value, as never was contained in any Journal of Science. This will cause us considerable additional labor and the expenditure of much money; of neither will we be sparing.

We must now conclude, expressing our deep sense of the liberal encouragement which it has been the unusual good fortune of THE CHEMIST not only to merit, but to obtain.

THE CHEMIST.

ADVERTISEMENT.

In commencing the third year of our labors, it is with no small degree of pleasure and exultation we announce to our readers, that The Chemist has continued to progress to an incalculable extent in the estimation of the public and the press, with every succeeding number since the completion of our first volume. From all parts of the United Kingdom, the Continent and America, has it been favored with testimonials of approbation and eulogy by far too numerous to admit of being here recorded: it is sufficient that we quote the almost universal language of every writer, that "The Chemist is a work which ought to be in the hands of every chemist and druggist, as well as of those engaged in chemical manufactures, and in medicine."

Our readers will bear in mind that this journal was established with the view of defending chemists and druggists from the aggressions and encroachments of the lower rank of the medical profession, and the chemical manufacturer from the schemes of deluded and designing men, by whom they are so often defrauded; and also to form an organ of communication through which to express their grievances and obtain a remedy, and more especially to provide them with a medium through which to receive and diffuse knowledge in science and the arts, as well as in all important matters of business connected with them. Whatever has at any time affected their interests, or threatened an invasion of or encroachment on their rights and privileges, The Chemist, their first and now their only disinterested organ, has been prompt and unremitting in its exertions to arouse their vigilance and aid them in their efforts to defeat the machinations of the enemy; and we may add, that the manner in which, during the last year, it fulfilled its pledges needs no notice here: the highly flattering expressions of gratitude, the numberless votes of thanks from all the provincial meetings of chemists, with their urgent requests to continue its advocacy of that cause which it had already so much benefited; and the deep sense of its usefulness to them, expressed by the most influential and enlightened of our metropolitan brethren, form the best evidences: while to us

they are sources of the highest gratulation, as being successful in the attainment of those objects, of the justice of which we were, and ever shall continue to be, fully satisfied; for we may here fairly put the question, what would have been the situation of the cause of chemists and druggists at the time when the apothecaries endeavoured to impose certain legislative measures on Mr. Hawes (who for his trouble has received so much dirty water), had not our journal formed a strenuous, extensive and recognized source of communication, devoted to the interests of that body for whose benefit it was first established? What periodical would have taken its stand against these sudden attacks, directed at once to the total destruction of the trade? On the contrary, almost all others, and particularly the apothecaries' tool, took up arms against it, and designated its members as "ignorant chemists and impostors," and heaped upon them numberless other epithets, from its ample stores of malignant calumny. Every effort was continually being used to induce the legislature to pass restrictive measures calculated to totally ruin this useful body; and the public were told almost every day that chemists were annually destroying thousands—nay, we may add, even more than uneducated and unlicensed *general practitioners*, of whom London alone contains many hundreds.

How far we aided in bringing about that measure which has since been adopted, and which we had hoped would have tended so much to cement the members of the trade in one well organized and united body, but which, alas, has terminated so grievously to our disappointment, that we can find no word that will demark its real character, except the vulgar and significant term "job," is now so well known, that observation is unnecessary, and is universally admitted, for it stands on record in the Advertisement to the first number of our journal. Thus, we had anticipated them in recommending its formation some fifteen months before any member of the trade took the trouble or saw the necessity of establishing a society; nor would it, we firmly believe, ever have been attempted, had not those worthy and intelligent members, Mr. Farmar, of Lambeth, and Mr. Dinneford, "put their shoulders to the wheel;" but we fear, like many others, the former gentleman in particular is not so disposed towards the present management of its affairs as long to remain its secretary: we speak from conviction—let time prove how correctly. In March last, on receiving a notice to the effect that a meeting of the trade was appointed to take place at the *Crown and Anchor*, in the Strand, we devoted the pages of this Journal (never indifferent to whatever is of importance to those for whose benefit and advantage this work is concerned,) to more vigorous and determined exertions, and to enable it to enter the arena with the enemy: the result was CONQUEST! For our zeal and services, as heretofore stated, we received the thanks of every provincial meeting. On the occasion of the second meeting took place the formation of the Pharmaceutical Society of Great Britain; to indicate the title of whose members by initials requires more than one fourth of the consonants of the alphabet.

Many of our friends, members of the society, were profuse in their expressions of gratitude for our exertions on behalf of the trade, and with such we were contented as marks of the success of our labors. We were not weak enough either to expect or desire any acknowledgments from the

Pharmaceutical Society; for if we had, we should only have placed ourselves in the situation of such as expect more from swine than a grunt of the snout. We now see that its meaning and object was to act shabbily by us, and to turn round and throw in our face a portion of the dirty water bestowed by it on Mr. Hawes; and we have long since been convinced, as no doubt he has been, that it is capable of bestowing nothing else.

We shall now give a brief account of the steps taken by the Society, showing how much they are opposed to those which ought to have been its chief objects, and which alone are in accordance with the interests of the trade, and are calculated to promote the better education of its members, and the greater diffusion among them of that knowledge which is more closely connected with their pursuits. That the conduct of the Council in allowing the proceedings which have been adopted, has brought the whole affair into ill repute, is too evident, from the information we received even at head quarters; and we have also had ample testimony of the fact, from the immense pile of letters which are daily heaped on our desk, coming from all parts of the kingdom, and more especially from the schism which we know to exist in the camp. Indeed, we are but too sure of the SPEEDY AND TOTAL EXTINCTION of this Society, and that, like the British Association, the pit of oblivion awaits it. Its chief functionary, and the enlightened secretary of the Parliament of Science, will, ere long, be shelved together, and have to congratulate each other upon retiring from their arduous duties, and on being about to enjoy the "otium cum dignitate."

This Society was established for the protection of the rights, and for extending the education, of the members of the trade. It has held monthly meetings or *soirées*, as they are termed, at a chemist's in Oxford Street: for what purpose do our readers imagine?—The legitimate objects set forth in the prospectus?—For the purpose of organizing proper measures to obviate the forthcoming legislative measure which will be attempted to be carried through parliament next session?—To establish a plan of education, more especially for provincial and country druggists; by far the most important and the most difficult of all subjects connected with the management of such a society? Our reply is, No; but, on the contrary, its meetings have the sole object of reading a *mélange* of papers—(save the mark!)—on pharmacy, and grouping them, and other promiscuous twaddle, into a medley, rejoicing in the characteristic misnomer of "Transactions!" If this production, which has at least one peculiarity, that it is of a style "sui generis," were intended as an opponent or rival of THE CHEMIST, we should indeed have little to fear; it would excite in us no concern or consideration, even were it as far above mediocrity as it is ineffably below it, in matter, arrangement, and literary merit.

The objects which have induced this deviation from the legitimate course and from the real interests of the trade, have our most unequivocal opposition, as they are not for the benefit of the whole, but for the aggrandizement and profit of the *select* few—we might say, for one individual. The motives which have led to this fine piece of show-off cannot be too highly condemned; and we regret that the Council should so long have been blind to its existence, or inert in its subversion. The subscribers have handsomely and liberally given their money for the purpose of bene-

fiting the trade in its most vital interests, and for its improvement, not to pay an Editor,—and that Editor one of the Council,—out of the funds of the Society,—a payment which we think shame alone should have prevented him from receiving or even asking; but we differ from the whole affair; as does every thinking member of the Chemical body, and most of our numerous readers, as well as the greater portion of the Council.

We would ask the question, Has the money subscribed been appropriated to one legitimate object for which it was intended or required? No. But we hear of periodical meetings, or *soirées*, held in Oxford Street, at which attend a few fee-seeking physicians, lending the *influence* of their *valuable* names for the ostensible and laudable purpose of encouraging the cultivation of Pharmaceutical Science, and discussing the point as to whether mercury is to be rubbed in this way or in that; and whether, like a cat's back, it cannot be rubbed against the grain without violent effects being produced.*

" Strange such a difference should be
'Twixt tweedle-dum and tweedle-dee."

The subscribers are justly indignant at being made to pay the exorbitant fee of two guineas per annum for the sole purpose of gratifying the ambitious cupidity and puerile vanity of one who seems, by his own method of rating his powers, to be very far indeed from having learned the important maxim, γνῶθι σεαυτόν. It may, perhaps, be ascribed to a monomania peculiar to some people, who have the same itching for " going a writing," as Mawworm felt when he exclaimed—" I wants to go a preaching!"

" Scribimus indocti doctique poemata passim."

We leave our readers to form their own conclusions of the sense with respect to this Editor, in which we quote the line—

" Cedite Romani scriptores, cedite Graii."

Our duties lie with the great body of the trade, and whatever concerns it, and it is a source of no small consolation to us that we are not reduced to the necessity of co-operating with or depending on the Pharmaceutical Society, or any other society, in our efforts or the means to conserve and promote its welfare.

How different would have been the progress and estimation of the Society, if it had pursued the main objects expected by its members, and for which it was established;† if it had devised some proper means of carrying into effect a sound plan of Pharmaceutical education, particularly for those who reside in the country, many of whom cannot afford to come up for instruction to so expensive a place as London, if it had come to some final determination on the great question of medical reform, more particularly with respect to the situation in which the trade is to be placed on the settlement of any legis-

* The name of Liebig, and a score of others, are prefixed to the list of members as a kind of decoy duck; but does this man think to entrap the majority of the trade by a stupid stale device like that to which he has resorted? We can only observe, that we are extremely sorry to see them in no better company, than they are when lent to aid such parties, giving borrowed reputation to the silly and childish productions of ignoramuses.

† To show the great doubt and distrust in which this Society is held, it is only necessary to state that, out of more than sixty chemists and druggists in Birmingham, not more than eight or ten could be induced to become members, although a meeting was convened, and they were screeched at for more than an hour.

lative measure; if it had adopted means to check such practices among its members, as are injurious either to their own body or to the public, or which are disgraceful and calculated to bring down upon them the calumny of their enemies, or to destroy the confidence of the community: if, in fine, it had evinced, both to the medical profession and to the public, that it was acting with enlightened views for the general good of all concerned in its decisions, and not pandering to private interests,—the most contemptible of all perversions of matters of public import. We hope immediately to see the Council wise enough, and, if they are not, the whole body of the Society, to retrace their steps, or it will be numbered with the many good things which, through bad management, have been consigned to destruction, and their projectors doomed to become the subjects of pity to their friends and contempt to their adversaries.

We have felt bound by our engagements to the public, and also in reply to our numerous correspondents, thus fully to state our opinions concerning this Society, knowing that hundreds are dissatisfied; and in the hope that we may, before it is too late, effect the good we and they had expected, but in which both have been so sadly disappointed.

We now come to another part of our duty, and this relates to the supply of matter, during the present year, in all departments of the Journal. We can safely promise our subscribers much new and original matter, including not only the Chemistry of inorganic, but likewise of organic, bodies, as well as in Chemical Manufactures and Pharmacy. We are on all these subjects pursuing an extensive course of experiments, which we have ample evidences will be attended with many important results, which we shall from time to time take the earliest opportunity of laying before the public.

No exertion will be wanting to maintain for The Chemist that character and patronage it has received for two years; nor are we even without considerable hope of increasing its title to reputation, and to the support of all whose pursuits are connected with Chemistry and the Arts. Success is almost always the sure result of strenuous exertion and determined perseverance in the pursuit of any object: ours will be the perfection of the work in every department, feeling, as we do, the great importance of its subjects; nor do we in the least fear that in the sequel our labors for its advancement will form an exception to the Latin proverb, "possunt quia posse videntur."

I. CHEMISTRY.

ON THE EFFLORESCENCES ON THE WALLS OF STABLES.

To the Editors of the Chemist.

Elland, Dec. 8th, 1841.

GENTLEMEN,—

My attention has been frequently drawn to the effects which the urine of horses produces on the walls of stables. It is a question, however, which has neither escaped the observation of Englishmen, nor has it been overlooked by the enlightened and laborious Germans, by whom it has been not unaptly styled "Salpeterfrass."

Notwithstanding the attention which has been devoted to the subject, I do not think it has as yet attached to itself that share of interest which it merits, more especially, as the damage when done admits of little or no remedy, and therefore some protection from the influence of the fluid in question ought to be provided. For a long period I was ignorant of the nature of the action thus exerted, and accordingly my wonder was oftentimes excited by its consequences.

In explanation of the phenomenon, it is almost unnecessary to state that the decomposition of urine commences shortly after its expulsion from the bladder, during which the urea and lactic acid first present, are converted into the lactate and volatile carbonate of ammonia. The former, being of a more fixed character, is carried off along with the water into the dung-reservoir. Some portion of the carbonate of ammonia is likewise conveyed away, and hence the alkaline nature of putrefying urine, which previously had an acid reaction. A very small proportion, however, of the carbonate continues in the urine, its volatility occasioning its diffusion through the surrounding atmosphere, and through the air of the stable. Hence the ammoniacal odor which usually pervades extensive establishments where horses are kept; and hence, likewise, the powerful smell emitted from the walls of these places after having been white-washed with quicklime.

Every one must have noticed the pungent odor then evolved, and the copious flow of tears occasioned by it. Were this the sole inconvenience, it might be readily endured. Such, however, is not the case; for it exerts a destructive action on the plaster of the adjoining walls; and should any one of them form the side of a dwelling-house, the plaster in question is invariably undergoing decomposition and falling off. Again, the latter, in consequence of the deliquescent nature of the nitrate of lime thus formed, is always damp, so that no paper can be preserved in a sound state for more than a month or two.

This inconvenience would seem to be considerably aggravated or not by the kind of materials constituting the wall, *i. e.* whether these are of brick or stone. The first are by far the worst,—a fact which admits of an easy explanation on chemical principles. Thus we are taught, by numerous authorities, that the oxides of iron and alumina are remarkable for their power of producing solid compounds with ammonia. Such are true salts, in which the ammonia operates as a base. According to Chevalier, ammonia exists in all the minerals of which iron is a constituent; and the same chemist has shown that, though hæmatite is devoid of all porosity, it still contains one per cent. of ammonia. Vauquelin discovered the same alkali in all rust of iron; and on similar grounds it is found, that minerals abounding in alumina or oxide of iron, are gifted with the singular property of attracting ammonia from the atmosphere. The odor evolved by aluminous minerals when moistened, may be traced to the exhalation of ammonia; and even gypsum, pipe-clay, &c., in consequence of the alumina present in them, emit an abundance of the volatile alkali when subjected to the action of a small quantity of caustic potassa. The ammonia, in all these instances, is readily liberated by bringing into play superior affinities.

Alumina and oxide of iron having such a powerful attraction for ammonia and its volatile preparation, we can easily account for the same disposition possessed by walls of brick. The last, it will be remembered, are composed chiefly of clay and ferruginous earth. Most kinds of stone, particularly that in request here, must have more or less of the like power, since it is usually of an argillaceous character, and tinged with the oxides of iron. It does not, however, display so great an attraction for ammonia as brick, and ought therefore, perhaps, to be preferred in the construction of stables.

The absorbing qualities of brick are not its only deficiencies. By concentrating the ammonia, if the expression may be allowed, it enables the alkali to effect a more energetic and rapid destruction of the lime contained in the building, in the course of which the former would seem to resolve itself into nitric acid. This change is the more readily accomplished, because of the presence of hydrogen, and because of the affinity displayed by the acid for the cal-

careous compound. The deliquescent nature of the salt when formed is such, that the wall containing it is rarely free from damp, not even in the driest weather, so that it operates injuriously on every thing with which it may come in contact. Paper, white-wash, and most other coverings resorted to, with the idea of concealing the defect, shortly prove their incapacity for doing so, for they often lose their colour and fall to pieces.

Again, the nitrification here alluded to, is much promoted by the presence of ammonia; for the nitrogen of ammonia, it ought to be remembered, shows a greater disposition to unite with oxygen, and thus yield nitric acid, than when existing in any other state. The nitrate of lime no longer fills the place of mortar, but gradually crumbles away; and as often as it may be replaced, so often is its destruction ensured. Independent of this kind of agency, it does not seem unlikely that the carbonate of ammonia, in addition to any decomposition which it may undergo, in becoming nitric acid, may itself exert a somewhat injurious action upon buildings. This would appear to be increased by the readiness with which it is absorbed by the different materials constituting them, and also by that power which it puts forth in changing the mortar into a carbonate, and depriving it of its adhesive property. This quality arises from the ammonia existing in combination with carbonic acid. We know that this gas acts injuriously on lime, because it is chiefly from its presence in the atmosphere, that all exposed plaster in the course of time is destroyed. That it may also prove hurtful as a carbonate, and as the alkali itself, we may infer from the fact of its capacity for being absorbed, as I have before stated; and this supposition will appear the more just, when I remark, that it invariably exists in a free and undecomposed state in plaster and walls of stables, and as such is readily evolved by the aid of several re-agents.

With these observations a question naturally arises as to whether the mischievous tendency thus exercised by the carbonate of ammonia of putrid and putrefying urine, could not be obviated. It may seem a simple subject for a lengthy discussion, but still as it is interesting to know the manner in which "Salpeterfrass" takes place, so is it desirable to be rendered familiar with the method in which, when necessary, this decomposition may be best prevented. I have introduced the subject, not because I can at the same time substitute an unobjectionable remedy, but more with the idea of attracting the attention of your readers to it, and extorting from some one of them a suitable method of guarding against the evil. I shall, however, offer a few suggestions which have occurred to myself, or which have been gathered from the labors of others.

With the intention of preventing the loss of ammonia in stables by the changes just noticed, and thus to enable the owner to consign it to the land, Liebig has proposed to strew the floors of these establishments with powdered gypsum. In this way carbonate of lime and sulphate of ammonia are generated. The latter, being devoid of all volatility, neither impregnates the air, nor does it penetrate the walls. It is evident, however, that this can work only in a partial manner, inasmuch as a considerable portion of the volatile carbonate is absorbed by the mortar and walls prior to coming in contact with the gypsum spread upon the floor. Doubtless, so much of the carbonate as is absorbed will be retained. Chloride of calcium and superphosphate of lime would act in a similar manner. Carbon or charcoal, provided it could be effectually employed, would also exert a powerful and beneficial influence. The peculiar porous texture of this substance enables it, in common with many other bodies, to inhale more or less of several gases, vapors, and liquids. Saussure found that charcoal prepared from box-wood, was capable of absorbing upwards of 90 times its volume of ammoniacal gas, and variable quantities of many other gases. No means, however, of this kind with which we are at present acquainted, would prove sufficient, *i. e.* would protect the lime and walls entirely from the hurtful action of ammonia. The best plan apparently consists in removing the urine and solid excrements as soon as possible after secretion and expulsion; in building stable walls, particularly when in contact with dwellings, of a tolerable thickness, or what would be better, with an empty space between the inner and outer wall, with proper drains for carrying away all damp and wet, and with walls of good stone, so hewn and put together as to require little or no mortar. The wall forming the interior of the stable might be annually whitewashed with powdered gypsum. Such would, probably, satisfy the corroding nature of the carbonate of ammonia for so brief a period; at which season another coat might be put upon it, the previous one being first scraped off, and consigned to the soil as manure. A wash of charcoal for the walls, though giving a sombre appearance, would likewise take up and, perhaps, retain the ammonia; but in all these cases the results would be merely partial, for they would not entirely prevent the latter from coming in contact with the wall, &c. Nevertheless, with the aid of gypsum or charcoal spread upon the floors, and with the plan of having the excrements removed to the dunghill as speedily as pos-

sible, these processes would become tolerable preservatives, and might be easily applied where such assistance was requisite.

The best and most useful scheme, however, would seem to consist in the suitable construction of the building. Add to this, if the costs were no consideration, I imagine that "saltpeterfrass" might be almost perfeetly prevented; and, of course, any method at all likely to bring about such results, is deserving of attention, not only on this account, but because of the saving thus effected in the ammonia. The value of the latter as manure is now well ascertained. In accomplishing this, the addition of one of the acids, as the sulphuric or muriatic, to the receptacle holding the excrements, is worthy of a trial. This would, in all probability, fix the ammonia as fast as it was generated. How far any or all of these plans are applicable to the case under consideration, can be ascertained only by experiment.

To these suggestions another may be added, calculated to exert a preservative action for a considerable time. For example, the inner wall of the stable might be constructed of large bricks, evenly moulded, and having one of their sides, *viz.* that eorresponding to the interior of the stable, covered over with a light-colored glaze, after the manner of earthenware. The sort of glaze spread over tiles, used for lining baths, would answer very well.

In large towns, where room is valuable, and where the stables necessarily stand in connection with dwellings, some provisions of the kind statcd are extremely desirable.

In conclusion, I may just mention the rationale of "Salpeterfrass." The ammonia evolved by the putrid urine resolves itself into nitric acid and water, its hydrogen and nitrogen attracting oxygen from the atmosphere. These two compounds immediately combine, and form the ordinary acid. The presence of water would seem to be absolutely necessary, inasmuch as nitric acid in its pure state cannot exist without it, and hence it happens that nitric acid never results spontaneously from any other compound than ammonia, where hydrogen is present. Thus, purc nitrogen will not combine directly with oxygen; neither will this element, as it exists in cyanogen, undergo the like change, because here the hydrogen is wanting, by the aid of which watcr is supplied to the nitric acid. The nitric acid, once fairly evolved, immediately combines with the lime, and thus gives rise to the formation of a soluble and deliquescent nitrate.

I remain, Gentlemen,
Very respectfully yours,
JOHN S. HILEY, A. B. M. B.

ON THE COMPOSITION AND ON THE PRODUCTS OF THE DISTILLATION OF STEARIC ACID.*

BY J. REDTENBACHER.

SINCE the investigations of M. Chevreul, no new analysis of stearic acid has been published; all the recent investigations concerning the fatty bodies have been based on those first results.

Since the publication of that great work, analytical processes have been greatly improved. M. Redtenbacher has devoted much time to the analytical examination of this acid, and of the products derived from it, employing the newest methods in use.

The first point which it was necessary to determine, was the composition of stearic acid, which, although the best studied, left uncertainties concerning the products of its distillation; and, at all events, ought to form the basis of all investigations.

In order to study the products of this distillation, I used, says he, some stearic acid from the manufactory of M. Merck, of Darmstadt. It was extracted from beef fat: it was perfectly white, crystallized, fusible at 56° C., and free from wax. I crystallized it seven or eight times in alcohol, until the acid, precipitated by evaporation from the alcoholic solution mixed with water, dissolved in potassa, and re-precipitated by hydrochloric acid, fused at 70° C.

I commenced by analysing the product thus procured, in order to ascertain whether it was perfectly identical with that of M. Chevreul. The numbers which I obtained for carbon were constantly below those of M. Chevreul; and those for hydrogen, rather higher.

The formula which I was led to deduce from seven analyses made by means of oxide of copper, and by employing all possible precautions, is the following:—

$$C^{68}\ H^{132}\ O^{5} + 2\ H^{2}\ O.$$

In order to control these results, I undertook the analysis of stearate of silver. This salt was obtained by precipitating an alcoholic solution of stearate of soda by a solution of silver; it was presented under the form of a very bulky, light, white precipitate, floating on water.

The mean deduced from several combustions of this salt gave for 2 grs. 165 of stearate of silver, 0 gr. 619 of metallic silver, that is to say, 28·62 per cent. of silver, or 30·73 per cent. of oxide of silver; the atom of acid weighs, according to that, 65·43.

Moreover, by the combustion with oxide of copper, I obtained numbers which lead to the formula

$$C^{68}\ H^{132}\ O^{5},\ 2\ Ag\ O.$$

The anhydrous acid is then

$$C^{68}\ H^{132}\ O^{4}.$$

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* *Annales de Chimie et de Physique*, Aout, 1841.

The hydrate then contains two atoms of water, which are found replaced in salts by two atoms of metallic oxide. The perfect accordance of the numbers obtained in all my combustions, whether made with oxide of copper alone, or with oxide of copper in a current of oxygen on products of different preparations, and particularly the accurate determination by means of the salt of silver, ought to remove all doubt as to the accuracy of my results, even when I could not succeed in preparing other combinations: moreover, the salt of silver was most capable of furnishing accurate results, on account of the high atomic weight of its base.

I prepared the salt of lead by precipitating, by an alcoholic solution of stearic acid, a solution of a salt of lead, mixed with acetic acid. The calcination of this salt, and its combustion with oxide of copper, gave me numbers which lead to the formula

$$C^{68}\,H^{132}\,O^{5},\ 2\,Pb\,O.$$

STEARIC ETHER.

I prepared this product by passing to saturation a current of hydrochloric acid into a solution of stearic acid in absolute alcohol. I afterwards gently heated the liquid, and agitated it with water until all the hydrochloric acid was removed.

I thus obtained a colorless, transparent, crystalline product, fusing at 30° C., with scarcely any odor, and decomposable by distillation. By boiling this ether with potassa, and decomposing the soap thus formed by hydrochloric acid, there was separated alcohol and stearic acid, which retained its point of fusion. This compound, submitted to analysis, furnished results which, reduced to hundredths, give

Carbon	77·17
Hydrogen	12·84
Oxygen	9·99
	100·00

These proportions correspond with a combination in which one atom of water of the hydrated stearic acid is found replaced by one atom of oxide of ethule, and whose formula is

$$C^{68}\,H^{132}\,O^{5} + C^{4}\,H^{10}\,O + H^{2}\,O.$$

Calculation gives numbers which agree very well with those deduced from experiment. Thus, we have

72 atoms of carbon	5503·3	77·49
144 atoms of hydrogen	898·5	12·65
7 atoms of oxygen	700·0	9·86
	7101·8	100·00

I have in vain endeavoured to prepare the ether

$$St + 2\,Ae\,O$$

indicated by M. Lassaigne: I think that neutral stearic ether does not exist, and that none but the foregoing compound can be obtained.

$$St + Ae\,O + H^{2}\,O,$$

or else the compound

$$\begin{cases} St + Ae\,O + H^{2}\,O. \\ St + 2\,H^{2}\,O, \end{cases}$$

analogous to the combinations of tartaric and phosphoric acids, that is to say, to the combinations of the polybasic acids.

Now these numbers correspond with a curious combination, which would be analogous to stearine, and in which glycerule would be replaced by ethule:—

$$2\,St\,Ae\,O + 3\,H^{2}\,O.$$

This formula would lead to the following numerical results:—

C^{140} . . .	10700·9	77·27
H^{280} . . .	1747·1	12·62
O^{14} . . .	1400·0	10·11
	13848·0	100·00

which are in perfect accordance, as may be seen, with those deduced from analysis.

STEARATE OF SODA.

I prepared stearate of soda by boiling hydrated stearic acid with an excess of carbonate of soda, and by pressing and drying the product. Dissolved in alcohol, filtered, decomposed by water, evaporated, and dried in the sand bath, this salt, submitted to analysis, led me to results which agree with the formula

$$C^{68}\,H^{132}\,O^{5},\ 2\,S\,O.$$

DRY DISTILLATION OF STEARIC ACID.

M. Chevreul has admitted, in his investigations concerning the fatty bodies, that stearic acid distilled without alteration, because the distilled product presented the same point of fusion as the acid employed. M. Chevreul described this distillation in the most complete manner: nothing escaped him; not even the very trifling disengagement of carbonic acid and water. I observed exactly similar phenomena. The distilled product, boiled with water, communicated a slightly acid reaction, without depositing sebacic acid on cooling.

A combustion of this distilled stearic acid gave the same results as the undistilled acid: the hydrogen differed a half per cent. without any loss.

The salt of silver of this acid, prepared in the same manner as that of pure stearic acid, furnished entirely different numbers: the hydrate of this acid was found to have the following composition:—

$$C^{30}\,H^{60}\,O^{4}.$$

These results were not susceptible of any interpretation: there was no doubt but the stearic acid had been modified by distillation. I have detailed these last analyses, in order

to show how I had been led to find the true products of the distillation of this acid.

If the salt of soda, prepared with the distilled and well dried acid, be treated by ether, in which it is almost insoluble, the solution deposits, by cooling, a white, crystalline substance, resembling paraffine, as well as an oil, with an empyreumatic odor. The salt of soda, thus purified, being decomposed by hydrochloric acid, furnishes an acid, whose point of fusion is 60° or 61° C. After several crystallizations in alcohol, its point of fusion had not changed: all its properties agreed with those of margaric acid.

The combustion of this acid, executed by means of oxide of copper, gave results which, reduced to hundredths, are—

	Analysis.	Atoms.		Calculation.
Carbon .	76·00	34	2598·8	75·92
Hydrogen	12·57	68	424·8	12·39
Oxygen .	11·42	4	400·0	11·69
	99·99		3423·1	100·00

Now this is exactly the formula which represents the composition of margaric acid.

The combustion of the salt of silver, by means of oxide of copper, and the calcination of the same salt, led me to the formula

$$C^{34}\ H^{66}\ O^{3}\ AgO,$$

which is the composition of margarate of silver.

Thus, it is evident that stearic acid is converted, by dry distillation, into margaric acid.

In order to make myself acquainted with the products which are simultaneously formed during this reaction, I distilled a new quantity of this acid, and, after having formed a salt of soda, I decomposed it by chloride of calcium, the salt of soda swelling up too much when treated with ether. This vehicle, boiling, withdrew from the salt of lime the two substances which I mentioned above: on the cooling of the solution, the solid substance crystallized. By means of several crystallizations in ether, this substance was presented under the form of white, pearly, diaphanous bracteæ, melting at 77° C.

Distilled with lime, stearic acid gave a product identical with that which I have just described, in both properties and composition.

If this product be regarded as a peculiar body, and if its formula be calculated by admitting it to contain one atom of oxygen, we have

$$C^{45}\ H^{90}\ O.$$

This body would therefore be equal to

1 atom of margarone $\left\{\right.$ $C^{33}\ H^{66}\ O.$
+ polymeric hydrocarbon of olefiant gas . . $C^{12}\ H^{24}.$

The results thus obtained render the existence of stearone very doubtful; for this body should be formed by means of stearic acid, by simple abstraction of carbonic acid. Now stearic acid, in being converted into margaric acid by dry distillation, is divided into two new combinations; and it is not very probable that this decomposition and the formation of stearone can simultaneously take place.

In submitting to evaporation the ether from which the margarone is separated, an empyreumatic oil, impregnated with a great quantity of that matter, is obtained. By a properly conducted evaporation, the first products may be obtained very nearly free from margarone. Submitted to analysis, this product gave me a composition in hundredths identical with that of olefiant gas.

Such are, in conclusion, the products which are formed by the distillation of stearic acid. Thus, the latter is divided by the action of heat into hydrated margaric acid, margarone, water, carbonic acid, and carburetted hydrogen.

These results explain why the product of the distillation of stearic acid retains the point of fusion of the latter, although margaric acid melts at 60° C., for this product is mixed with margarone, which melts at 77° C.

The action which is operated in the dry distillation of stearic acid is conformable to the ordinary decompositions by fire. The bibasic acid is divided; there results from it water, and a monobasic acid, which, in its turn decomposed, gives carbonic acid and margarone: finally, a hydrocarbon is formed, —the last product which is presented at the end of the distillations of organic matters. The reaction may be expressed as follows:—

6 atoms of hydrated margaric acid	$C^{204}\ H^{408}\ O^{24}$
1 atom of water . . .	$H^{2}\ O$
Margarone	$C^{33}\ H^{66}\ O$
1 atom of carbonic acid .	$C\ O^{3}$
Polymeric hydro-carbon	$C^{34}\ H^{68}$
4 atoms of hydrated stearic acid	$C^{272}\ H^{544}\ O^{28}$

The analysis of margarone demonstrates, in a positive manner, that it contains less oxygen than the acid whence it is derived. However, if we consider it only in this body, the proportion of the carbon to the hydrogen, it is remarked that it is the same as in the acid.

By analysis we can ascertain, numerically only, the relative quantities of the principles of a combination; the results obtained ac-

quire value only inasmuch as they express a theoretical idea.

Stearic acid contains the same number of atoms as hyposulphuric acid. Now, it is known that the latter is decomposed under the influence of heat, so that its oxygen is divided, and gives rise to two products, each containing one part of radical; there is then produced a higher of oxydation.

By the action of heat, stearic acid thus gives rise to margaric acid, which, for the same atomic weight, contains more oxygen than it. The formation of an inferior oxide of the same radical, may also be the consequence of this division of the oxygen.

If the analysis, contained in this memoir, be examined, it will be seen that stearic and margaric acids contain the same proportion of carbon and hydrogen, so that the latter acid being represented by $C^{34} H^{66} + 3 O$, the former may be expressed by $2 (C^{34} H^{66}) + 5 O$.

By expressing this radical, which may be called margarule, by R, we should have—

Margaric acid $= R + 3 O$ margarulic acid.
Stearic acid $= 2R + 5 O$, hypomargarulic acid.

The existence of this radical acquires still more probability, if it be considered that one of these acids may be converted into the other by the action of heat. By considering margarone in this point of view, it may be represented as the first oxide of the same radical $C^{34} H^{66} O = R + O$. This formula applies equally well to the analytical results found by M. Bussy.

The dry distillation of stearic is thus explained in no less simple a manner, for stearic acid contains the elements of margaric acid and of this margarone :—

3 atoms of hydrated margaric acid	$C^{102} H^{204} O^{12}$
Margarone	$C^{34} H^{66} O$
Water	$H^2 O$
	$C^{136} H^{272} O^{14}$

The decomposition of margaric acid, with or without admixture with lime, may likewise be explained by

Margarone	$C^{34} H^{66} O$
2 atoms of carbonic acid	$C^2 O^4$
1 atom of water . . .	$H^2 O$
Polymeric hydrocarbon .	$C^{32} H^{64}$
	$C^{68} H^{132} O^6$

The existence of one common radical in the most common acids, throws new light on the theory of organic compounds.

Stearic acid may also be converted into margaric acid, under the influence of nitric acid, or of a mixture of sulphuric and chromic acids. (See an important paper, by M. C. Bromeis, in *The Chemist*, No. XXIV. page 353.)

ON THE NECESSITY OF ESTABLISHING AN UNIVERSAL SYSTEM OF CHEMICAL SYMBOLS, EQUIVALENTS, AND THERMOMETERS.

BY CHARLES WATT, JUN.

At a time like the present, when chemistry is making such rapid progress, and when the use of symbols has become so general, as being the best means of teaching the ultimate constituents of bodies, and of assisting the memory in retaining that knowledge when it has been acquired, I conceive it to be of the utmost importance to the further advancement of the science, and that the advantages to be derived from the use of symbols (which are very numerous) may have their full scope, that one universal system should be agreed on among chemists, to which, hereafter, in their works and lectures, all should adhere. This is of much greater importance than, at first sight, may appear evident; for it is clear, that by multiplying the systems, they are adding to the difficulties, already sufficiently numerous, which beset the path of the student.

That an universal system of symbols is a great desideratum, all are perfectly agreed, and any endeavours used to attain the object would, I think, meet with the success which attended the labors of the illustrious Lavoisier in the memorable reformation of chemical nomenclature : a little time and labour certainly were necessary, ere the object was accomplished; but now that it is attained, the result fully compensates for the trouble; and while it has immortalized him who commenced it, it has done as much, if not more, than any thing else towards placing chemistry on its present basis. It is said, that if one system were resolved upon in Great Britain, we should be unable to persuade our continental brethren to come into our terms; but I think, nay, I am confident, that if we succeeded in effecting our objects at home, they would see its advantage, and ultimately follow the same plan. Besides, the same arguments were used at the time of the reformation of chemical nomenclature before alluded to; the futility of which, it is needless to say, has been fully proved.

To show how injurious the multiplicity of writing symbols of systems at present in use must be, we will suppose a person attending lectures at any institution, where a certain system is taught; when he goes home, after having attended the lecture, he takes up a book written by a man who teaches another system. I ask, will this not confuse him? will it not impede his progress? If he has a tutor to go to (and how few have), it is explained to him; otherwise he is left in uncertainty as to which is right and which

wrong, or, at least, he is confused and puzzled, and unable to make out the cause of the difference. At the present time, almost every author uses a system of his own; and although a person acquainted with one is able to make out the rest, yet, if only one system were in use, it would save the student much embarrassment, and even the proficient would be spared the trouble sometimes necessary to make out the substance expressed by the symbol.

To place this subject in its proper light, I may give a few examples of the various methods used by various authors in writing the symbols of some of the most simple bodies —those with which we are most acquainted, —and, I may remark at the same time, that although they are sufficiently perplexing in consequence of their number, yet those which are used for denoting the more complex bodies are much more so. It is to be recollected, that symbols have but just come into use; and having increased to so great a number in a short space of time, I may ask, what shall we not have some few years hence, if the system is not altered? The following are some of the symbols used by authors for sulphuric and molybdic acids. Inorganic acids :

$$\dddot{S}.\ (S + {}_3O).\ S'.\ S\ O_3\ \ddot{\dddot{S}}$$

$$\dddot{M}.\ (Mol + {}_3O).\ Mol'.\ Mo\ O_3\ \ddot{\dddot{M}}.$$

The following are some of the symbols in use for acetic and oxalic acids. Organic acids :

$$C_4\ H_3\ O_3.\ \bar{A}.\ Ac'.\ A$$
$$C_2\ O_3\ 2\ (C\ O) + O.\ Ox'.\ \dddot{C}^2 *$$

I think that sufficient proof has been given of what has been said; and it is clear, that an immediate alteration cannot be too strongly urged, especially at this time, when discoveries are daily occurring. The longer the task is deferred the more difficult of accomplishment will it be, and the parties who will contend for their own systems will have greatly increased in number.

It is not my intention here to attempt to decide as to which system is least liable to objection, for all are in some measure defective; but I may say, that I think the Berzelian system is as little objectionable as any other, if not preferable. Simplicity should be the main thing attended to in the choice, and will be found the greatest recommendation which a system can possess. That the use of so many different symbols for one article is calculated to cause numerous errors, I know, from having had opportunities of witnessing them; in fact, it will be evident to every reflecting mind—indeed, to every person who has any knowledge of the present state of chemical science.

But it is not only an universal system of symbols that I would advocate; every thing that has been said respecting them, is equally applicable to the difference existing at the present time among authors, as to the standard from which the equivalent numbers of the various bodies shall be taken: the points from which they all start are arbitrary; therefore, it little matters which is selected, so long as it is universal. That the diversity which exists is calculated in an eminent degree to perplex students, is evident, *primâ facie;* for one author tells him, that the atomic weight or equivalent of a body is one number; and a second, that it is another. It appears to me, that that system which takes hydrogen as unity, being the lightest substance, however, is preferable.

I would urge, before concluding this part of my subject, that the same plan should be pursued respecting the scale for thermometers, and as to the point from which they should start. As all these, like the equivalents, are arbitrary, it will not be of great importance which is adopted, provided that every author, in writing, uses the same one.

The initial of the name of the scale, as F. for Fahrenheit, R. Reaumur, and C. Centigrade, is put after the degree indicated, as they differ widely from each other; and it not unfrequently happens, that from mistake it is omitted, which makes a vast difference when the operation to be performed requires accuracy of temperature, and often is the cause of failure.

The Centigrade and Reaumur scales have not, in my opinion, sufficient divisions for a thermometer for an experimentalist of the present day, and Fahrenheit's scale has its defects.† I think, therefore, that a new scale should be decided upon, the zero of which should be the greatest degree of cold ever produced. These desiderata might be accomplished by the Chemical Society; or a meeting or meetings might be called, and committees formed, to carry these objects into execution; and that, if accomplished, they will do much to advance science, and remove many difficulties in the way of students, no one will attempt to deny.

I cannot conclude without once more urging the attention of chemists to what has been previously said. I think, generally speaking, we shall find that such things as

* From the above it is manifest, that authors are determined to differ from each other, in however slight a degree.

† Fahrenheit's zero was the zero of his day (if I may so speak), but not of ours.

these are of much more importance to the advancement of science than many imagine. If the various authors of the present day were aware of the evil, I think they would be satisfied to copy those symbols previously used, although possessing defects, rather than multiply what are far too numerous already.

ON THE NECESSITY OF AN UNIVERSAL SYSTEM OF CHEMICAL EQUIVALENTS.

[Since the above was in type, we have received the following, which we insert, although received after the 15th of the month.]

To the Editors of The Chemist.

Sudbury, Dec. 16th, 1841.

GENTLEMEN,—

I beg leave to call the attention of the chemical world to a great desideratum in chemical science; that is, the co-operation of all chemists for producing uniform numbers for expressing the atomic weight of bodies.

The philosopher to whom, I believe, must be awarded the merit of having first established the fact by experiment, that bodies combine in definite proportions, is Dr. Dalton; it is true, that with Dr. Higgins the fact first originated; but there it remained a mere hypothesis, which might or might not be true; and the value which is attached to it at the present day, and that it is sanctioned by Davy, Wollaston, Brande, Thomson, and others, too numerous to mention, is a sufficient indication of its merit. It is the theoretical part to which I would call attention.

There exists a great diversity of opinion as to whether oxygen or hydrogen is best to be taken as a standard for expressing the relative value of other bodies. The assuming of hydrogen as a standard is sanctioned by Sir H. Davy, Professor Brande, Dr. Turner, and others; and, being the lightest body at present discovered, there appears to be sufficient and powerful reasons for its selection; its chief inconvenience is the great advance of numerical value requisite for heavier bodies, but this applies with much greater force to the high numbers, sanctioned by Professor Berzelius, and used in Mr. Noad's Chemistry, now publishing.

Dr. Wollaston, together with Thomson, Berzelius, &c. assume oxygen as a standard. Dr. Thomson regards its numerical value as 1; Dr. Wollaston as 10; and Berzelius as 100.

Oxygen can be obtained pure by the process recommended by Mr. Donovan; it is doubtful whether hydrogen has ever been obtained pure; so that, on this point, oxygen may be deemed the safest to reckon from. I think it cannot be recommended upon any other ground; but I leave others to judge and form their opinion, and, if possible, obtain one uniform scale of equivalent numbers throughout the world.

Now for a word or two on symbols. The symbols which are generally used, are those of Professor Brande and Professor Berzelius. The objections to be urged against those of Berzelius, are, that by expressing the atoms of oxygen by dots, they are very liable to become perplexed by imperfect printing, or errors of the press; in expressing a compound he often omits the sign +, and writes the combined elements side by side. Those of Professor Brande, which I generally use, express the composition of a body with much greater clearness, are easier to be remembered, and cannot but be understood. It may, perhaps, be objected to his, that, being more abbreviated, they do not convey sufficient indications of atomic weight; that the composition of sulphuric acid is more clearly expressed by $\dot{S}$ than by Brande's S′; but this cannot affect the general reader, who, I think, will prefer the latter method.

In writing this, I wish only to stimulate others that have more power to try to get this important object brought about upon a sure and sound plan, not liable to be upset by succeeding discoveries.

I am, Gentlemen,
Respectfully yours,
W. O.

ON THE GASES DISENGAGED BY MARINE PLANTS.*

BY M. AIMÉ,

PROFESSOR OF PHYSICS IN THE COLLEGE OF ALGIERS.

HAVING had frequent opportunities of observing the plants which grow in the sea, I have remarked that they are ordinarily covered with a great number of bubbles, and that this phenomenon is most visible when the water is calm. In certain localities, the disengagement of the bubbles is so abundant, that a kind of scum, similar to that which is met with in freshwater marshes, is formed. I, at first, thought that the gas produced was analogous to marsh gas; but analysis having indicated a very different composition

* *Annales de Chimie et de Physique*, Août, 1841.

to what I expected, I thought it was an interesting fact, and I here present its examination.

The disengagement of bubbles seems to take place at all seasons of the year; for I have observed it in winter and summer; it depends solely on light. At the rising of the sun, it is difficult to collect sufficient gas to make an analysis, while, towards evening, if the sky has been cloudless during the day, the number of bubbles is considerable. In order to ascertain that light alone contributed to their formation, I placed in a large vessel filled with sea water, which I frequently renewed, several plants with their roots, and I found that an exposure of a few minutes to a vivid light, was sufficient to immediately produce bubbles on the surface of its leaves, and that being kept in darkness for a certain time caused them to disappear. These plants have been kept alive for two months, and constantly presented the same results. In order to render the observation more conclusive, I examined, several days in succession, some tufts of plants on the beach. After sunset, I removed all the adhering bubbles by agitating the leaves, and the next morning I again went to observe them. I thus saw that there was no appreciable quantity of gas produced during the night, or that that which was formed was immediately dissolved in water, while, as soon as the rays of light came in contact with the plant, the disengagement of bubbles took place.

All the varieties of plants which I have examined were covered with bubbles at the surface; but several of them have also the property of forming them in their interior; these are, generally speaking, those with soft leaves, as the ulvæ, confervæ, &c.

I have also observed these internal bubbles, and I ascertained that light increased their volume, frequently even in a sufficient degree to produce a rent in the leaf which contains them, and that darkness greatly diminishes, without, however, completely annihilating them.

By analysing the gas formed, I found a difference between the results of the external and internal bubbles, and between the gas collected in the morning, and that collected in the evening.

	Ox.	Nit.
Gas of the internal bubbles, collected before sunrise . .	17	83
Gas of the internal bubbles, collected after sunset . .	36	64
Gas of the external bubbles, collected before sunrise . .	21	79
Gas of the external bubbles, collected in the sunshine at 10 a.m.	55	45

The last gas is that which is disengaged and forms scum at the surface of the water when it is not agitated; it is produced in such quantity, that I have often been able to collect a litre of it, by shaking the leaves of marine plants distributed over five or six square feet of horizontal surface.

From an examination of the numbers obtained, it is perceptible, that the results of analysis arrived at depend on the time at which the gas is collected, on the state of the sky, on the season, and probably also on the latitude of the place. My analyses were made in Africa, in Algiers, during the months of July and August, which, in that locality, are the hottest of the year.

Sea water having, like fresh water, the property of dissolving carbonic acid, I thought that this gas must act its part in the respiration and expiration of plants, but that it escaped analysis on account of the solvent property of the water.

I took some fresh plants, with their roots, and placed them in sea water, contained in a well-closed flask. After an exposure of twelve hours in the shade, I found that the air in the flask contained a considerable quantity of carbonic acid. I then reversed the experiment; I placed in the sun the apparatus containing carbonic acid: the disengagement of bubbles greatly increased, and a portion of the carbonic acid was converted into oxygen.

In order to ascertain whether the internal and external bubbles arise from the gases contained in the water, or from those of the plant, I put a leaf of ulva, containing an internal bubble, into a flask of boiled sea water, after having washed the leaf in some of the same water. The flask, having been carefully closed, was exposed to the light for several hours: no bubbles were formed on the exterior of the leaves, but the internal bubble greatly increased, and became more than twice as large as before the experiment. The flask was kept in the dark for twelve hours, and the bubble was reduced to half of its original size.

The same experiment was repeated with other leaves, and the external bubbles might be gathered; but, to succeed, it was necessary to act with the direct rays of the sun; for if the disengagement is not rapid, the water, which is deprived of air, absorbs the gas as fast as it is formed, and the bubbles are invisible.

It must be remarked that, in the preceding experiments, account was taken of the temperatures, and they were managed so as to have the same degree of heat in the shade and in the sun.

TESTS FOR PHOSPHOROUS ACID.*

BY M. WOHLER.

Some phosphoric acid, when examined to ascertain whether it contained arsenic, gave by heat, with sulphurous acid, a yellow precipitate of sulphuret of arsenic. This phosphoric acid contained, as was shown on a more attentive examination, arsenious and phosphorous acids.

It was seen, indeed, that phosphorous acid possesses the property of being converted into phosphoric acid by the action of sulphurous acid, especially when aided by heat. This transformation is caused by the decomposition of the water; and there is a formation of sulphuretted hydrogen, and a reciprocal decomposition of this sulphuretted hydrogen and the sulphurous acid into water and sulphur.

If phosphorous acid be mixed with a solution of sulphurous acid in water and heated, an abundant precipitate of sulphur is immediately formed. It is a very good means of discovering whether phosphoric acid contains phosphorous acid, as is often the case: if it contains arsenious acid at the same time, it turns yellow on the precipitation of sulphuret of arsenic.

The presence of phosphorous acid is very easily detected by means of a small gas apparatus, similar to Marsh's. Indeed, if phosphoric acid containing phosphorous acid be mixed with a liquid disengaging hydrogen gas, phosphoretted hydrogen is formed, which may be recognized by its smell alone; and if this gas be inflamed, it burns with a whitish, brilliant flame, very different from that of pure hydrogen gas. If this flame be directed against a porcelain plate, there is always remarked, and in a very evident manner, especially if the place be not too light, a circle of green light in the flame thus spread out, as when phosphorous burns in confined air or in chlorine.

* *Journal de Pharmacie*, Nov. 1841.

NEW AND VERY POWERFUL GALVANIC BATTERY.

To the Editors of The Chemist.

Nov. 24th, 1841.

Gentlemen,—

I beg to inform you that I have constructed a galvanic battery, of very great power, and occupying an extremely small space. Its construction is very easy, consisting simply of a porous diaphragm of the usual form, is wrapped round with thin copper wire, the end of which terminates in a mercury-cup, or binding-screw connection; the interior of the porous vessel is fitted with an iron rod, sufficiently wound round with common hat-wire to pass easily therein; and it is also furnished with a connecting screw, &c., as in the copper side of the battery.

The great power of this battery is occasioned by the great extent of metallic surface exposed to the action of the acid; and by employing zinc instead of iron for the side, its power would be still further increased. I could not obtain any zinc wire at the time, or I should probably have used it for that purpose. When united with the coil machine its effects are extremely energetic, as might reasonably be expected. With earnest wishes for the success of your interesting periodical, I beg to subscribe myself,

Gentlemen,
Respectfully yours,
P. Barnett.

EXPERIMENTS ON THE COMBINATION OF CHLORINE WITH SULPHUR.

To the Editors of The Chemist.

Dulwich, Dec. 9th, 1841.

Gentlemen,—

Having recently been engaged in experiments on the combination of chlorine with sulphur, and the result, in my opinion, being very dubious, I take the liberty of submitting the subject to the notice of your readers.

I first generated chlorine of gas, by the action of hydrochloric acid,* on peroxide of manganese, and, with the aid of a very gentle heat, I obtained a brisk evolution of pure gas; which I tested by its rendering vegetable coloring matter totally colorless. Having thus far succeeded, I next constructed a pneumatic trough, filled with water at a temperature of not less than 90° F. to prevent any absorption. I again tested it, and found that it still retained the properties of pure chlorine. Having thus appeased my fears as to its impurity, I then supported an appropriate recipient, containing flores sulphuris, (previously washed and dried, to remove sulphurous acid,) excluded entirely from the atmosphere. I next brought the chlorine in immediate contact with the sulphur; and after its transmission, to my great astonishment, I found it no longer in an uncombined state, but converted into a compound gas which powerfully reddened

* The muriatic acid was diluted with a small quantity of water to prevent it from fuming.

litmus test, and on bringing in contact ammoniacal gas, the dense fumes of hydrochlorate of ammonia were instantaneously formed, satisfactorily indicating the presence of hydrochloric acid.

The only manner in which I can account for its formation, is on the supposition that sulphur. flores contain a great quantity of sulphuretted hydrogen, which becomes decomposed by the stronger affinity of chlorine for hydrogen, and hydrochloric acid gas thus generated. But it is scarcely credible that sulphur is contaminated with sufficient hydrogen to cause so great an evolution of the acid.

I am particularly desirous of obtaining further intelligence as to the cause of such a phenomenon, and if you support my supposition, that sulphur. flores are contaminated wiith sufficient hydrogen, in conjunction with the aqueous vapour in the atmosphere, to generate the great quantity of hydrochloric acid gas which is given off.

I am, Gentlemen,
Yours respectfully,
W. Bush.

REVIEWS :—

ELEMENTS OF CHEMISTRY; *including the most recent Discoveries, and Applications of the Science to Medicine and Pharmacy, and to the Arts.* Illustrated by 236 wood-cuts. By Robert Kane, M. D., M. R. I. A., &c. &c. &c. Dublin: Hodges and Smith; Longman and Co., and Simpkin and Co., London; Maclachlan and Co., Edinburgh. 1841.

It is with the greatest pleasure that we have lately observed the press of England teem, as it has done, with valuable works on Chemistry; we think it may fairly be considered as conclusive evidence that the science is in a flourishing state, and that it is receiving that attention from the public, of which a science that explains all the phenomena of nature and art is so eminently deserving;—a science on which the manufactures of our country are so dependent, and to which we are indebted for so many of our most valuable medicines. It is to Chemistry that the manufacturer looks for aid, when beset with difficulties, and to it he rarely looks in vain; it is to Chemistry that medicine owes its present state of comparative perfection; and hence we are of opinion that every youth should study the science; and that it must, ere long, become a necessary part of the education of a gentleman, we feel assured.

The work before us is an excellent illustration of the present state of chemical science and from the high reputation of the author, will, we hope, meet with the reward due to the production of every man who devotes his time to the advancement of science, more especially when endowed with the talent which the author of the work before us has exhibited on this and on former occasions.

This book will be very serviceable to the student, to whom we most earnestly recommend it, and we likewise feel assured that the proficient in chemistry will do well to peruse it.

Every man who works in the laboratory, has something new to offer—observes something which others have not; and as it is well known that Dr. Kane has done much in chemical science, and much to bring organic chemistry to its present state, his work must be interesting to every lover of chemical science. The style in which it is written is so admirably simple, as to render even the more complicated parts of the science intelligible to the most youthful mind; a desideratum of which, although so necessary, most authors have lost sight, albeit they term their works "Elements," and intend them for beginners.

Dr. Kane has adopted the plan now most generally pursued as regards arrangement; *viz.*, that of beginning by first describing the general principles of the science, which, although it has some objections, as, to use the author's words in his preface, "it is impossible to avoid the difficulty of introducing the names of many substances, with whose history the reader cannot be supposed to be conversant," is, however, less liable to objection than the plan pursued by some authors: for, "by entering in the commencement on the description of individual substances, reference to the principles of affinity and the laws of constitution is continually necessary, in order that the reactions of these bodies may be understood." Each of these plans has its defects; but we agree with Dr. Kane, when he says, that he believes the latter to be the most embarrassing.

There is one feature in this work, of which we very much approve, *i. e.*, in the explanation of the general principles of the science, whenever it is necessary to speak of individual substances, instead of using the chemical names of those substances, the commercial names are used; and the reason we approve of it is, because the parties for whom this work is intended, are generally, at the commencement, unacquainted with the meaning of the chemical names, and are acquainted with only the commercial

ones. We have often had occasion to witness the difficulty caused the student by what we have mentioned, which may appear to the proficient a trifle; although, in reality, a serious evil, inasmuch as it adds to what is already surrounded with difficulties. In proof of what we have advanced in favor of this work, we may quote the following :—

"When the cohesion between the particles of a solid and those of a fluid is more powerful than between the particles of the solid itself, the latter is not merely moistened by the fluid, but it abandons altogether the solid form, and becoming liquid, mixes uniformly with the fluid, and is said to have been dissolved by it; by this peculiarity of cohesion, bodies are divided into the soluble and insoluble; thus *common salt* and *Glauber salt* are soluble, whilst *chalk* and *white lead* are insoluble in water. These classes are, however, connected by a series of intermediate degrees of sparingly soluble bodies, such as *cream of tartar and plaster of Paris*. Bodies which are insoluble in water, may be yet easily dissolved in other fluids; thus resinous bodies, which do not dissolve in water, dissolve in alcohol. A great deal of the success of vegetable proximate analysis, depends on the skill with which the solvent powers of various fluids may be successively applied."

After giving a detailed account of the electro-chemical theories of Ampère, Berzelius, Davy, Faraday, Graham, and Becquerel, the author gives his own, which we extract in full, for the benefit of our readers.

DR. KANE'S ELECTRO-CHEMICAL THEORY.

"The influence which electricity thus exercises upon affinity, and the modifications in its results, or producible by its means, although proving a most intimate connexion, do not go, as I believe, so far as to demonstrate a complete identity of cause.

"It is possible that, hereafter, some sublime generalization may embrace the phenomena of heat, of light, and electricity, of cohesion, and gravity, as well as of chemical affinity, within one law, and indicate how, by varied manifestations of a single agent, their separate peculiarities may arise; but though we may look forward to such a state of science, we dare not rashly seek to anticipate its approach; and I look upon electricity as producing, and being produced by, chemical phenomena, precisely as we find heat to influence as well as to be evolved by chemical combination.

"Where electricity is brought into play so powerfully by the action of a simple body upon a compound fluid, it is, I consider, unreasonable to imagine that in the combination of simple bodies, or of compound bodies, with each other, no electricity should be set free; particularly when it is proved, that in such cases some electricity does appear, although in quantity, bearing no proportion to the feeblest galvanic battery. If I were to suggest an electro-chemical theory, such as might agree with the facts that have hitherto been discovered, I would consider that bodies in their free state are perfectly destitute of excitation; but that chemical union, or the degree of intimate approximation which precedes union, may be a source of electrical disturbance, and is that which, in all ordinary cases, gives origin to the electricity employed in our experiments. When united bodies are likewise destitute of electrical properties; in iodide of potassium, the bond is the affinity of iodine and potassium for each other, and not that the iodine is in a permanent state of negative excitation, whilst the potassium remains positive.

"If hydrogen gas be burned in oxygen, there is evolution of electricity, of which only a trace escapes immediate re-combination, and the form of light and heat, but the existence of which, in a highly intense form, has been demonstrated by Pouillet. The oxygen and the vapour of water produced, assume the positive condition; the residual hydrogen becomes negative. If carbon be burned in oxygen, there is likewise combustion and evolution of electricity, of which the positive passes off with the carbonic acid, and the negative rests upon the carbon. The evolution of heat may be in these cases an independent effect of combination, but I would look upon it as being more probably the result of the electricities evolved. If the bodies which act upon each other are dissolved in water, there is no combustion, but heat is still evolved, and the electricities unite with one another without the necessity for any intermediate circuit. It is only where the replacement of one body by another occurs, that the establishment of the chain of inductively polarized molecules becomes necessary; for the particles of zinc and hydrogen, which become oppositely united, have no power to combine, and hence cannot be restored to neutrality, unless by the medium of a third body, to which both may impart their excitations. That the zinc and hydrogen upon the one hand, and the copper and hydrogen upon the other, do not unite, is, I conceive, fatal to those views which assume the identity of chemical and electrical, or, as they call it, current of inductive affi-

nity; for if a molecule of copper in the acid stood in the place of, and acted as, a molecule of chlorine, it should unite with the hydrogen, in the place of allowing it to pass off free.

"The art of chemical union being such as to produce electrical excitation and discharge before it is completed, and the permanent combination of its elements being the result of the return to the neutral state, it is easy to understand that, when these conditions are reversed, the chemical affinity should be superseded, and the bodies brought into the state in which they had been at the moment of excitation, and whilst their elements, oppositely excited, were yet separate from each other. A compound body is, therefore, as I apprehend, decomposed by the battery, from having this electrical state given to it by the current, and the transfer of its elements across the liquid, is accomplished by a series of neutralizations and excitations, accompanying the unions and decompositions by which they pass to the electrodes, on which they yield up their ultimate excitation, and appear isolated and completely neutral. The quantity of electricity necessary to decompose a body is, therefore, the same as it evolved when its elements entered into union; and it should hence follow, that the current of electricity evolved in the union of chemical equivalents of simple bodies with each other, should be the same. That this actually occurs, appears probable from the analogy of heat: an equivalent of oxygen, in combining with various metals, evolves the same quantity of heat; and if the heat be a consequence of the neutralization of electricity, the quantity of this evolved should be the same also. It appears likewise that an equivalent of sulphuric acid, in combining with different bases, evolves the same quantity of heat; and to decompose the various salts thus formed, the same quantity of electricity should be required; and hence, that the two actions are equivalent to each other, may satisfactorily be referred to the same source.

"Such is the interpretation I put upon the phenomena of electro-chemical decomposition, and the relations of electrical forces to affinity. In the molecular condition of polar excitation, which accompanies the passage of a current, I adopt fully the explicit mode of representing the actions of bodies upon each other, proposed by Graham, but I consider it too hypothetical to assume that such molecular state naturally exists in bodies; it may or it may not; but in the absence of evidence that it does, I am not inclined to pre-suppose it unnecessarily. I look upon the current as being produced by the union of opposite polarity, which are themselves not the cause, but the consequence, of the chemical affinities of bodies. Graham is not disposed to admit that the union of simple bodies may be accompanied by electrical phenomena; and to exclude also from the application of an electro-chemical theory the combination of acids and bases with each other, as not capable of generating currents; but the reason of this is, as I imagine, that the currents so generated, are necessarily closed." —Page 323.

The following are the author's very excellent observations:—

ON THE CLASSIFICATION OF THE ELEMENTARY BODIES.

"The principal classifications of the simple bodies that have been proposed, are those of Berzelius, founded on their electro-chemical relations, and of Thomson, who divides them into supporters and non-supporters of combustion. It has, however, been fully shown that, in combustion, each body is mutually a supporter and a combustible: oxygen burns in hydrogen or in the vapor of sulphur, just as much as hydrogen or sulphur burn in oxygen; Thomson's principle is, therefore, radically defective; and the electro-chemical theory, although far superior as a principle, is liable to weighty objections of a somewhat similar kind. These have been already, however, so far noticed, and the arrangement of the simple bodies in that series so fully given, p. 303, that it is unnecessary to recur further to the subject.

"The kind of classification that is suited to the present wants of chemistry, must be founded upon the general analogy of properties between substances belonging to the same class, and on their isomorphous replacement of one another. This last character is not absolute; for, from the dimorphism of many of the simple bodies, it is often difficult to assign their true crystalline relations to each other, and in many cases we do not possess any positive information of their forms.

"Graham has recently proposed a classification which expresses, more completely than any other, the natural relations of the simple bodies. The first class consists of oxygen, sulphur, silinum, and tellurium. The parallelism in properties of the three last is complete, and their compounds are strictly isomorphous; their similarity to oxygen is not so perfect, but they resemble it in their method of combination, and in the characters of the substances which they

form in uniting with hydrogen and the metals.

"The second class comprises magnesium, calcium, manganese, iron, cobalt, nickel, zinc, cadmium, copper, hydrogen, bismuth, chromium, aluminum, glucinum, vanadium, zerconium, yttrium, and thorium. The similar salts of the protoxides of this class are isomorphous; and as has been already shown under the head of isomorphism, the equivalents of a protoxide of this class replace one equivalent of an alkali. Chromium, aluminum, glucinum, vanadium, and zerconium, do not form protoxides, but sesquioxides, the salts of which are isomorphous with those of the sesquioxides of iron and manganese. A remarkable connexion is established between this class and the preceding by the isomorphism of the manganic acid (Mn. O_3.) and chromic acid (Cr. O_3.), with sulphuric acid (S. O_3.) indicating that under certain circumstances these metals may change from one natural family to another.

"The third class contains barium, strontium, and lead. Their salts are strictly isomorphous, and they are connected together by great similarity of chemical properties. Thus the sulphates of the metals of the second class are all easily soluble in water, whilst the sulphates of this class are almost insoluble. Calcium approximates to this condition by the sparing solubility of the sulphate of lime; and the connexion between the two families is still more fully shown by the dimorphism of the carbonate of lime, it having in one form the figure of the carbonate of magnesia and iron, and in the other, that of the carbonates of barytes and lead.

"The fourth class consists of potassium, sodium and silver. The similarity of chemical properties of potassium and sodium is sufficiently evident; and although their compounds are not frequently isomorphous, yet there is good reason for attributing that to the dimorphism of each. Silver differs remarkably in its chemical relations from potassium and sodium, the only grounds for inserting it in this class is the isomorphism of the sulphate of silver with anhydrous sulphate of soda.

"The salts of potash are perfectly isomorphous with the salts of ammonia, which contain an atom of water; and hence, if the base of the ammoniacal salts ($N_1 H_3$ + H O) be written N H_4. O, it may be considered as an oxide of a compound radical which is isomorphous with potassium, and would rank, did we not know its composition, in the present group. This view of the composition of the ammoniacal salts was suggested by Berzelius, who gave to that compound radical the name ammonium; but I have since shown that the replacement is really two equivalents of a hydrogen compound, as already noticed in speaking of the second class.

"Fifth class, chlorine, iodine, bromine, and fluorine. This group is best characterized by similarity of chemical properties; and, so far as observation extends, their isomorphism appears to be complete. It is connected with the first and second classes by means of manganese, of which two equivalents replace, in truly isomorphous compounds, one of chlorine.

"Sixth class, nitrogen, phosphorus, arsenic, and antimony. In their chemical history, these compounds exhibit considerable, though not complete, similarity. The corresponding compounds of arsenic, and antimony, and phosphorus, are generally isomorphous, but in no case has isomorphism been observed between their compounds and those of nitrogen. A certain analogy appears to exist between those of nitrogen and those of the fifth class, as the nitric acid corresponds remarkably in properties to the chloric and iodic acids, with which, however, it is not isomorphous. Nitrogen seems also to replace oxygen in many cases in the proportion of one third its equivalent weight

"Seventh class, tin and titanium connected by the isomorphism of titanic acid and peroxide of tin.

"Eighth class, silver, and gold, from their isomorphism in the metallic state.

"Ninth class, platinum, paladium, iridium, and osmium, from the isomorphism of their double chlorides, by which also Graham considers this class to be connected with the seventh.

"Tenth class, tungsten and molybdenum; the tungstates and molybdates being isomorphous. These metals will probably be found to be of the same family with chrome, as chromate of lead has been found crystallized in the same form as the molybdate.

"Eleventh class, carbon, boron, and silicon; of these substances no isomorphous relations are known; they are brought together by a general, though imperfect analogy of properties.

"Graham makes no attempt at classifying mercury, cerium, columbium, lithium, rhodium, or uranium.

"I agree completely with the general principles of this classification, but, in a few cases, researches made since it was drawn up by Graham, render some alterations necessary; thus the similarity of constitution between the compounds of bismuth and copper, which had induced him to insert bismuth in the second class, has no real existence, and I

would transfer it to the same class with antimony; their sulphurets being isomorphous, and their chemical properties being, generally speaking, very similar. Indeed, it is almost certain that the oxide of bismuth is not a protoxide, but a sesquioxide, and hence corresponds to the oxide of antimony.

"I do not consider the isomorphism of sulphate of soda, and sulphate of silver, as being sufficient grounds for ranking the latter metal in the fourth class. We have already seen numerous examples of isomorphism amongst substances of totally different chemical constitution; and the properties of the compounds of silver resemble so completely those of lead, as to demonstrate positively that it belongs to the same natural group. When copper enters into combination with a double equivalent Cu_2, it becomes likewise a member of the lead and barytes group, as is shown by the sparing solubility of its sulphate and chloride, and being isomorphous with silver, furnishes additional evidence of its true position.

"Silver and gold being isomorphous only in the regular system, and their compounds being totally dissimilar in constitution, I do not retain the eighth class of Graham.

"I have satisfied myself of the perfect analogy of paladium with copper; it therefore must be separated from platinum, and removed to the second class. When mercury enters into combination with the equivalent 101, 4 (Hg,) it coincides, in the nature of its compounds, with paladium and copper, and attaches itself to the second class; but when its equivalent is 202, 8 (Hg_2,) its compounds resemble those of lead and silver, and, like copper, it then becomes a member of the third class.

"A classification of such as this, although necessary for the philosophical study of the relations of the simple bodies, could not, without considerable inconvenience, be strictly adhered to in an elementary work like this; I shall, therefore, having thus laid down those general principles, place it for a time aside, and commence the study of the non-metallic bodies, and their compounds with each other, without reference to any arrangement except that of treating first these subjects, that may be useful to towards understanding or illustrating those that follow."—Page 386.

In conclusion, we congratulate all lovers of chemical science on the appearance of so valuable an addition to the literature of that science. It is a work which should he in the possession of every man of science, manufacturer, physician, general practitioner, and druggist in the kingdom.

REVIEWS:—

ELEMENTS OF CHEMISTRY; *including the Applications of the Science in the Arts.* By THOMAS GRAHAM, F.R.S.L. & E. Professor of Chemistry in UNIVERSITY COLLEGE, London, President of the Chemical Society, &c. &c. Part VI. LONDON: HYPPOLYTE BAILLIERE, 219, Regent Street. 1842.

THIS work, the sixth and last part of which forms the subject of the present notice, is another most valuable addition to Chemical literature. This part is devoted to Organic Chemistry, which subject Mr. Graham has treated in a manner worthy of his deservedly high reputation, and it is with great pleasure that we add our testimony to the numerous others in its favor.

After giving full directions for organic analysis, with descriptions and wood-cuts of the various apparatus used therein, "referring for the minute instructions necessary for its exact execution to Professor LIEBIG's valuable tract on Organic Analysis, translated by Dr. W. GREGORY, and forming Part I. of Griffin's Scientific Miscellany," the author proceeds to treat of all the organic substances discovered up to the period of publication.

We may here give our readers, not as a specimen, for the selection would he attended with some difficulty, the learned Professor's remarks on the

"ACTION OF OXYGEN.—EREMACAUSIS.

"Organic compounds, when dry and in a state of purity, are generally capable of resisting the action of air or of free oxygen, at the usual temperature; but a considerable number are affected by that element: the various essential or fixed oils absorb oxygen in different degrees; the first becoming resins, and the second acquiring the drying properties of varnishes; the essential oil of bitter almonds is gradually converted into benzoic acid, and the vapor of ether passes slowly into acetic acid, while white indigo and other coloring principles undergo remarkable changes from rapid absorption of oxygen. The direct oxidation of alcohol and ether, which gives rise to acetic acid, is greatly favored by the contact of spongy platinum. The highly oxygenized acids act with much more energy upon organic compounds, and give rise to various products, according to the quantity of oxygen communicated. Thus alcohol is converted by oxidating matters into actal, aldehyde, acetic acid, formic acid, oxalic acid, carbonic acid, and water. In these re-actions the oxygen is frequently observed to effect the hydrogen exclusively, which is converted into water, while a quantity of oxygen exactly equivalent to the hy-

drogen thus withdrawn enters into combination with the remaining elements, and appears to be substituted for hydrogen. Thus by the action of four atoms of oxygen upon one atom of alcohol, $C_4 H_5 O + H O$, two atoms of hydrogen are withdrawn as water, while two atoms of oxygen are at the same time absorbed by the remaining elements of the alcohol, which becomes hydrated acetic acid, $C_4 H_3 O_3 \times H O$. When anhydrous sugar, $C_{13} H_9 O_9$, is treated with hypermanganate of potash, the nine atoms of hydrogen which the former contains are replaced by nine atoms of oxygen, and six atoms of oxalic acid is formed, $6 C O_3$.

"The presence of water greatly promotes the action of the oxygen of the atmosphere upon organic substances. The name *eremacausis* has been applied by LIEBIG to the slow combustion or oxydation of organic matters in air. Vegetable juices evaporated by a gentle heat in air allow a brown or brownish black substance to precipitate, known as *extractive matter*, and similar in properties from whatever juice it is formed. It is insoluble, or very sparingly soluble in water, but dissolved with facility with the alkalies. By the action of air upon solid animal or vegetable matters, a similar pulverulent brown substance is formed, known as humus. According to an observation of De Saussure, the sawdust of oak wood converts oxygen into carbonic acid without any change of the volume of the gas; but while dry sawdust lost three parts by weight of carbon in this way, it diminished in weight by fifteen parts altogether, showing that twelve parts of water were at the same time separated from the wood. Hence the proportion of carbon in decaying wood increases with the progress of its decay; and it is concluded that the hydrogen only is oxidised at the expense of the oxygen of the air, while the carbonic acid is formed from the elements of the wood (LIEBIG). The composition of pure woody fibre or lignin being $C_{36} H_{22} O_{22}$, two of one equivalent of hydrogen and three equivalents of carbonic acid. In all varieties of wood coal, the proportion of oxygen in relation to the hydrogen is diminished, these elements existing in the original woody fibre in the same proportion as in water, indicating a disengagement of carbonic acid from their substance, which appears still to go on at great depths in all strata of wood coal. The composition of the splint coal of Newcastle, and cannel coal of Lancashire being C_2 and $H_{13} O$; according to the analyses of both Richardson and Regnault, mineral coal is obviously formed from woody fibre in a different manner from brown coal. The composition of both splint and cannel coal is obtained by the subtraction of three atoms of carburetted hydrogen, three atoms of water, and nine atoms of carbonic acid from the formula of wood (LIEBIG):—

Three atoms of carburetted hydrogen . . .	$C_3 H_6$	$C_{36} H_{22} O_{22}$ = Wood.
Three atoms of water	$H_3 O_3$	
Nine atoms of carbonic acid.	$C_9 O_{18}$	$C_{12} H_9 O_{21}$
Mineral coal		$C_{24} H_{13} O$

Caking coal from Caresfield, near Newcastle, is $C_{20} H_9 O$, or contains the elements of cannel coal, *minus* the constituents of olefiant gas $C_4 H_4$ (LIEBIG). The inflammable gas of coal mines is principally light carburetted hydrogen, but it has been observed by Bischoff occasionally accompanied by notable quantities of olefiant gas. Such decompositions, however, are not different specimens of mouldered oak wood, (the humus from oak wood) and were found to be $C_{35} H_{20} O_{20}$, and $C_{34} H_{18} O_{18}$, or for every two atoms of hydrogen oxidised by the air, one atom of carbonic acid ($O O_2$) has been formed at the same time from the elements of the wood and set free. When water is present, and the access of air restrained, the decomposition of wood appears to proceed in a different manner; for while carbonic acid is generated as before, a certain quantity of water enters into chemical combination; white mouldering beech wood being found to have a composition corresponding with the formula $C_{33} H_{25} O_{24}$. There is reason, however, to suppose the interference in such cases of mouldering, of a species of fermentation, such as was observed when rags were placed in heaps and wetted in the preparation of a substance for the fabrication of paper, according to the old process in use before the application of chlorine. The rags became warm and disengaged a gas, while their weight diminished from 18 to 25 per cent. It is probable that in this, as well as other putrefactive processes, the oxygen of the water assists in the formation of carbonic acid.

"Wood coal or brown coal, which retains the structure of the wood unchanged, appears to be produced by a similar process of decomposition. A specimen free from bituminous matter was found to have a composition expressed by the formula $C_{33} H_{21} S_{14}$; and may therefore have been produced from

woody fibre by the separation due to eremacausis, and indeed take place in materials covered by such a mass of strata as must entirely exclude air, but are more analogous to the internal reactions of organic matters, known as a species of fermentation, in which the elements of a compound substance (such as sugar) divide themselves into two or more groups, without the incorporation of any extraneous element, except, perhaps, the constituents of water.

"The absorption of oxygen by alcohol in its acetification is a true eremacausis, so also in the process of nitrification. Oxidation is promoted in many organic bodies by contact with an alkali; thus alcohol holding potash in solution soon becomes brown from oxidation, and a resinous matter appears with all the products of the decomposition of aldehyde. The oxidation of gallic acid, hematin, and many other compounds, is promoted by the same influence; many vegetable substances exhibit a rapid absorption of oxygen on the addition to them of ammonia, and form splendid violet or red colored liquids, such as the coloring principles of the lichens. On the other hand, eremacausis seems to be entirely prevented when water is excluded, or when the substance is exposed to a temperature of 32°. The process of fermentation, putrefaction, which are different from eremacausis, appear to be necessarily preceded by an absorption of oxygen. Thus the juice of grapes pressed under mercury, and collected in a jar filled with that metal, was observed by Gay-Lussac to keep without change, but on admitting a bubble of air to the liquid, the vinous fermentation immediately commenced. The perfect exclusion of air is also the basis of the valuable process for preserving animal and vegetable food, without the use of antiseptics, first introduced by Appert. The materials are usually kept in canisters with a quantity of fluid, which is kept in a state of ebullition for some time, and the openings hermetically closed with solder, while the vessels are entirely filled with steam. Eremacausis is also prevented, or much retarded, by aromatic substances, empyreumatic substances, and oil of turpentine, the vapors of which retard the oxidation of phosphorous and phosphuretted hydrogen in a similar manner. It is also arrested by mineral acids, and salts of mercury, which appear to act by combining with the organic matter: alcohol, a strong solution of sugar, common salt, and many other saline substances, are supposed to owe their antiseptic properties in a great measure to their affinity for water, which reduces animal or vegetable matters in contact with them to a state of dryness in which they are little liable to decomposition. Thus a piece of dry butcher's meat covered with dry salt is found after twenty-four hours swimming in brine, the salt attracting water from the meat, and not leaving it humid enough for chemical action."

In conclusion, we may observe that no work is better entitled to the success it has met with.

LONDON ELECTRICAL SOCIETY.

Tuesday, Nov. 16th.

Mr. Gann communicated the "Results of an experimental Investigation into the Nature of Ozone." Our readers are aware that Prof. Schönbeira first instituted experiments upon that peculiar odor, emitted by an electrical machine, when in good action; and that he conceived it to proceed from some simple substance, to which he has given the name of *ozone*. (*See* "The Chemist," No. VII., July, 1840.) He assumes it to be an anion, the cathion of which is unknown; and states that it is obtained only when electrodes of the noble metals are employed.

Mr. Gann has succeeded in obtaining it when *copper* and *iron* terminals are brought into connection and separated repeatedly in jars of air on the several gases; although it is true these terminals are not properly electrodes: under such circumstances, he concludes, however, that it is liberated or developed by all metals under certain conditions.

A very interesting experiment was described, wherein the gas obtained from the electrolysis of muriate of soda was examined. That developed at the anode presented no odor until the chlorine, with which it was mixed, had been washed from it by means of ammonia. When the odorous oxygen, obtained by electrolysis, was mixed with the proper proportion of hydrogen and exploded in the eudiometer, the odor departed with the gas; it also left oxygen, through which a succession of sparks from the Leyden jar had been passed. As the experiments on this subject are still in progress, we forbear giving the opinions which the author states upon it.

A fourth paper from Mr. Pine was read, on "The Tendency of Electricity to promote the Growth of Plants." Also a note from Mr. Noad, Mem. Elec. Soc. "On the Powers of his Water Battery;" of which a notice was given in the last number of "The Chemist."

The Secretary then read a translation of a "Note upon a Phenomenon presented by Solution of Nitrate of Silver, decomposed by the Current," by M. C. Matteucci. If a weak solution of this salt is electrolyzed, the

cathion is a black deposit, but it assumes the silver hue, the instant that the current ceases to pass; and if, after a portion has become white, it be dropped between the electrodes, while the current is passing, it becomes black on the sides of the particles towards the anode. Several modifications of these experiments were given, the whole seeming to prove that the black deposit is an oxide of silver, and that it is reduced to its metallic form by the mere separation of the current.

The Secretary then read extracts from Mr. Weekes' Electrical Register for October, of the condition of the atmosphere in connection with its meteorological phenomena.

II. CHEMICAL MANUFACTURES.

FORMATION OF METALLIC COPPER PLATES FROM COMMON IMPRESSIONS OF PRINTS, BY THE AID OF THE ELECTROTYPE.

To the Editors of The Chemist.

Nov. 22nd, 1841.

GENTLEMEN,—

I have the satisfaction of being able to announce to you that I have procured electrotype plates from impressions of steel engravings, and also of lithographs and wood-cuts. The method I adopted is simply this:—I wetted the back part of the engraving with a solution of nitrate of silver, until the face thereof exhibited a damp surface on those parts only where no lines were seen. When this was done, the face, or printed side, was exposed to the vapors of phosphorized alcohol, when a metallic lustre immediately presented itself. It was then dried and cemented to a piece of wood, and introduced into a proper electrotype apparatus, and a cast was deposited thereon in the usual way.

If you think this piece of information of any service to the readers of your excellent work, it is entirely at your service.

I am,
Gentlemen,
Your obedient servant,
M. NEWNUM.

EXPEDITIOUS METHOD OF MULTIPLYING COPPER-PLATES BY MEANS OF THE ELECTROTYPE.

BY T. W. NAYLOR.

THE method I am about to describe furnishes means of multiplying engravings to an almost unlimited extent, and with great expedition, since only one operation in the galvanic apparatus is necessary, and by the employment of the decomposition cells, recommended by Professor Jacobi, any number may be executed at one time, premising that a sufficient supply of apparatus is provided. The process is simply as follows:—A sheet of smooth paper is covered on one side with japanner's oil size, and then evenly spread over with bronze powder or black lead: when this is done, and the paper has become thoroughly dry, apply the metallic surface thereof to the face of steel or copper plate, and pass them together through a press of sufficient power to make on the prepared paper a good sharp impression, having the lines, &c. in relief; and when this is cemented to a piece of wood, it forms the mould or matrix intended to receive the subsequent deposit of galvanic copper.

ON THE EXTRACTION OF INDIGO FROM THE POLYGONUM TINCTORIUM.*

BY M. TROREL.

AT the commencement of October, 1839, Count Chastellux, who for two years had successfully cultivated the *polygonum tinctorium*, employed me to extract the indigo from it: my trials terminated about the end of the same month. I took notes, which I gave to the Count. This little memoir reached the ears of M. Robiquet, who wished me to send it to the *concours*, then open: I declined doing so, finding it too incomplete.

I had not then any data concerning the method to be adopted; I knew only that M. Baudrimont had tried precipitation by acids, and that he had not succeeded in the large way.

I first analysed some polygonum: I ascertained that the stems contain no coloring

* *Journal des Connaissances Necessaires*, Nov. 1841.

matter; that the dry leaves did not give any, either with the acids or with lime. I treated the fresh leaves successively:—

1st. By hydrate of lime mixed with the juice of the leaves. I obtained only a greenish matter, almost entirely compound, of fecula and impurities.

2nd. By prolonged maceration (eight days), stirring the liquor for ten hours, and lime water. Bad product, of a dull green.

3rd. By digestion during four hours, in water heated to 60° C. It was then that I wished to try what acids, and what proportions of them, would be the best adapted for precipitation: all were successively tried with a quart of liquor. By sulphuric acid, 12 grammes promptly precipitated the coloring matter; by hydrochloric acid the same effect, but in rather a longer time; 3 parts of hydrochloric acid and 1 part of nitric acid yielded fine indigo.

4th. By infusion for ten hours, water heated to 80° C. and with beer yeast. I obtained a fine product, which, with other samples, was sent to M. Robiquet. The following is the process at which I stopped:—5 kilogrammes of leaves were bruised in a small tub, with a hurdle to maintain them. I poured in 40 quarts of water, heated to 80° C., into which I put 60 grammes of yeast; ten hours after, the liquid was decanted, and passed through a very fine horse-hair sieve. I then gradually poured in 375 grammes of sulphuric acid, and stirred the liquor for a quarter of an hour with a piece of willow. A scum of fine blue color, and in large quantity, was formed; this mass was removed, and the liquor, left in the air for twenty-four hours, was laid on a cloth. I added the scum, and I washed this precipitate with water containing 1½ per cent. of potassa, then with spring water; I dried it on a stove, and I obtained 24 grammes of indigo, of very fine quality.

Beer yeast has since been disputed. I had ascertained that when there was a powerful disengagement of carbonic acid, indigo was more promptly produced; and that if oxygen were absorbed without a sensible disengagement of that gas, the blue color was weak. This induced me to use yeast. I had also remarked, that if the infusion be prolonged beyond twelve hours, precipitation by the acids no longer took place.

Count Chastellux, by his experiments last year, discovered that hydrochloric acid might be used instead of sulphuric, and that less might be put in. Again, the infusion must not be prolonged beyond six hours.

The production, according to my experiments, may be estimated at 625 grammes for 100 kilogrammes of leaves.

Count Chastellux collected, in the year 1839, from 2 acres, 120 kilogrammes of leaves, or at the rate of 6000 kilogrammes to the hundred acres, which should give 37½ kilogrammes of indigo, which may be estimated at 16 francs per kilog., or 600 francs the crude product of a hundred acres. Count C. accurately noted all the expenses in general; he found, including rent and seed, 240 francs the hundred acres,—rent and hand labor being very low here. There would remain, therefore, 360 francs clear profit.

The process of extraction is so simple, that every farmer might carry it on. The following is a good arrangement of the place and of the means to be employed in this operation. Either the plant, stalk and leaves, may be indiscriminately cut, or the leaves alone may be gathered.

It is necessary, as much as possible, to have water at hand, and a very airy outbuilding. On an even line, and at 1 metre and 30 centimetres above the ground, a row of large tubs should be placed close to each other. Under each of these there should be a tub containing nearly double the quantity of water used in each infusion; these two series should be furnished at the bottom of each with a stop-cock: the tubs destined to receive the decantation should be placed at least thirty centimetres from the ground. On each side, in order not to restrain the circulation and the work, two great reservoirs should be established, 30 centimetres deep, and each furnished with two stop-cocks, one at the bottom and the other six centimetres higher. Along this lower series of tubs, and under the stop-cocks, a wooden shoot should be established, going to the reservoirs placed on each side.

There should be made in the ground, under the lower stop-cock of the reservoir, a large hole, of at least one metre in depth, to place it in a filter. It is there that the coloring matter flows out. It would also be advantageous to place the boiler on a furnace sufficiently high for the warm water to arrive, by means of tubes, as high as the tubs.

The infusions are commenced at 5 P. M., putting into each tub 20 kilogrammes of bruised leaves, with a hurdle, and pouring on 100 quarts of water at 60°. Next day, at 5 o'clock, it is decanted, putting under the stop-cocks a very fine horse-hair sieve; the leaves are washed with ten quarts of warm water. This is to be followed up, by adding to each tub containing the decanted liquor, 500 grammes of hydrochloric acid, by degrees, stirring rapidly for a quarter of an

hour; the scum, which will lie at the surface of the liquid, is then removed, and the liquor is afterwards let out into one of the reservoirs. It will then be necessary, in order to keep back all the impurities formed by the stirring, to place either a cloth or a very fine sieve under the stop-cock of the tuhs; twenty-four hours after, the precipitate being quite formed, the water is to be run out by opening the upper stop-cock; and the lower one is afterwards to be turned. The blue scum set aside at the time of the stirring is then added. This precipitate must afterwards be washed with a very weak solution of potassa, then with spring water; it is afterwards put in moulds and dried on a stove.

III. PHARMACY, &c.

PHARMACEUTICAL JOBBING.

To the Editors of The Chemist.

Gentlemen,—

You have incidentally alluded, in some of your past numbers, to the mismanagement and inefficiency of the Pharmaceutical Society; but there are some points which deserve notice, upon which you have not touched.

In looking over the list of the Council of the Society, and finding among them some of the leading members of the London druggists, it surely would not be too much to expect a manifestation of ability, energy, and entire disinterestedness. But what evidence have we of the exercise of these qualities? The Society appears to be represented by one individual,—not the President, nor the Vice-president, nor the Secretary,—but by a member of the Council,—restless, ambitious, and anxious to get all into his own hands. Availing himself of the position which he has forced himself into, he has already commenced making the Society a means of emolument and profit. Is it fitting, that a member of the Council should be receiving the funds of the Society in exchange for the scientific contributions which are furnished by the members? In other bodies, similarly constituted, it is customary to have a law to the effect that no member of the management shall derive any emolument from the funds under his charge. Ought not this to be the case with the Pharmaceutical Society? The reason that no premises are taken for the Society, and no official measures adopted for carrying out the objects at first contemplated, is very evident; while an individual is thus raising himself into notoriety—taking the whole business into his own hands, and his own house—and establishing a property for himself, which he draws from the funds of the Society.

There are many who, like yourselves, have been disgusted by observing the gross jobbing that is going on.

I am, Gentlemen,
Respectfully yours,
Junius.

[We entirely agree with Junius, and wonder that the Council and members have so long been blind. They ought immediately to put a stop to so glaring a deviation from the proper management of the affairs of the Society. We know that many members of the Council are dissatisfied at what our correspondent complains of. The advertisement to this number contains our opinions in full, and our readers will see that we have been on the watch, if others have neglected their duties.]

ON THE IODIDES OF GOLD, WITH REMARKS ON THE PROPORTIONAL NUMBER OF THAT METAL, AND ON THE MEDICINAL EMPLOYMENT OF THE PROTIODIDE.*

BY M. J. FORDOS.

Being employed, by M. Soubeiran, to prepare some iodide of gold for the use of the hospitals, I followed exactly the process given in the French Pharmacopœia, and I obtained nothing but a brownish product, containing very little iodine. This compound was totally different, in its physical properties and composition, from the iodide of gold described in books; it was a mixture of iodide of gold and reduced metal. I wished to account for this alteration; hence the following researches, whose results, although very incomplete, cannot but be interesting. I could have desired to extend these investigations still further before publishing them; but being compelled to in-

* *Journal de Pharmacie*, Nov. 1841.

terrupt them, and not knowing when I shall be able to resume them, I thought that science might gain something from their publication, and I therefore determined to publish them. But, before proceeding further, I should state that the study of gold is attended with the greatest difficulties. It is a capricious metal: Proust and Pelletier found it so, and I am convinced that no experimentalist will find researches on gold easy.

Hitherto we knew of only one compound of gold with iodine; that was the combination formed with one proportion of gold and one of iodine, the protiodide described in books, and we know but very little concerning the properties and preparation of this compound. The experiments which I have made are of a nature to throw some light on the properties and preparation of the protiodide of gold: they seem also to remove all doubt of the existence of an iodide corresponding with the perchloride. Finally, they tend to confirm the proportional weight of gold established by Berzelius, which M. Pelletier had been led to modify, in consequence of his analysis of the protiodide of gold.

PROTIODIDE OF GOLD.

Protiodide of gold is presented under the form of a citron yellow, or greenish yellow powder. The latter character generally (I do not say *always*) indicates an excess of gold, or rather of free iodine. At least, iodide of a fine yellow, when first prepared, becomes greenish yellow after being kept some time, iodine being disengaged.

Iodine and gold are very feebly combined: thus, under many circumstances, these two elements are separated. Time, unaided, appears to be capable of destroying the combination. Iodide of gold contained in a flask liberates iodine, which corrodes the cork, the sides being at the same time covered with a golden layer. The product becomes of a dirty, greenish yellow, which goes on increasing; and if the iodide be analysed, its composition is found to be altered. A temperature of from 50° to 60° C. considerably accelerates this decomposition. By placing iodide of gold in a drying stove heated to the above temperature, at the end of a few days, it is seen to be covered with reduced gold. This alteration may be much more readily obtained, by exposing the iodide to a current of dry air, at the temperature of 70° C.: I was enabled to obtain, in three hours, a layer of reduced gold on the surface of the iodide. Finally, if the iodide be heated to the temperature of 120° C., or even a few degrees below, it is decomposed into iodine, which is disengaged, and into gold, which remains.

The iodide of gold is inodorous: however, when it has remained some time in a flask, it gives out an odor of iodine, owing to incipient alteration.

It is insipid; but, if kept in the mouth, it becomes sapid, on account of its decomposition by the saliva.

Chlorine, dissolved in water, does not exert any action on iodide of gold; but gaseous chlorine instantly decomposes it, and if we operate under the influence of a small quantity of water, a yellowish liquor resembling the solution of perchloride of gold is obtained.

Bromine also instantly decomposes the protiodide of gold, and if a small quantity of water be added, an orange coloured liquor is obtained, which probably contains perbromide of gold.

Iron filings, put in contact with the protiodide of gold, under the influence of water, removes all the iodine, liberating the gold. An iron spatula, used in forming the iodide, would be sufficient for altering it.

The protiodide of gold is insoluble in water, whether cold or warm; but water decomposes it at the boiling temperature into iodine, which is disengaged, and gold, which remains. The decomposition is slow, but complete.

Caustic potassa instantly decomposes it; the gold is set free, and, doubtless, iodide of potassium and iodate of potassa are formed.

Sulphuric and nitric acids do not alter it at the ordinary temperature; but, with heat, they decompose it into gold and iodine. They evidently act only by the elevation of temperature. What action, indeed, can be exerted on iodide of gold by sulphuric and nitric acids, since it has been shown that there exists neither nitrate nor sulphate of gold?

Hydrochloric acid acts like the two preceding acids; it is only at the boiling temperature that it decomposes iodide of gold. It is true, however, that, towards the temperature of 70° C., I have seen it take a reddish tint in its contact with the iodide.

If sulphuric, nitric, and hydrochloric acids have no action on iodide of gold at the ordinary temperature, it is not so with hydriodic acid; the latter is instantly decomposed; it assumes a reddish or brown color, more or less deep, according to the proportion of the iodide of gold to the hydriodic acid. A portion of gold is set free, and there remains in the liquor about one third of it, which I suppose to be in the state of hydriodate of periodide of gold.

Iodide of potassium acts like hydriodic acid, only one part of gold is set free, and there remains in the liquors gold, which I

suppose to be a double iodide of gold and potassium—(iodaurate of iodide of potassium).

Protiodide of iron presents the same phenomena as iodide of potassium.

Chloride of sodium and chloride of ammonium exert only a very slow action on protiodide of gold, even when heated to 35° C.

Sulphuric ether decomposes iodide of gold: the gold is set free, and the iodine is dissolved in the ether. However, the liquor always contains a little gold, I know not in what form; perhaps in the state of periodide.

Alcohol at 40° Cartier (96° Centesimal) acts like ether, except that its action is slower: it is some hours before the action commences, and it is only at the end of several days that the decomposition is complete. Weak alcohol has a slower action than concentrated; but etherised alcohol has a much quicker action.

Sugar-candy, mixed with iodide of gold, gives a powder of a fine yellow, which speedily becomes of a dirty yellow, owing to the decomposition of the iodide. The cork of the flask was greatly corroded by the iodine.

A mixture of gum arabic and iodide of gold underwent no appreciable alteration during the time that I kept it, namely, from the 27th of October, 1840, to the 6th of January, 1841. Gum arabic retards, or, perhaps, even prevents the slow decomposition of the iodide. I think, therefore, that the protiodide of gold should be mixed with gum arabic, to facilitate its preservation.

Iodide of gold, under the form of pills made with gum arabic and a little water, underwent no alteration during eight weeks.

Pommade, made with iodide of gold and lead, is decomposed from one day to another.

The saliva is far from being without action on the iodide of gold. After some time, the latter communicates to the former a brownish violet tint, at the same time being powerfully altered.

When the stomachal membrane of a cat, wetted with distilled water, and sprinkled with iodide of gold, was placed in a stove heated to 35° C., the latter was found to be considerably altered, and the stomach was covered with a reddish liquid. The stomach, in the living animal, appears to me to exert the same action.

The study of the properties of iodide of gold, will enable us to obtain this combination, with constantly uniform physical and chemical properties. But, previously, it may not be uninteresting to throw some light on the various modes of preparation given in works, and to show the difficulties in obtaining a pure product.

According to the Pharmacopœia (French), a solution of iodide of potassium is added to a solution of perchloride of gold, until no more precipitate is formed. The precipitate is laid on a filter, and it is washed with alcohol to remove the iodine precipitated at the same time as the iodide, and the chloride of potassium formed. In calling to mind the action which alcohol exerts on iodide of gold, we can easily form an idea of the little uniformity in the physical properties and the composition of the iodide thus prepared. It has sometimes happened, in operating on fifteen grammes of precipitate, that I have obtained only a product of a brownish color, containing very little iodide of gold. However, a beautiful product may be obtained by the process of the Pharmacopœia; but it is only with much difficulty, and by observing many precautions which are indispensable: it must be washed in an *éprouvette* by agitation and decantation: the alcohol and the iodide should be left in contact as short a time as possible; and care must be taken, as soon as the alcohol acquires no more color, to wash the iodide with distilled water, in order to remove the last traces of alcohol. It is necessary that the alcoholic washings should be made in a few hours.

If, instead of heating the above precipitate by alcohol, it be boiled in alcohol, in order to drive off the iodine, as MM. Berzelius and Dumas recommend, we obtain only a mixture of reduced gold and iodide of gold; and if the iodide of gold were long kept in boiling water, ultimately nothing but metallic gold would be obtained.

I have repeated the process, which consists in acting with iodurettted hydriodic acid on divided gold, operating as directed by M. Pelletier; that is to say, by treating divided gold to which I added nitric acid, little by little, so as to leave an excess of hydriodic acid: I filtered it. I obtained a deep brown liquor, which I suppose to be hydriodate of periodide of gold. On adding to this liquor, when hot, some nitric acid, a vivid action ensued, and an abundant precipitate was formed, consisting of a mixture of iodine and iodide of gold. M. Pelletier recommends the iodine being driven off by ebullition; which destroys one part of the iodide: it is far preferable to have recourse to the heat of the stove, as I shall presently show.

I have not tried the process which is based on the reaction of hydriodic acid on peroxide of gold. It may easily be conceived that water and protiodide of gold must be formed, and that iodine must be set free. These are, therefore, the products which will be obtained by not employing an excess of

hydriodic acid; for this acid in excess would give rise to the formation of hydriodate of periodide of gold.

The process which I have employed for procuring the protiodide of gold which I used in my experiments, is based on the same reaction as that of the Pharmacopœia; but it differs from it in the mode of separating the iodine precipitated at the same time as the iodide. The following, however, is a process which I consider useful to be given in detail.

I put a solution of perchloride of gold into an *éprouvette;* I added to it, gradually, a solution of iodide of potassium, taking care to agitate it, and to let it rest each time, in order to see whether the iodide rendered the supernatant liquor turbid. I knew when the period of the complete precipitation of the gold approached, by the promptitude with which the precipitate was formed, and the faint reddish color of the liquor. At this time, the iodide of potassium required to be added drop by drop. It is easily perceived, moreover, when an excess of iodide of potassium is put in: the liquid acquires a deep color, owing to the solution of the iodine or the iodide of potassium in excess. An excess of alkaline iodide may give rise to the soluble iodaurate. When the precipitation is finished, it is allowed to settle, and is decanted. Distilled water is added to the precipitate, it is agitated, left to settle, and decanted a second time; this is continued until all the chloride of potassium is removed. The matter is then laid on a filter and allowed to drain; then the filter is spread on several folds of blotting paper, placed on a plate, which is put in a stove heated to 30° or 36° C. The surface of the matter is removed twice a day, or only once in the twenty-four hours: at the end of three or four days, according to the thickness of the layer, all the iodine is dissipated, and we have a protiodide of a fine yellow, which is not sensibly altered by a longer stay in the stove. Iodide of gold, prepared on the 20th of September, 1840, and left in the stove until the 27th of the same month, gave by analysis, one gramme being operated on:—

Sept. 24.	Iodine, 0·390	Gold, 0·610
25.	0·391	0·609
26.	0·390	0·610
27.	0·391	0·609

The process which I have just described has always yielded me an uniform light product, of a fine yellow color. It is preferable to that of the Pharmacopœia, since it more readily gives always an identical product, when the temperature of the stove has not been carried too high. In this process, instead of precipitating the perchloride of gold by iodide of potassium, it may be precipitated by hydriodic acid. Care must also be taken not to put in an excess of either precipitant, which, as with iodide of potassium, would have the inconvenience of giving rise to the formation of soluble double iodides: iodide of gold and hydrogen (hydriodate of periodide of gold), or iodide of gold and iron. I also think it preferable to precipitate the perchloride of gold by iodide of iron, prepared immediately before, which, consequently, is neutral, while iodide of potassium almost always contains free alkali, which is disadvantageous. I even think that it is generally the best way to substitute protiodide of iron for iodide of potassium, in the preparation of insoluble iodides, which are obtained by double decomposition. Thus, for example, it is more easy to obtain iodide of lead of a fine yellow with iodide of iron than with iodide of potassium. I have seen the latter give, in the solution of acetate of lead, only a greenish yellow precipitate.

COMBINED PERIODIDE OF GOLD.

We have seen that, when the protiodide of gold is treated by a solution of iodide of potassium, only one portion of gold is set free, and that the filtered liquor contains gold in solution. Everything leads us to believe that this liquor contains iodaurate of iodide of potassium: but when evaporated on the open fire, or in a sand bath, iodine is disengaged and gold is precipitated. Evaporation in a moderately heated stove likewise gives rise to disengagement of iodine and precipitation of gold. However, crystalline needles are sometimes obtained, which are very soluble in water, and whose solution gives, on second evaporation, iodine which is disengaged, and gold which is precipitated. I have never obtained but a small quantity of these crystals, and I have never been able to isolate them from the metallic gold which accompanies them.

The solution arising from the action of hydriodic acid on protiodide of gold, and in which I suppose the existence of hydriodate of periodide of gold, is also altered, even by evaporation in the stove. However, here again I sometimes obtained crystalline needles, containing combined gold, but in too small quantity for them to be separated from the gold, or for them to be submitted to examination.

PROPORTIONAL NUMBER OF GOLD.

I found protiodide of gold to present the following composition:—

	First Analysis.	Second.	Third.	Fourth.
Gold . .	0·612	0·611	0·610	0·612
Iodine .	0·388	0·389	0·390	0·388.

Which makes, in hundredths, taking the mean of the above four analyses:—

Gold	61·125
Iodine . . .	38·875
	100·000.

In equivalents :—

Gold .	1 equivalent	. 248·60
Iodine .	1 ditto	. 157·95
		406·55.

I here give only four analyses ; but I made ten, all agreeing with one another. However, I add more confidence to the above, because they were made with two iodides of whose purity I have not the least doubt. I operated each time on a gramme of matter, which I decomposed in a small capsule placed on red-hot coals. Moreover, I dried the iodide in the following manner, before analysing it. I placed a capsule containing iodide of gold on an experimenting glass, surrounded at its base with fresh burnt and still warm lime. I covered the whole with a bell glass, which I luted to a marble slab. I left the iodide under this bell for fifteen days : I then analysed it, with the above result.

I also made several analyses with iodide of gold taken from the stove, or in the flask wherein I kept it ; I never found its composition appreciably differing from the above.

I did not analyse the iodide dried at 100° C. In an experiment wherein I exposed some of the iodide to the temperature of 70° C., in a current of dry air, I effected, at the end of three hours, an evident alteration of the compound. I never repeated the experiment.

I did not analyse this iodide dried *in vacuo:* the easy alteration of this compound made me fear injuring the pneumatic apparatus, which I should have used for that purpose. However, I ascertained that a gramme of iodide of gold, left for twenty-four hours in the vacuum, by the side of a vessel containing sulphuric acid, did not undergo any very sensible diminution of weight.

Thus iodide of gold is formed of

Gold	61·125
Iodine . . .	38·875

By admitting the proportional weight of gold of Berzelius, and by considering the iodide as formed of one proportion of gold and one proportion of iodine, we arrive at the following results :—

Gold	61·15
Iodine	38·85
	100·00,

numbers scarcely differing from those which I obtained.

M. Pelletier assigned the following composition to iodide of gold :—

Gold	66
Iodine	34
	100,

numbers which very greatly differ from those which I obtained, and from those indicated by theory, admitting the proportional number of Berzelius.

M. Pelletier, struck with this result, and being persuaded that iodide of gold is of uniform composition, thought that the Berzelius' proportional number of gold should be modified, or that a composition should be admitted for this iodide very different from those which this kind of combination in general presents. The first opinion was most rational : M. Pelletier adopted it, and proposed the number 299, for the equivalent number of gold, instead of 248·6, that of Berzelius.

But it results from the analyses, which I have made of iodide of gold, that this compound, when pure, is composed, in hundredths, of

Gold	61·125
Iodine . . .	38·875
	100·000,

and not of

Gold . . .	66
Iodine . . .	34
	100,

as M. Pelletier found. That chemist had, without doubt, analysed some iodide containing free gold : hence the excess of gold in the analysis ; hence also the higher proportional weight.

The analysis of iodide of gold, far from leading to a modification of the equivalent of gold of the illustrious chemist of Stockholm, tends, on the contrary, to demonstrate its accuracy.

MEDICAL EMPLOYMENT OF IODIDE OF GOLD.

The employment of iodide of gold in medicine does not appear to have given always uniform results. But have practitioners always employed a pure iodide ? Have they taken into consideration its easy alteration ? Have they avoided associating with it substances capable of decomposing it ? These are questions which one is very naturally led to ask; and if we consider, in the first place, the difficulties which present themselves in the preparation of protiodide of gold, and, in the second place, the few data which we possess concerning the instability of this combination, we shall see the necessity of making new investigations before banishing from therapeutics a medicine, which, better administered, may perhaps be of great ser-

vice in the healing art. It is not, however, that I wish to make an apology for protiodide of gold; for experience alone can teach us its importance in medicine. But experiments ought to be made with protiodide of gold associated with substances incapable of altering it. Gum arabic appears to me to possess this property. The iodide may be administered, mixed with gum, under the form of pills, or, better still, under the form of powder. When it is wished to employ it as a pommade, it must be made just before being used.

ON THE PREPARATION OF MEDICINAL WATERS WITH MAGNESIA.

To the Editors of The Chemist.

Gentlemen,—

Your correspondent, J. S., *of Peterborough*, alluding to the preparation of medicinal waters, states, that during his experience he has frequently observed the inferiority of watcrs prepared by triturating the essential oil with magnesiæ carbonas in comparison with those distilled, and asserts that they are always weaker, and that they soon spoil. As regards strength, that may be increased *ad libitum;* and with respect to the latter assertion, I have generally found the case to be the reverse, when prepared with aqua distillata. J. S. further states, that he has "lately made an observation which leads him still more strongly to object to such waters." The value of his observations and experience may be estimated by the following discovery, which he has made known, and for which the profession, doubtless, will be much indebted. He writes, "Having occasion to dissolve ℥ss ferri iod. in ℥xij aquæ cinnamon., I perceived an immediate decomposition, and from the rich color recognised ferri sesquioxydum;" a singular recognition, truly! My attention having been drawn to the remarkable conclusion thus inconsiderately arrived at by your correspondent, I was induced to investigate the circumstance, doubting the correctness of his deductions, and I beg to lay before you the results. I tried a solution of the iodide with aqua cinnam. prepared without magnesia, according to the formula he recommends as being calculated to obviate "the decomposition of those delicate chemical combinations, the use of which daily increases," and found, as he would have done had he tried the experiments, the same indication of the presence of ferri sesquioxydum, viz. the rich color.

He continues: "On inquiring, I found that the water was prepared with magnesia, and I therefore concluded that the small quantity of magnesia in solution, having passed through the filter, decomposed the ioduret of iron, forming ioduret of magnesia, and precipitated tbe ferri sesquioxydum, formerly called subcarbonate of iron." The fact is, no decomposition takes place; J.S. having confounded the simple act of precipitation with that of decomposition, inasmuch as the precipitate he observed, consisted simply of surplus iodide of iron, which the menstruum employed would not thoroughly dissolve, and not of carbonate of iron, as J. S. imagined, and which, in his anxiety to publish his acquaintance with chemistry and modern nomenclature, he had styled a sesquioxide. The rich color which influenced your correspondent in his decision, is partly to be ascribed to a slight action of the iodide upon the essential oil contained in the cinnamon water, but chiefly to the coloring principle existing in the iodide itself, of which J. S. seems to have taken no account. When a solution is attempted with aq. cinnam. *distill.*, there is a very perceptible granulation of the precipitate, which is deposited almost instantaneously, probably owing to the greasy nature of the water, leaving the fluid less highly colored than when a solution is attempted with water in which the essential oil is simply suspended by minute division, as when triturated with or filtered through pulv. carb. lign., which, I believe, is universally admitted to be insoluble. Simple distilled water would, therefore, appear to be the best aqueous menstruum, though J. S., according to his notions of decomposition, might still pronounce it as incompatible, as there is an impalpable though evident precipitate, and the liquor also evinces a disposition to assume the red color on standing, but which is not distinctly visible, unless held between the eye and the light, owing to the opacity of the liquor from the more perfect solution of the iodide.

Here I may remark, of how frequent an occurrence, experienced by every dispenser, is the employment of various liquids as solvents or vehicles for tbe administration of remedies, which are already charged to saturation with other matter: the effect, in certain cases, is obviously either decomposition or precipitation, and the one is not unfrequently mistaken for the other, as in the above case. As there has been of late some little disquisition respecting the relative merits of the London and Country Chemists, it would be as well that gentlemen should hesitate ere they promulgate their opinions too extensively; at least, it is desirable that they should practically test those opinions, and satisfy themselves that their

conclusions are correctly drawn ere they assume to themselves the office of instructing their brethren in pharmacy. In the above case, your correspondent has been guilty of propagating a gross fallacy; and with an ostentatious display of pretended chemical knowledge, has attempted to explain, on philosophical principles, an effect which never existed but in his own imagination. I am, Gentlemen,

Very respectfully yours,

St. Marylebone, G. C———.
Dec. 1841.

To the Editors of The Chemist.

Gentlemen,—

I should be glad to learn, through the medium of your valuable Journal, the best extemporaneous formulæ for the following preparations, viz.—

Arquebusade,
Eau de Cologne, and
Bleaching liquid.

The first is not much in request now, but the genuine article is very dear; the second is also, for the preparation of which I possess Dr. Granville's receipt, but cannot always obtain the essences therein directed: with regard to the last, I know it is customary with some druggists to sell the sol. calc. chlor. as a cheap substitute for Hudson's liquid; but unless the strength is regarded carefully, the texture of the cloth or linen is destroyed.

I would also beg to inquire whether any of your correspondents or readers have yet made use of the naphtha rectificat. as a menstruum for tinctures, such as tinct. myrrhæ, &c., and if so,—does it answer? I do not see why it should not be used for such purposes, thereby effecting a very great saving in the preparation of horse tinctures, embrocations, &c.

Yours respectfully,

Dec. 13, 1841. Enquirer.

MAGNETIC OXIDE OF IRON.

To the Editors of The Chemist.

Bristol, Nov. 29th, 1841.

Gentlemen,—

Having occasionally heard the black (or as it is not unfrequently called the magnetic) oxide of iron extolled as superior to other ferruginous preparations, without being able to find any thing more than mere cursory remarks respecting it in medical works, you will not, I hope, consider it an unwarrantable liberty, if I make your Journal the medium through which to inquire the peculiar properties and advantages of this preparation.

Perhaps, in an early subsequent number, some one of your readers, who may be better informed, will be kind enough to give me his opinion on the article, with a statement of its advantages and peculiar effects as a remedial agent.

I am, Gentlemen,

Your obedient Servant,

A Subscriber.

UNION OF THE REGULAR PRACTICE AND HOMŒOPATHY.

PATHOLOGY, *founded on the Natural System of Anatomy and Physiology; a Philosophical Sketch, in which the Natural Classification of Diseases, and the Distinction between Morbid and Curative Symptoms, afforded by Pain or its Absence, are pointed out; as well as the Errors of Homœopathy and other Hypotheses.* By Alexander Walker. London: John Churchill, Princes Street, Soho.

We inserted in our Journal some months ago, with a perfectly impartial feeling, several letters from a worthy contributor, embodying a general view of the homœopathic practice, which gave rise to critical remarks in deterioration thereof, from another respected correspondent. Our readers could not have failed to witness the warmth which was displayed by each of the worthy disputants, and which was obviously waxing to a temperature beyond the tolerance of peace-loving editors, whose only aim was the elucidation of truth. Both parties, however, laid down their weapons, perhaps in despair of any prospect of either becoming a proselyte to the opinions of the other. It is with this contest fresh in our minds, that we draw the attention of our readers to a new work, which professes to explain the whole matter between such disputants. The volume will be allowed, on perusal, to be a masterly composition, containing many new views on pathology; but its chief importance consists in the inculcation of the necessity of a scientific union of the regular practice with that of the homœopathists. The following is a digested view of its contents:—

The Author has—given the natural arrangement of anatomy, physiology, pathology, and materia medica;—more accurately analysed some fundamental principles, as to the causes, nature, and symptoms of disease;—shown that past hypotheses of the nature of disease have been founded upon partial views of the functions;—proved that diseases present to us deranged organism and function,

and an effort of nature to cure these ;—distinguished symptoms into morbid and curative, directly opposed to each other ;—proved that the distinction between morbid and curative symptoms is essential to all scientific practice, and that, after collecting the symptoms, the first object is to distinguish the morbid from the curative ones ;—shown that the art of medicine consists in the management of both kinds of symptoms—opposing the former and assisting the latter ;—proved that the law "contraria" is the guide for the treatment of morbid symptoms, to which no other law is applicable ;—shown that minute doses are inapplicable in acting according to this law ; or that morbid symptoms require proportionally larger doses ;—proved that the law "similia" is the guide for the treatment of curative symptoms, or assisting the efforts of nature ;—explained the efficacy of homœopathic medicines and of minute doses—as being in harmony with and coming in aid of the curative symptoms ;—proved that pain distinguishes the morbid from the curative symptoms ; explained its cause and nature ; shown that it precedes and causes the curative symptoms, by inducing slight injection or incipient inflammation of parts ; and that the latter is the instrument of the vis medicatrix naturæ ;—shown how far both parties, regular and homœopathist, are right, and how far wrong ;—established the truth and the precise and definite application of the two great laws ;—shown the necessity of their union and application in a natural system.

We congratulate our numerous Pharmaceutical friends, that this really scientific treatise bids fair to maintain their establishments in their present amplitude of supplies, and that there will henceforth be no misgivings, that the medicine chest will dwindle down to the dimensions of a moderate sized snuffbox. But the *purity* of the small-dose practice will be indispensable !

The work is very handsomely printed ; and the reader cannot fail to derive instruction from it, whatever may be the result upon his mind as to the above given reasons for introducing it to his notice. Some explanation is due from our medical shepherds, as to the wide difference between committing our poor bodies to the one or the other class of professors !

TO CORRESPONDENTS.

Our correspondent W. Bush (see p. 16) is not sufficiently explicit to enable any one, without repeating his experiments, to account for the formation of muriatic acid. Previous to putting the sulphur in contact with the chlorine, did he test the chlorine for muriatic acid ? Was the chlorine gas in contact with water at the same time as with the sulphur ? How long did he keep the chlorine and sulphur together, ere he perceived the presence of muriatic acid ? When our correspondent answers these questions, we feel confident that we shall not only be able to account for the phenomenon, but, what will be more advantageous to him, show how it is to be avoided.

The subject on which R. O. F. writes has already occupied enough of our columns ; we therefore decline inserting his communication, with thanks. We can insert nothing on that subject which is not new.

A Country Chemist, Plymouth, is thanked for his suggestion.

A Subscriber, Kimbolton, will find a table of French weights and measures in No. XII. of this Journal.

A Correspondent, who writes from Wadebridge, is informed that we are not in the habit of doing what he requires gratuitously. We would have written to him per post, but that his signature could not be deciphered. As to his second question, relative to the Pharmaceutical Society, we think they must do so.

A Correspondent, Elgin, may, we believe, obtain what he wants at Mr. Palmer's, Philosophical Instrument Maker, 103, Newgate Street, London.

A Norfolk Druggist is thanked for his communication.

Harleston. The fact mentioned by our correspondent is not new.

We fear that "A Young Chemist, Lancaster," must bear with the evil in question.

"A Bookbinder" and "A Retail Druggist" ask questions which do not come within our province.

Mr. W. Gibson.—The Pocket Formulary, published by Sherwood and Co., Paternoster Row, London.

Nota Bene.—*All Communications and Books for Review must be addressed, "To the Editors of The Chemist, care of Mr. Hastings, Publisher, 13, Carey Street, Lincoln's Inn Fields, London." Communications must be prepaid, and sent before the 15th of each Month ; Books for Review, before the 10th.*

THE CHEMIST.

I. CHEMISTRY.

ON THE COMBINATIONS OF LEAD.*

BY M. J. PÉLOUZE.

Oxamide and allantoin, in acting on the elements of water, by the intermedium of the alkalis, are both converted into oxalic acid and ammonia. According to Dumas, to whom its discovery is due, the first of these substances is formed of two compound radicals, which are, on one hand, oxide of carbon $C^2 O^2$, and on the other, amidogen $N^2 H^4$.

Liebig and Wöhler, who made us acquainted with the artificial production of allantoin, have not pointed out what appears to them to be its most probable formula; but it is clear that the composition $C^4 H^6 N^4 O^3$ does not allow it to be supposed that it contains oxide of carbon and amidogen, which would pre-exist in it as in oxamide.

This observation not agreeing with the ideas generally adopted by chemists, concerning the constitution of the amides, and particularly of oxamide, I undertook some experiments on this substance, chiefly with the view of combining it with metallic oxides, and making it lose water, as Liebig and Wöhler did in regard to allantoin. If, indeed, oxamide had experienced any loss of water in its union with bases, this dishydratation would have been sufficient to show that it contains neither oxide of carbon nor amidogen, since, as we know, in the free state, this compound is allowed to be represented by these two binary compounds. I therefore tried to combine oxamide with oxide of silver and oxide of lead, but I could not succeed. In this respect, the question of the amides did not receive any new elucidation; but, in pursuing these experiments, I arrived at some new results, which form the principal subject of this memoir.

A boiling solution of oxamide is not altered by the nitrate or acetate of lead; but if to either of these salts a little ammonia be added, there precipitates an abundance of small white shining laminæ, soft to the touch, which are formed of 90·5 oxide of lead, and 9·5 anhydrous oxalic acid. It is a new degree of saturation of oxalic acid, a tribasic oxalate of lead = 3 Pb O, $C^2 O^3$, in which the oxygen of the base and that of the acid are in equal quantity, and which corresponds with oxalic acid crystallised in water.

The decomposition of oxamide into oxalic acid and ammonia, no doubt assisted by the insolubility of the new salts, is much more prompt here than with the alkalis and the dilute acids.

The tribasic oxalate of lead is also formed by pouring oxalate of ammonia into a solution of tribasic acetate of lead; but in this case it is presented under the form of an amorphous powder, without lustre.

The salt, in whichever way prepared, absorbs carbonic acid from the air, and is converted into a mixture of carbonate and neutral oxalate of lead.

Acetic acid readily removes from it its excess of base.

Nitrate of lead acts in the same way; when boiled with this salt, it is rapidly converted into the neutral oxalate, while it in its turn becomes basic.

When, instead of acting with oxamide on ammoniacal nitrate of lead, in presence of a large quantity of water, concentrated liquors are operated on, there are deposited, even during ebullition, agglomerated, shining crystals, which, collected on a filter, washed in cold water, and dried *in vacuo*, are formed of tribasic oxalate of lead, combined with the neutral nitrate. The formula of this salt is,

$$3\,Pb\,O,\ C^2 O^3 + 3\,Pb\,O,\ N^2 O^5\ H^2\ O.$$

These same crystals, kept in the boiling liquor in which they were formed, are gradually changed, when the latter contains neutral nitrate of lead, into another double

* *Comptes Rendus*, No. 23, Dec. 6, 1841.

salt, composed of oxalate of nitrate of lead, whose formula is,

$$Pb\,O,\ C^2\,O^3,\ Pb\,O,\ N^2\,O^5,\ {}_2H^2\,O.$$

It was to be presumed that the double salts of which I have spoken, were directly formed by the contact of the neutral and tribasic oxalates of lead with the nitrate, under the condition of operating in sufficiently concentrated liquors. It is this, indeed, which takes place.

Tribasic oxalate of lead, introduced into a boiling solution, formed of one part of nitrate of lead and two of water, is changed into white shining crystals, which are nothing more than the first double salt formed by the combination of the tribasic oxalate with the neutral nitrate of lead.

These crystals are destroyed by longer boiling, and are converted into neutral oxalo-nitrate $=$ $Pb\,O\,C^2\,O^3 + Pb\,O\,N^2\,O^5,\ {}_2H^2\,O$.

The boiling filtered supernatant liquor allows to deposit, by cooling, fine crystals of monohydrated bibasic nitrate of lead.

However, the most simple means of preparing the last of these double combinations, consists in making one of the two salts of which it is composed, act directly on the other.

Neutral oxalo-nitrate of lead crystallises in very brilliant hexagonal laminæ, insoluble in cold water, slowly decomposed by it into nitrate of lead, which is dissolved, and into insoluble neutral oxalate. Boiling water destroys this salt with the greatest rapidity. Submitted to the action of heat, it loses its water, and very soon allows reddish vapors mixed with carbonic acid to be disengaged.

MONOHYDRATED BIBASIC NITRATE OF LEAD.

Bibasic nitrate of lead has long been known, but only in the anhydrous state. M. Péligot showed that it might be obtained, combined with one atom of water.

This salt is not very soluble in cold water, and much more so in boiling water, from which it crystallises with facility on cooling. Carbonic acid decomposes it, bringing it to the neutral state.

Dried *in vacuo* and raised to 100° C., it does not lose the smallest quantity of water. It is only at a much higher temperature (between 160° and 190° C.) that it loses its water of crystallisation; this dishydratation takes place very slowly. Towards 200° C., the salt turns yellow and disengages reddish vapors. At a red heat, it is completely decomposed, and yields $\frac{78}{100}$ of its weight of oxide of lead. It gave by analysis 3·1 of water and 19 of nitric acid. Its formula is,

$$2\,Pb\,O,\ N^2\,O^5,\ H^2\,O.$$

This salt is formed under many circumstances, but none is more worthy of attention than that of which I am about to speak.

When a mixture of ceruse, nitrate of lead and water is gently heated, the whole mass very soon becomes agitated by a tumultuous motion. Carbonic acid is disengaged in so great abundance and with such rapidity, that one might almost think there was free nitric acid in the liquor. The latter, filtered while hot, allows to deposit, on cooling, a considerable quantity of monohydrated bibasic nitrate of lead.

The basicity of the salt never descends below this term, however great may be the excess of ceruse. I assured myself of this by the examination of the salt, and by the want of action of the bibasic nitrate of lead on the carbonate of lead.

Placed in the same circumstances as the nitrate of lead, the neutral acetate produces nothing similar; it does not disengage carbonic acid from the ceruse, and remains intact.

Tribasic oxalate of lead, boiled with a large quantity of water, and with an excess of nitrate of lead, is converted into the neutral oxalate, and the neutral nitrate in its turn becomes the hydrated bibasic nitrate.

Again, when a solution of acetate of lead is boiled with an excess of neutral oxalate, the filtered liquor is not rendered turbid by carbonic acid,—a circumstance which shows that the former salt has yielded half of its base to the latter.

SUBOXIDE OF LEAD.

M. Dulong has stated, that in decomposing oxalate of lead by heat, a black, amorphous powder was obtained, which he looked upon as a new oxide, containing less oxygen than the protoxide, or litharge. M. Boussingault repeated M. Dulong's experiments, extended them, and arrived at the same conclusions.

Most chemists, inclining, however, towards his opinion, regarded the question as being not quite resolved. Some of them, and particularly Winkelblek, even contradicted the existence of a suboxide of lead, and regarded it as an atomic mixture of lead and protoxide, or even as a variable, though intimate, mixture of those two bodies.

In investigating the cause of this diversity of opinions, I thought it might be attributed to the very diversity of the products which oxalate of lead furnishes, supposing its decomposition by heat were effected at unequal temperatures. The results which I obtained have confirmed me in that view.

I therefore applied to the decomposition of oxalate of lead, the same rules which I made known in 1833, with respect to the

pyrogenous products of tannin, and the result has been as I expected.

Oxalate of lead, heated in a retort placed in an oil-bath, gave signs of decomposition at a temperature approaching 300° C. The temperature was kept as stationary as possible.

Gases were very slowly disengaged. They consisted of carbonic acid and oxide of carbon. They were collected and analysed during the operation, which occupied a very long time. They constantly presented the exact proportion of 75 to 25, or 3 to 1. Towards the end only, when, to finish the operation, it became necessary to slightly elevate the temperature, the quantity of carbonic acid increased a little.

This proportion of 3 to 1 between the carbonic acid gas and the oxide of carbon is that which theory indicates, admitting that the fixed product of the retort is a suboxide whose formula is Pb^2O, or a mixture of equal quantities of lead and oxide of lead.

Indeed, 2 ($PbO\ C^2O^3$) = Pb^2O, or $Pb + PbO + C^4O^7$, which are reduced into C^3O^6 = 6 volumes of carbonic acid, and CO = 2 volumes of oxide of carbon.

If the retort which contains the oxalate be directly heated on coals or over the flame of a spirit-lamp, as MM. Dulong, Boussingault, and Winkelblek did, the temperature can no longer be governed, and the gases perpetually vary in their proportion, which indicates a complicated decomposition.

The suboxide of lead, prepared as I have said, and after having taken all possible precautions to avoid contact with the air, is a perfectly definite compound. It is of a deep black. It does not contain metallic lead, for, whether dry or in the presence of water, mercury does not remove from it the smallest quantity of that metal, and there never is any amalgamation except with a product prepared at too high a temperature.

It does not contain a protoxide of lead, as I proved in a decisive and very simple manner, by boiling it, out of contact with the air, with a solution of cane sugar, which readily dissolves litharge, and which absolutely takes nothing from suboxide of lead.

Nitric, sulphuric, hydrochloric and acetic acids, dilute or concentrated, do not form peculiar salts with suboxide of lead; they convert it into very finely divided metallic lead, and into the ordinary oxide, with which they combine.

The soluble alkalis act in the same manner. Nitrite of lead itself changes it into protoxide of lead and metallic lead. It disappears in a dilute solution of that salt, and the boiling filtered liquor allows a mixture of nitrate and basic nitrate of lead to deposit.

Mixed with a small quantity of water, in contact with the air, the suboxide of lead acts in a remarkable manner, and of which there is no rational explanation, except by admitting that it constitutes a definite compound.

It then produces much heat, rapidly absorbs the atmospheric oxygen, and is converted into a white powder, which is the ordinary hydrated oxide. A mixture of litharge in fine powder, and finely divided lead, produces nothing similar.

Heated to dull redness, the suboxide of lead is decomposed into a mixture of lead snd protoxide. This decomposition is readily recognized either by amalgamation, or by boiling in solution of sugar, or dilute acetic acid, which leaves a residue of lead, which, instead of being in the state of a very finely divided powder, as with the suboxide, is presented under the form of a mass, which requires only to be pressed between the fingers to be changed into a compact mass with a metallic aspect.

The mixture is also immediately distinguished from the suboxide by its color being of a greenish yellow.

Tribasic oxalate of lead is decomposed by heat, like the neutral oxalate; but the gases vary in their proportions throughout the operation, and the residue is a mixture of suboxide and protoxide. I assured myself of this with a boiling solution of sugar, which dissolved much protoxide of lead.

The analysis of the suboxide of lead did not require to be made after the examination of the proportion of the gases arising from the oxalate.

This proportion sufficiently indicates, for the suboxide, the formula Pb^2O, which likewise confirms the properties of this compound.

100 parts of suboxide of lead, heated in contact with the air, gave 103·7 and 103·6 of protoxide.

This oxidation is effected with great facility, for the suboxide of lead is pyrophoric, and when it is heated in a weight of its own mass, the latter takes fire.

The now indisputable existence of suboxide of lead is of importance to the general history of the combinations of oxygen with the metals. This suboxide, doubtless, will not long remain the only one, but will be placed at the head of a series of compounds which will present, with the highest degrees of oxidation of metals, this very singular point of resemblance, that neither of them can be combined with the acids.

Oxalate of zinc gives, by dry distillation,

ordinary oxide of zinc, and equal volumes of carbonic acid gas, and oxide of carbon.

Oxalate of copper is decomposed with the greatest facility: it allows almost pure carbonic acid to be disengaged, and gives a residue of metallic copper, in red, shining, malleable bracteæ, some of which are several millimètres in size, even though they arise from a pulverulent and amorphous matter which has not undergone fusion.

ON THE THEORY OF THE FORMATION OF CARBONATE OF LEAD.

Everybody knows the process of manufacturing white lead proposed by M. Thénard, and first adopted by M. Roard, in his manufactory at Clichy. This process, which is known by the name of the *French process*, to distinguish it from that first employed in Holland, consists in passing carbonic acid into a solution of tribasic acetate of lead. This salt gives up to the carbonic acid two thirds of its base, which is deposited in the state of ceruse; and, having thus become neutral, it may be again used for the same operation, after having directly combined it with oxide of lead. It is conceived that a large quantity of white lead might thus be produced by a comparatively small proportion of acetate of lead, and consequently of acetic acid. There would be no limit to the production of white lead with the same vinegar, if this salt did not retain a small quantity of acetate of lead.

A modification of M. Thénard's process has been made in England, which has, if I may so express myself, converted it into a process *by the dry way*. It consists in mixing with litharge about one hundredth part of its weight of acetate of lead, and in passing carbonic acid over this mixture, previously moistened with a very small quantity of water. In a few hours all the litharge is converted into a carbonate, and the operation is completed.

Carbonic acid and oxide of lead alone unite only very slowly. It must therefore be admitted that the few thousandths of acetic which are found in the preceding mixture, are carried over the entire mass of oxide of lead, to constitute a basic acetate which is perpetually being destroyed and again formed.

The Dutch process, which was carried, some years since, to Lille, where it became the subject of a very important manufacture, consists in exposing sheets of lead to the vapor of vinegar, and to the exhalations from horse-dung. The vinegar used is beer vinegar of an inferior quality, containing a very small quantity of real acetic acid. From the examination which I made of this vinegar, and from the numbers for which I am indebted to the kindness of MM. Lefevre and Decoster, white lead manufacturers at Lille, the weight of the real acetic acid does not amount to a hundredth and a half of the weight of the lead; and it is known that in good operations almost all the lead is converted into white lead. Mr. Graham arrived, in England, at similar results; he even found less acetic acid than I did, relatively to the weight of the lead.

It is therefore impossible that the carbonic acid of white lead can arise from the decomposition of the vinegar.

Manufacturers, on the other hand, well know that white lead is not obtained unless currents of air are established in the above-mentioned mixtures.

The theory of this fabrication is therefore very simple, and similar to that of the others of which I first spoke.

The air furnishes the oxygen, and the vinegar, in being vaporised under the influence of the considerable heat produced by the fermentation of the dunghill, combines with the oxide of lead, from which it is very soon displaced by the carbonic acid disengaged in abundance from the dunghill. In Dutch white lead, which has not been washed, a large quantity of acetic acid is found.

I think that such is the mode of action; and for the ten years since I left Lille, where I was enabled to study this manufacture, I have always looked upon this theory as being most rational. At that time, almost all chemists thought that carbonic [acetic?] acid contributed by its elements to the formation of white lead.

I made an experiment which perfectly demonstrates the part acted by vinegar in the formation of ceruse. I composed an artificial atmosphere of oxygen and carbonic acid, and I left in that atmosphere a sheet of lead placed over a vessel containing vinegar. At the end of three months, the sheet of lead was covered with a white crust of ceruse. The proportion of the latter was such as to show that the oxygen and carbonic acid were absorbed. The vinegar was found almost undiminished. The quantity which had served for determining the formation of the white lead, was so small that it could not be estimated.

Another very curious experiment shows, in my opinion, the part really acted by acetic acid in the formation of white lead, and the necessity of using in this operation an acid capable of producing with oxide of lead a subsalt, decomposable by carbonic acid. If, in the preceding experiment, formic acid be substituted for vinegar, which does not produce a basic salt with oxide of lead, white lead is not formed, even after a contact of

several years between the vapors of formic acid, metallic lead, oxygen and carbonic acid. Formic acid very much resembles acetic acid in its affinities, and is almost as volatile, but it does not form a basic salt with oxide of lead, and the neutral formiate of that metal is not decomposed by carbonic acid; for that reason it is not applicable to the formation of white lead.

ON THE TENDENCY OF ELECTRICITY TO PROMOTE THE GROWTH OF PLANTS; AND SOME OF THE CAUSES OF ITS ACCUMULATION IN THE ATMOSPHERE DURING THE WINTER AND SPRING SEASONS.

IN A LETTER ADDRESSED TO THE SECRETARY OF THE ELECTRICAL SOCIETY, BY THOMAS PINE, ESQ.

Sir,—In confirmation of the statements which I have submitted to the Society,* on the electricity of the atmosphere, and its relations to vegetation, I would add, here, a few facts and experiments.

If the ball of a thermometer be electrified positively on the exterior, (moistening it for an outside coating, while the interior coating is supplied by the mercury,) a stream of light will shoot through the vacuum to the summit of the tube; but the column of mercury will remain *stationary*. I know not whether there be any novelty in this simple experiment, but it appears to me beautifully illustrative of several distinguished properties of the electric fluid; for instance, that its particles are mutually repulsive; a spark applied to the outside of the tube instantly expelling a corresponding portion from the inside, with a force sufficient to cause it to shoot through a vacuum of several inches: that the fluid is *essentially luminous*, since it is so in the absence of air; that it does not *necessarily* produce *heat*, since no *variation* appears in the scale of the thermometer by this experiment; and, consequently, that a vacuum allows the fluid to flow through it with perfect freedom, imposing *no* resistance to its movements; since any resistance to it is, I conceive, uniformly accompanied with the production of a corresponding degree of *heat*; and lastly, that glass, and probably every gravitating body, contains certain specific portions of the electric fluid. I am not aware of any incorrectness in these inferences, which suggested themselves on my presenting the positive ball of a charged jar to the globe of the thermometer, with the view of ascertaining whether the charge would produce any effect upon its temperature; but the mercury appeared simply to act as a conductor, showing no change whatever in the scale. The facts, I conceive, coincided entirely with the simplest views of the electric principle; or that its phenomena result from disturbing the equable diffusion of a most subtle fluid through all substances. How far its *essential luminosity* tends to identify it with solar light, in conjunction with the electrical effects which that fluid, whether by its direct influences, or by its combination with air and vapors, produce upon vegetation, are offered for the consideration of the Society.

On the 28th of April, I sowed mustard-seed in similar soils contained in Leyden jars, respectively electrified *positively* and *negatively*. In four days the plants issued from the soil, in both jars, but those in the *negative* jar were the most advanced;—plants under ordinary circumstances did not appear till about two days later. On the 12th day of May, the plants in the negative jar had grown to $2\frac{3}{4}$ inches; those in the positive jar to $2\frac{1}{4}$ inches; those in the ordinary state to $1\frac{1}{8}$ inches; the electrified plants were in every respect strong and flourishing, in proportion to their superior height. These experiments are confirmed by those, on a more extensive scale, of Dr. Carmory, who observed generally, "that while plants grow better in soils *negatively* than *positively* electrified, the influence of *any* electricity is more favorable to vegetation than its absence."

In a very obliging letter, Mr. Weekes, of Sandwich, to whose co-operation, through every stage of my inquiries, I am greatly indebted, states, that "various seeds—coriander, carraway, mustard, fennel, &c. &c., sown in a limited portion of soil, kept moistened, and *insulated*, do most unequivocally germinate much sooner, and thrive with much greater rapidity, when supplied with a continuous stream of artificially excited electricity, produced by the agency of a galvanic apparatus." "These experiments," he adds, "I have had constantly progressing in various stages, during many weeks of the last summer, (1834,) and I assure you, with the same uniform result." I would observe, that my jars were *uninsulated* for the purpose of allowing a slight current of the fluid to pass through the plants: the jars underwent a gradual discharge, the charge being frequently renewed; and my inference was, that while a current of electricity in *either* direction is favorable to the growth of plants, that which promotes their *natural* tendency of imbibing it from the atmosphere, by increasing the ordinary relatively *negative* state of the soil, is the more efficacious.

* *Vide* Proceed. Elec. Soc. pp. 2, 6, and 106.

A medical electrician residing in Maidstone, whose powerful machine was in frequent action during the day, assured me of the following facts:—A narcissus plant, which, from previous neglect, was in a languishing state, having been placed in his laboratory, soon began to exhibit signs of extraordinary vigor; it grew to the height of thirty-six inches, or about *thrice* its ordinary dimensions; flowers retained their colors while the seeds were forming, and at last dropped off without assuming a withered appearance; a turk'scap lily drooped during the hours of the night, and resumed its vigor with the returning action of the machine.

These statements may appear to partake of the marvellous, particularly that of the narcissus; but it may be conceived that the superior temperature of the room, with the peculiar attention which appears to have been paid to its cultivation, gave a result similar to the vegetation in latitudes nearer the equator, where the quantities of electric fluid contained in the atmosphere are both greater and more uniform than in our own; and the growth and luxuriance of plants vastly exceeding those of our climate.*

Have we not reason to conclude that, in proportion as means can be adopted for increasing the quantity of the electricity of the atmosphere relatively to that of the earth, the growth of plants may be promoted; and that in this view the electrical windmills, proposed by Mr. Williams, may be deserving of public attention?†

The following experiment, to which my attention was called by the vigilant friendship of Mr. Weekes, affords a direct illustration of the electro-vegetative influence of the atmosphere in the torrid regions. For the double purpose of ascertaining the power of spines in modifying the electric relation of the atmosphere and the earth, and in affecting the progress of vegetation by their electric influence, Mr. Asher insulated a sextuple spine of the *gladitzia triacanthos* at the top of his house, and brought a wire from it to an insulated flower-pot, in which he planted five grains of maize; a similar sowing was made in an uninsulated pot, for the purpose of comparison. The experiment continued from the 6th to the 20th of June, including two stormy days. The electrometer gave considerable signs of electricity in the flower-pot, and by using the condenser, sparks were produced. The electrified grains were found to pass more rapidly through the first periods of vegetation. When Bengal rose-trees were subjected to the same experiment, the flowers of the electrified plant appeared more rapidly and more abundantly than in the other case. Is it not probable that, if the pot had not been insulated, the electric current would have been drawn down to the plants by the attraction of the spines with superior energy, from its free communication with the negative state of the earth; as the electroscope, appended to the inside of a charged jar, shows much more signs of electricity when the outside is in communication with the earth, than when it is placed on an insulating substance? Hence, in general, it is that plants grow and flourish in the ordinary course of nature, under the constant influence of an electric current between the atmosphere and the earth, continually acting through the capillary pores upon the sap and juices of the plants, imperceptibly to our senses, but evincing its presence by the rise and diffusion of those liquids through the vegetable fabric: whereas, were an insulator placed between the body of the earth and the plants, accumulations of electric matter might be traced within and around them; but the vegetative process would be much retarded, in consequence of the stoppage imposed on the electric currents.

* The following account is given by Bishop Heber, of the state of the weather in India, and the effects of storms in that climate:—June 10th, 1824. During the greater part of last month the weather was intensely hot, though a temporary relief was afforded by a few north-westers, accompanied by heavy showers, thunder and lightning. These storms were some of them very awful at the time; but, as they increased in frequency, their fury abated. The change which these storms produced, both on the animal and vegetable creation, is great. The grass and trees, which always, indeed, retain a verdure far beyond what I had expected, have assumed a richer luxuriance. *A fresh crop of flowers has appeared on the trees and shrubs;* the mangoes and other fruit-trees have increased to *treble* and *quadruple* the bulk which the first specimens exhibited; the starved cattle are seen every where devouring the young grass, which, *young as it is, is already up to their knees.*"—Journey in India, Vol. I. p. 104.

† In re-perusing the last chapter of Mr. Williams's "Climate of Great Britain," I am much struck with the admirable manner in which he treats of atmospherical electricity, and of practical modes of operating upon clouds and vapors by means of electricity, either in dispersing or accumulating them on seasonable occasions. The work throughout presents very enlightened views on electro-vegetation.

A powerful electrical machine, or system of machines, as suggested by Mr. Williams, ("Climate of Great Britain," pp. 348—351,) acting constantly in drawing up the fluid from the soil, and dispersing it through the atmosphere within a given space, must operate considerably to increase their opposite electricities; and plants within that space would be seen to "pass through the first periods of negation," in which *aërial* electricity is more immediately concerned, with more rapidity, and to acquire a superior growth and luxuriance, than in their present ordinary state. Exotic plants from warm climates would, in all probability, be much improved by an increased quantity of electric matter in the contiguous atmosphere.

Perhaps the peculiar preparatiou which is made in countries, proportioned to their remoteness from the equator, for causing the germination of plants, requires to be somewhat more particularly remarked. In the torrid zone there is little variation in the seasons and the state of vegetation,—the budding, the leafing, and the maturing processes go nearly hand in hand; but in our northern latitude, the autumnal decline and wintry repose of nature, approaching to deadness, require special arrangements for giving a commencement to germination; and the same necessity must exist in yet greater degrees in higher latitudes. The effects of the fall of the leaves will appear the more important when we consider the immense quantities of moisture which they transfer into the atmosphere in combination with the solar rays. On this subject Mr. Weekes writes to me as follows:—"During my long course of experiments, a curious fact presented itself frequently to my notice, and I was most forcibly struck with what I consider the astonishing quantity of water thrown off by the perspiration of the leaves of plants, and which was necessarily accumulated in a reservoir of my apparatus."*

I have made mention of this fact in my first letter; the transpiration, as I have observed, appears to be produced by the action of the rays of light upon the pores of the leaf, and to be affected by their union with the aqueous element in the form of gas, as in ordinary evaporation from the sea, or any purely watery surface. I find a fresh leaf on the tree, as the vine or apple, &c., causes a much more rapid and copious suffusion of water in a glass inverted upon it, from its under surface, than from a corresponding surface of water, alike open to the solar rays; but if the glass be covered by an opaque substance, or blackened with ink, so as to prevent the immediate action of the rays upon the leaf, no vapor is formed, the rays being prevented from entering into combination with the water. The fall of the leaf, therefore, from the larger and more actively vegetating plants, must be attended with a vast increase of the quantity of the rays in the atmosphere, which, were the leaves remaining in vigor, would combine with water from their pores in the form of vapor; and they are thus left either to combine with the air in maintaining its expansion and its temperation, or to float in it in an uncombined electrical state.

Another cause which must operate to increase the quantities of solar rays in the atmosphere, or rather to prevent so great a diminution as would otherwise arise from their fewness in northern latitudes, during the winter and early spring months, is the sloping direction in which they enter it and spread through its upper regions. The electricity which we find in the atmosphere in cold, dry weather, is probably the principal means of maintaining the vitality of plants at temperatures, which, without this influence, would probably prove fatal to them; and, as in ordinary cases, its phenomena appear to be occasioned by the light diffusion of the fluid over the surface of hodies, of non-conductors especially, without entering their pores; so the solar electricity seems in this manner to be diffused over the upper strata, and gradually to dispense its influence to the lower strata of the aërial regions.

There are, perhaps, no phenomena which have given rise to more conjectures concerning their nature and causes, than the auroras or floating lights of the polar regions. But as they are so nearly imitated by a stream of the electricity floating through an attenuated portion of the air, so the same causes which appear to operate to produce the electricity of our winter and early spring, must exist and operate in proportionably greater degrees at the same seasons in the northern polar regions. They are surely the peculiar provisions of a gracious Providence for storing up some portions of the solar rays, and imparting some glimmerings of light, some electric vitality, and even warmth, to those otherwise dreary and cheerless regions! On my imparting this idea to Mr. Weekes, he expressed himself strongly inclined to the opinion that the

* It is remarkable that plants (as I have repeatedly found in sprigs of mint with the stem inserted in a phial of water, and the leaves in an atmosphere of hydrogen) yield hardly any vapor for the first two or three days in that gas; after which transpiration proceeds in some degree.

rays, as they approach towards a parallel with the atmosphere of the northern pole, instead of penetrating it at nearly right angles, as in the torrid climes, must rest in its higher and more attenuated strata, and produce the phenomena, which occasionally exhibit a portion of their brilliancy in our horizon.

I remain, Sir,
Your obedient Servant,
THOMAS PINE.

Maidstone, Oct. 18th.

DECOMPOSITION OF WATER BY BROMINE.*

BY M. BOURSON.

BY passing steam and the vapor of bromine into a porcelain tube heated to a whitish red, the bromine being in excess, I obtained a large quantity of very pure oxygen, which evidently proves that the water was decomposed, yielding its hydrogen to the bromine. M. Balard, who thought that bromine did not decompose water, operated in a glass tube, and could hardly have obtained a temperature sufficiently high to enable the decomposition to take place; but it is remarkable that, by passing an excess of steam in proportion to the vapor of bromine, I obtained a gaseous, colorless body, having the odor of garlic, insoluble in water and in potassa, and burning with a purple flame like cyanogen. I operated, first, in an earthen tube, and I thought that a little carbon, or some organic matter, contained in the tube, might have produced oxide of carbon; but I repeated the experiment in a very clean porcelain tube, and I obtained the same results as in the earthen one. I have not sufficiently examined this gas to enable me to give more extended information concerning it. I intend to examine it in detail, and I will give the results as soon as my investigations are completed.

INVESTIGATIONS CONCERNING MARGARIC ACID.†

BY F. VARRENTRAPP.

WE find, in Redtenbacher's Memoir, that steäric acid is converted, by distillation, into margaric acid and into secondary products. Varrentrapp has devoted his attention to the distillation of different fatty bodies, such as beef fat, pork fat, olive oil, and crude oleïc acid. He performed all his distillations in glass retorts, on the open fire, in order to be able to regulate the temperature conveniently. At the commencement of the operation he was obliged to heat it rapidly, in order to dissipate the moisture which is mixed with the matter, and which occasions eruptions calculated to break the vessels. As soon as the matter begins to boil regularly, the fire is lowered.

The products of the distillation condense well in the receiver; by employing a Woulfe's apparatus, a hydrocarbon may also be collected, as well as that body whose odor is so disagreeable; at the same time, as the latter is disengaged, carburetted hydrogen and carbonic acid are developed. The product of the distillation of beef fat was almost of the same consistence as the undistilled fat; that which was procured from pork fat had solidified still further; olive oil and oleïc acid gave unctuous products, especially where the distillation was slowly conducted.

The crude product was pressed, in order, as much as possible, to separate the liquid matter. The residue was dissolved in alcohol, in which it was crystallised several times, after which the product was saponified; the filtered and limpid solution of this soap was finally precipitated by hydrochloric acid.

The product thus obtained and submitted to a new distillation was, from whatever fat obtained, perfectly white and crystalline. Its point of fusion was between 55° and 60° C. I again dissolved it in alcohol, and I redistilled it, after having saponified it; the point of fusion was not changed, and I remained convinced that the temperature indicated was really the point of fusion of margaric acid obtained by dry distillation.

Several analyses of acids, prepared with different fatty bodies, gave results which greatly accord with one another; whence it follows, that the acids distilled from the different fatty bodies are identical. I may, therefore, henceforth, dispense with indicating in the following pages, every time, the primitive matter of which I made use.

In order to determine the atomic weight of my acid, I melted a certain weight of it in a small closed tube, with a great excess of oxide of lead. The water thus expelled was ascertained by the loss of weight sustained by the mixture.

Two operations of this nature gave the following results:—

I. 0·970^{gr} lost 0·032^{gr} of water = 3·29 per cent.

II. 1·239^{gr} lost 0·041^{gr} of water = 3·30 per cent.

* *Comptes Rendus*, No. 25, December 20, 1841.

† *Annales de Chimie et de Physique*. Août, 1841.

According to that, the atomic weight of the hydrated acid is equal to 3522—3513.

I prepared, with great care, the salt of soda; the combustion of this salt led me to the number of 3700 for the atomic weight of the acid.

As the salt of silver ought to furnish results of greater accuracy, I precipitated the alcoholic solution of the salt of soda by an alcoholic solution of nitrate of silver. Another portion of salt of silver was prepared by dissolving margaric acid in alcohol charged with ammonia, and by precipitating the ammoniacal soap by an alcoholic solution of nitrate of silver.

The mean of the determinations obtained by the calcination of the margarate of silver prepared by these two means, gives for the atomic weight of the anhydrous acid the number 3340, and for the hydrated acid 3453.

These numbers disagree with those which were obtained by melting the hydrated acid with oxide of lead. The quantity of carbon found in the salt of silver does not agree with that which the free hydrate presents, and which was found to be uniform in several analyses.

The preparation of the salt of lime enabled me to do away with these differences. By treating this salt with alcohol, or, better still, with ether, I extracted from it two substances, one of which was liquid, and the other crystallisable. Both were more carbonised and less oxygenous than the acid, and did not combine with bases.

This circumstance readily explained the excess of carbon found in the free acid: the salt of silver was exempt from it, having been precipitated from an alcoholic solution in which the small quantity of these foreign matters remained dissolved.

In consequence, I saponified the acid obtained by distillation, with an excess of carbonate of soda, and I added to it chloride of calcium until precipitation ceased; the presence of the carbonate found in the latter greatly facilitated the treatment by ether. This vehicle then dissolved the above mentioned carbonaceous matters.

The salt of lime, thus purified, was decomposed by hydrochloric acid; the acid which I obtained melted exactly at 60°.

The formula which I deduced from the analyses of this acid, as well as from those of the margarate of silver, is, for the hydrated acid—

$$C^{34}\ H^{66}\ O^3 + H^2\ O,$$

and for the margarate of silver,

$$C^{34}\ H^{66}\ O^3 + Ag\ O,$$

numbers which agree as nearly as possible with the results found.

I prepared the salt of lead by precipitating an alcoholic solution of the salt of soda by a solution of acetate of lead, acidified by a small quantity of acetic acid; without this precaution a variable quantity of basic salt is always precipitated. Submitted to analysis, this salt gave numbers which agree with the formula.

$$C^{34}\ H^{66}\ O^3 + Pb\ O.$$

The margarate of soda was prepared by means of a caustic and concentrated ley, for that is the best method of obtaining a perfectly neutral combination. It is very soluble in alcohol: dissolved in a large quantity of water, this salt is decomposed and gives an acid salt which is precipitated in the crystalline state, and a basic salt which remains in solution. The analysis of the neutral salt led me to the formula

$$C^{34}\ H^{66}\ O^3 + SO.$$

I prepared the salt of baryta by precipitating a solution of the salt of soda in dilute alcohol by an aqueous solution of chloride of barium. The formula of this salt is represented by

$$C^{44}\ H^{66}\ O^2 + Ba\ O.$$

If the neutral salt of lead be left for several days in digestion with basic acetate of lead, at a gentle heat, the precipitate, light and bulky as it is at first, becomes clotted, and cannot be melted without being decomposed. This new precipitate contains acetate of lead combined with margarate of the same base.

The quantity of acetic acid contained in this salt, although very trifling, may easily be rendered evident, by distilling the salt with sulphuric acid and dilute alcohol. In this manner a liquid is obtained which smells strongly of acetic ether.

The numbers found by analysis lead to a very complicated formula, namely,

$$2\,(C^{34}\ H^{66}\ O^3,\ Pb\ O) + C^4\ H^5\ O^3,\ 6\ Pb\ O.$$

—a composition which agrees very well with experiment.

Finally, I prepared the margarate of oxide of ethule, described by M. Laurent, by saturating with hydrochloric acid gas an alcoholic solution of margaric acid; even before the greater part of the alcohol was saturated with gas, margaric ether separated in the state of a light oil.

As, according to M. Laurent, the fatty ethers are not decomposed by alkalis dissolved in water, I agitated the product with a solution of carbonate of soda, and afterwards, several times, with boiling water, in order to saponify and dissolve the acid which still remained in the ether. By operating in this manner, I could not obtain a product of uniform composition. I succeeded no better by trying to distil it. Indeed, the ether is decomposed below 200° C., and this is then distilled over an alcoholic liquid charged with acid.

In order to analyse it, I used ether which had not been treated by boiling water, to remove the hydrochloric acid. The results which I obtained agree very well with the following formula :—

$$C^{34}\ H^{66}\ O^3 + C^4\ H^{10}\ O.$$

MARGARONE.

I likewise prepared margarone, according to M. Bussy's process, in order to see if its composition agreed with the formula of margaric acid obtained by distillation. For this purpose I distilled a mixture of one part of margaric acid, and one fourth part of fresh burnt lime, until all the products passed over colored. I boiled the margarone thus obtained with an alkaline solution, in order to saponify the acid which passed over at the commencement of the distillation, and I afterwards dissolved it several times in ether, in order to free it from a liquid body which always accompanies it, and dissolves it in great quantity. If a larger proportion of lime than above indicated be employed, nothing but a liquid product is obtained, from which margarone cannot be extracted.

When margarone is distilled, it comes over partly without alteration, and it is partially destroyed. A carbonaceous deposit remains in the retort, and the distilled product is found impregnated with a liquid body, from which it may be freed by repeated crystallisations in ether.

Burnt with oxide of copper, this substance furnished results which agree with the formula

$$C^{33}\ H^{66}\ O.$$

I have not examined the liquid body which is produced in the preparation of margarone, not knowing how to purify it from the latter.

It has been seen that pure margaric acid, submitted to dry distillation, is decomposed in small quantity, giving rise to carbonic acid, and to a crystalline matter, which, as analyses show, is identical with margarone. This product is of a brilliant pearly white, fusible at 76° C., and remains diaphanous after solidification. Purified by several crystallisations in ether, it yielded results which coincide with those afforded by margarone obtained by the distillation of margaric acid with lime.

From all the facts observed, it results that margaric acid, produced by distillation, does not present the same composition as M. Chevreul assigned, according to his analyses, to the acid extracted from human fat.

The conclusions at which I have arrived are likewise supported by the mode of formation of margaric acid, by means of the distillation of steäric acid as explained in Redtenbacher's memoir. (See THE CHEMIST, No. XXV.) The composition which hitherto had been attributed to margaric acid is, therefore, no longer admissible.

In order to acquire more positive proofs, I examined the margaric acid extracted from human fat. For this purpose, I saponified this fat, and, after having removed, by pressing, the greater part of the oleïc acid, I subjected the margaric acid to several crystallisations in alcohol, until its fusing point was 60° C.

Four analyses of this product led me to results identical with those which the analysis of the acid obtained by distillation had furnished.

The examination of the salts of soda and silver confirmed these first results.

In like manner I examined the margaric acid extracted from olive oil, which had been prepared with great care by M. Meyer. This acid presented all the properties of the acids which I prepared myself, whether by the saponification of human fat, or by means of the distillation of steäric acid and other fatty bodies.

The formula of the anhydrous acid is therefore

$$C^{34}\ H^{66}\ O^3,$$

and, consequently, for the crytallised acid,

$$C^{34}\ H^{66}\ O^3 + H^2\ O.$$

If margaric acid be distilled alone, only a small portion is decomposed ; the presence of margaric acid in all the products of the distillation of fatty bodies may, therefore, be thus accounted for. The composition which I have found for the distilled acid perfectly agrees with that of the acid extracted either from human fat or olive oil. The identity of these products can no longer be doubted.

ON THE FAT ACID OF COCOA NUT OIL.*

BY M. C. BROMEIS.

BRANDES was the first who investigated the nature of the fat acid of cocoa-nut oil. In its composition, this acid differs very essentially from the other fat acids. MM. Pélouze and Boudet, on the contrary, regard it as identical with artificially prepared elaïdic acid. The results which I have obtained do not agree with those of MM. Pélouze and Boudet, nor with those of Brandes.

To prepare this acid, I used the crude acid, which is obtained by pressing cocoa-nut oil, and for which I was indebted to the kindness of M. Simon, of Berlin.

This crude acid was first submitted to pressure, which freed it from a small quan-

* *Annales de Chimie et de Physique.* Août, 1841.

tity of oleïc acid which presented the characteristic odor of cocoa-nut oil; after which I twice crystallised it in alcohol.

Finally, by a third crystallisation, and by again saponifying it, I obtained this acid in a state of perfect whiteness, and entirely deprived of smell. Separated from the soap, it did not present a crystalline appearance; it was hard, brittle, and rather transparent at the edges. The acid, crystallised five or six times in alcohol, and exhausted by boiling water, in order to remove the last traces of this product, melted exactly at the temperature of 36° C. M. Brandes found its point of fusion to be between 25° and 27° C.

The analysis of this acid, which I will designate by the name of *cocosteäric acid*, furnished numbers, which lead, for the hydrated acid, to the formula

$$C^{27}\ H^{54}\ O^{4}.$$

By decomposing an alcoholic solution of cocosteärate of soda by a solution of nitrate of silver, a precipitate was obtained, which constitutes the salt of silver. Submitted to analysis, it gave results which agree with the formula

$$C^{27}\ H^{52}\ O^{3},\ Ag\ O;$$

whence it follows that the anhydrous acid should be represented by

$$C^{27}\ H^{52}\ O^{3}.$$

COCOSTEARIC ETHER.

This compound is obtained by passing to saturation a current of hydrochloric acid gas into an alcoholic solution of cocosteäric acid; the ether floats on the top of the liquid. To obtain it pure, it is agitated first in water, then with a dilute solution of carbonate of soda, and, finally, it is thoroughly distilled, or placed over chloride of calcaem. Thus obtained, cocosteäric ether is perfectly limpid, and presents, like the other fat ethers, the odor of renette apples.

Submitted to analysis, this compound gave numbers which lead to the formula

$$C^{31}\ H^{62}\ O^{4},$$

which may be decomposed into

$$C^{27}\ H^{52}\ O^{3},\ C^{4}\ H^{10}\ O.$$

COCOSTEARATE OF SODA.

This salt is prepared by saponifying the pure acid with carbonate of soda, pressing the soap produced; dissolving the latter in absolute alcohol, and concentrating by evaporation. The analysis of this salt agrees with the formula

$$C^{27}\ H^{52}\ O^{3},\ S\ O.$$

M. Brandes endeavoured to determine in the direct way the quantity of water contained in cocosteäric acid: experiment gave him one atom more water than the acid would contain if considered as monobasic. This circumstance led him to look upon two atoms of water contained in the hydrate as being equivalent to one atom of base.

I repeated this experiment, by melting in a straight glass tube dry cocosteäric acid, with an excess of well powdered oxide of lead, until no more water condensed in the upper part of the tube. The results which I obtained, although perfectly agreeing with each other, were at variance with those of M. Brandes. The preceding analyses led directly to the formula

$$C^{27}\ H^{52}\ O^{3} + H^{2}\ O,$$

for the acid of cocoa nut oil.

This acid, unlike the other fat acids, does not seem to be decomposed by distillation. A small quantity which I distilled did not present the smell of carburetted hydrogen; no gas was developed, and I did not observe either the formation of sebacic acid, or any variation in the point of fusion of the acid employed. This character essentially distinguishes cocosteäric from elaïdic acid.

NEW MEANS OF OBTAINING IODIC ACID.*

BY M. BOURSON.

We are acquainted with various processes of preparing iodic acid; but all of them do not succeed very well. Thus, it is first obtained by acting with chlorous acid on iodine: a mixture of nitric and hyponitric acids, and a host of other very tedious and expensive processes are also employed.

Having to prepare this acid, I remarked that, by treating one part of iodine by four parts of nitric acid, as concentrated as possible, that is to say, that which contains only one equivalent of water, and which is called *monohydrated nitric acid*, the color of the iodine very quickly disappeared, aided by a gentle heat, and it did not vaporise at all, as is the case when it is treated by a mixture of nitric and hyponitric acids. The acid obtained was at first in the form of small white agglomerated crystals; by evaporating to dryness these crystals and the nitric acid, in a porcelain capsule, afterwards leaving the dried product in the open air at a temperature of about 15° C., the acid attracted moisture, and acquired a syrupy consistence; then having put it in a place where the temperature was rather higher, and the air more dry, it formed, at the end of a few days, very fine white rhomboïdal crystals.

ON THE DOUBLE IODIDE OF ARSENIC AND MERCURY (IODARSENITE OF MERCURY).†

BY M. E. SOUBEIRAN.

Mr. Donovan has proposed the employ-

* *Comptes Rendus*, No. 24, Dec. 13, 1841.
† *Journal de Pharmacie*, December, 1841.

ment of the combination of the iodides of arsenic and mercury in leprosy and different kinds of psoriasis and lupus, in preference to the mixture of the three elements of which these iodides are composed, and which appear to have, in their union, a favorable influence in the cure of certain obstinate affections. He recommends the employment of this medicine in solution, fearing, and with good reason, if it be administered in the form of pills, the unequal distribution of the matter throughout the mass, the decomposing effect of the excipients, and especially the too direct action on one point of the stomach. Mr. Donovan's formula may be found in THE CHEMIST, Vol. I. page 40.

By representing the matters employed by arsenious acid, protoxide of mercury and iodine, as in Mr. Donovan's Memoir, we have

Arsenious acid . .	0·208
Protoxide of mercury	0·415
Iodine	1·305
	1·928

By representing the components as in the state of iodide of arsenic and iodide of mercury, we have

Iodide of arsenic . .	0·979
Iodide of mercury .	0·649
Iodine in excess . .	0·235
	1·863

The excess of iodine is not sufficient for making the biniodide of mercury: the matters are then represented by

Iodide of arsenic . .	0·979
Iodide of mercury .	0·899
	1·878

Such, indeed, appears to have been Mr. Donovan's intention; at the commencement of his Memoir, he calls to mind the red iodide of mercury, and its property of acting in some respects as an acid, and he gives to the compound the denomination of *iodhydrargyrate*. It must be said, however, that the idea of introducing protiodide of mercury into the preparation better agrees with the proportion of 1 to 2 which he wished to establish, in his formula, between the arsenic and the mercury, each in the state of oxide, namely:—

Arsenious acid . 0·208	Arsen. acid 0·208
Oxide of mercury 0·415	Ox. of mer. 0·431

But it is this that proves that it is biniodide of mercury which enters into the compound.

Having to prepare Mr. Donovan's liquor for the *Hôpital des Vénériens*, I several times repeated this process; there remained each time undissolved arsenic: thinking that this might have been owing to the arsenic not having been sufficiently finely divided, I carefully avoided that cause of error; it might also arise from the volatilisation of the iodine which is inevitable during the trituration; then it was necessary to modify the process; but as at this time I recollected the directions in the Memoir to the effect that the mercury should have been converted into the hydriodate, I suspected that the deposit of arsenic might arise from the biniodide, instead of the protiodide, having entered into combination. In order to assure myself of this, I triturated together 1 gramme of iodide of arsenic, and 65 centigrammes of protiodide of mercury; I moistened them with 100 grammes of heated distilled water, added gradually; there was a deposit of metallic arsenic. I may add, that I previously ascertained that the iodide of arsenic which I used, prepared by Serullas' process, was completely soluble in hot water. When, on the contrary, I made the experiment with 1 gramme of iodide of arsenic and 90 centigrammes of iodide of mercury, all was dissolved. Thus, supposing that it was not Mr. Donovan's intention to make the biniodide, it is that compound alone which makes part of the preparation in question.

It appeared to me much more convenient to appreciate the compounds in the state of iodide, than to base the doses on the quantities of arsenious acid, oxide of mercury and hydriodic acid which represent them. I also considered it necessary to modify the formula so that its two essential elements, the iodides of mercury and arsenic, should be found in it in simple proportions. Instead of the formula,

Iodide of arsenic . .	0·979
Iodide of mercury .	0·899
Distilled water . .	98·122
	100·000

I would employ

Iodide of arsenic . . .	1
Iodide of mercury . .	1
Distilled water . . .	98
	100

After having moistened the two iodides with a little water, I poured on them the boiling water, which dissolved them; I filtered, and I added a sufficient quantity of water to make 100 grammes of liquor.

The liquor then contained exactly 1 per cent. of each iodide. 1 gramme contains one centigramme of each of them, and very nearly 2 milligrammes of arsenic in the metallic state, and 5 milligrammes of mercury in the state of deutoxide.

These are almost exactly the proportions employed by Mr. Donovan.

Donovan's Formula.		Soubeirau's Formula.	
Arsenic .	0·158	Arsenic .	0·165
Mercury .	0·400	Mercury .	0·445
Iodine . .	1·318	Iodine . .	1·489

With regard to Mr. Donovan's process, it results from the above experiments, that he did not insure the complete solution of the arsenic: it is best to resort to the trituration and solution, by aid of heat, of the two iodides separately obtained.

Mr. Donovan has also given a formula for a draught, for which see THE CHEMIST, Vol. I. page 40.

LITHOFELLIC ACID, A NEW PRINCIPLE OF BILIARY CALCULI.*

BY FR. GÖBEL.

DR. GÖBEL has just discovered in a biliary calculus from the Zoological Cabinet of Dorpat, a new body, to which he has given the name of *lithofellic acid*. To obtain it, he dissolved the calculus in boiling alcohol, of 99 per cent., and he slowly evaporated the filtered liquor, which was of a greenish brown color. Crystalline, solid deposits were formed, also of a greenish brown color, which were bruised and agitated several times in cold alcohol. By this means, he succeeded in extracting the coloring matters of the bile, which are more easily dissolved in alcohol, and in obtaining a yellowish powder. Re-dissolved in boiling alcohol, the lithofellic acid was separated, by a slow evaporation, in solid crystalline crusts. The crystals were rhomboidal prisms terminated by an oblique surface. If the crystals which are not quite freed from the coloring matters of the bile be examined with the microscope, there may be perceived between those which are entirely colorless and those which have only a slight yellow color, the coloring matter under the form of small globules.

Lithofellic acid dissolves in 29·4 parts of alcohol of 99 per cent., at 20° C., and in 6·5 parts of boiling alcohol. It requires 444 parts of absolute ether at 20° C., and 47 parts at the boiling temperature.

It melts, at 204° C., into a fluid liquid, of a yellowish color, and forms, on cooling, a solid, colorless and crystalline mass. The point of fusion was determined in a mercury bath.

Heated in a small retort, this acid disengaged, by boiling, white vapors which condensed into a yellowish liquid, and gave a mixture of empyreumatic oil and a little acid. The product of the distillation had a penetrating odor similar to that of oil of amber. There remained in the retort a small quantity of carbonaceous matter. This empyreumatic product appears to contain a new acid; with potassa it forms a soap, which hydrochloric acid decomposes in the aqueous solution, and which leaves the empyreumatic oil on the filter.

When lithofellic acid is heated with a solution of potassa or soda, if the solution be concentrated, the saponification and separation are very soon effected. If, on the contrary, the solution be weak, the soap dissolves in the liquor, and is separated only by concentration. The soap floats at the surface under the form of a fluid mass, while the liquid is warm, and is of a very light yellow color; it is easily separated from it. It forms, after cooling, a solid mass resembling white colophane. It dissolves in ether, alcohol and water, and the acids completely decompose it. Lithofellic acid may then be entirely separated from the aqueous solution, and gives, by desiccation, a soft, white powder, which does not grease paper. One part of soda saturates nearly ten parts of lithofellic acid.

Lithofellic acid dissolves in liquid ammonia, and hydrochloric acid separates it, without alteration, from this solution, under the form of a white powder. If the ammoniacal liquor be evaporated in a sand-bath, there is likewise a decomposition, and lithofellic acid separates in white scales. The soap of soda and lithofellic acid gives, with the salts of silver, mercury, iron, lead, platinum, lime, and baryta, combinations which are little or not at all soluble in water.

The action of nitric acid on lithofellic acid gives rise to a new acid. The liquor evaporated after this reaction leaves a mass of a fine lemon yellow, which completely dissolves in the solution of soda, and separates from the concentrated liquor under the form of soap: the latter readily dissolves in water, and hydrochloric acid isolates from it a brown mass, insoluble in water, which is solidified after cooling, and may then be reduced to powder.

The biliary calculus examined was almost entirely composed of lithofellic acid; it seems to be of a very peculiar nature, entirely unknown to this day.

The new acid analysed by MM. Ettling and Will, gave them, in these experiments, the following composition:—

	I.	II.	III.
Carbon .	71·19	70·80	70·23
Hydrogen	10·85	10·78	10·95
Oxygen .	17·96	18·42	18·82
	100·00	100·00	100·00

They found in two analyses the atomic weight of this acid combined with oxide of silver = 4213 and 4276, or that of the hydrate, at one atom of water = 4327 and 4388.

* *Journal de Pharmacie*, November, 1841.

The formula which best agrees with these results is

$$C^{42}\,H^{76}\,O^{3} = C^{42}\,H^{74}\,O\,7 + Aq.$$

Which gives, in 100 parts:—

42 atoms of Carbon .	3185·86	71·43
76 ,, Hydrogen	474·22	10·63
8 ,, Oxygen .	800·00	17·94
	4460·08	100·00

The formula resulting from Dr. Göbel's analysis would be

$$C^{36}\,H^{66}\,O^{7}.$$

The composition of this acid cannot therefore be regarded as definitively fixed; however, its solubility in alcohol, its easy saponification, the hardness of its crystals, its point of fusion, and the great quantity of oxygen which it contains, appear to the author of its discovery to be sufficiently essential characters to distinguish it from the other fat acids.

ON SULPHUROUS ACID AS A REAGENT.*

BY A. DUFLOS.

Sulphurous acid is employed in analytical chemistry as a means of reducing selenious and tellurous acids. Wöhler has also shown that it converts arsenic acid into arsenious; and that, consequently, in cases where it is necessary, in seeking for arsenic in organic mixtures, to submit arsenious acid to the action of oxidising substances, it may be conveniently used for reducing the arsenic acid formed, in order to facilitate the precipitation of the arsenic in the state of sulphuret.

To these analytical applications must be added another, which is no less worthy of attention. Sulphurous acid is a very valuable auxiliary in the separation and quantitative determination of iodine and copper. It is known that, when a solution of deutoxide of copper is added to a liquor containing in solution iodine combined with metals, the iodine is separated, half in the state of insoluble iodide of copper, and half in that of free iodine, which remains in solution. This solution is prevented by employing with the solution of oxide of copper, another of protoxide of iron; the latter passes to the state of peroxide. This mode of separating iodine is doubtless very good for obtaining iodine in the large way, but not when the quantitative determination of the iodide of certain substances is required, because the iodide of copper thus formed always contains iron, which renders the result inaccurate, without taking into the account that it may often be presented in cases where it is absolutely necessary that it should be avoided. Larphati has already, it is true, endeavoured to remedy this inconvenience, by employing, instead of the mixture of sulphate of copper and iron, a solution of chloride of copper in hydrochloric acid; but, in the first place, this re-agent is not always at hand, and is very alterable; again, it is not unfrequently desirable to determine at the same time the quantity of iodine and of chlorine in the same liquor: it is therefore indispensable to avoid adding chlorine. All these inconveniences are avoided by using a solution of sulphate of copper, in concentrated aqueous sulphurous acid. All the iodine, which exists in the liquors in the state of hydriodate, is then completely precipitated, under the form of iodide of copper quite insoluble in the liquor, which contains free sulphuric acid, whilst, under the same circumstances, chlorine and bromine do not form any insoluble combination. The iodide of copper is collected on a filter of soft blotting-paper, well washed, dried, and finally heated to 120° C., in a bath of chloride of calcium, in a small glass tube closed at one extremity, until there is no further perceptible diminution of weight. Divided by 1.501, the weight of iodide of copper gives as a quotient the corresponding quantity of iodine.

* *Annalen der Chimie und Pharmacie*, Vol. XXIX. page 253.

ON THE FERMENTABLE PROPERTY OF VARIOUS KINDS OF SUGAR.*

BY H. ROSE.

There exists, between the fermentable property of cane and grape sugar, a remarkable difference, which seems to have been hitherto unnoticed; at least it is mentioned in no work on chemistry or technicology. Indeed, the solution of grape sugar requires, to pass into the state of alcoholic fermentation, only a very small quantity of yeast; whilst with that of cane sugar, a very considerable quantity of it is necessary. When equal quantities of cane and grape sugar are dissolved in equal quantities of distilled water, it requires at least eight times more yeast to induce alcoholic fermentation in the solution of cane sugar, than is necessary for producing the same change in that of grape sugar.

Under the influence of a greater quantity of yeast, cane sugar is converted into grape. This matter, therefore, seems to be, of all substances, the only one susceptible of being decomposed by yeast into carbonic acid and alcohol.

If the solution of cane sugar be made to

* *Journal de Pharmacie*, November, 1841.

enter into fermentation, and interrupted, long before it is terminated, by the addition of a large quantity of strong alcohol, it is found that the portion of sugar yet undecomposed by the fermentation, is converted into grape sugar.

The fermentable property of cane sugar, therefore, owes to the same causes as that of starch, of many kinds of gum, and of sugar of milk, the power of passing, under certain circumstances, into alcoholic fermentation. These bodies are first converted into grape sugar under the influence of different substances; but of all vegetable matters susceptible of undergoing this conversion, cane sugar, without contradiction, most readily and promptly submits to it: it likewise determines alcoholic fermentation so easily, that it may be ranked among the fermentable kinds of sugar; but it has no more right to this title than starch, several kinds of gum, and sugar of milk.

MEANS OF OBTAINING SULPHITE OF COPPER IN FINE CRYSTALS.*

BY M. BOURSON.

I SATURATED a concentrated solution of potassa with sulphurous acid, so as to form a bisulphite of that base: I poured this salt into a cold solution of sulphate of copper; only a very slight precipitate of sulphite of copper was formed, which I separated by filtration. I afterwards exposed the liquor to a gentle heat; a large quantity of sulphurous acid was disengaged, and during this disengagement very fine crystals of sulphite of copper of a beautiful deep red color were produced.

In this case, therefore, soluble bisulphite of copper is formed, and is decomposed by heat into the neutral insoluble sulphite.

REVIEWS:

PROCEEDINGS OF THE LONDON ELECTRICAL SOCIETY. Session, 1841-2. Edited by THE SECRETARY. London: Simpkin, Marshall, and Co. Part III.

WE are glad to see this promising work so ably maintaining the high character with which it started. The papers which it contains are valuable; it is very well conducted, and handsomely printed.

The following are some of the more important of the contents of the present part:—Observations on the Electric Effects of the Gymnotus, by Professor Schönbein; Mr. Noad on a Water Battery; Mr. Weekes's Monthly Register of the Electrical, &c. State of the Atmosphere for September, October, and November; The Tendency of Electricity to promote the Growth of Plants, by Mr. Pine (extracted in our present number); Description of an Hydrostatic Galvanometer; Notice of a New Galvanic Battery, by J. A. Van Melsen; with articles by Mr. Walker, Oersted, Matteucci, Tessau, Crahay, &c. &c. &c.

We strongly recommend those who devote attention to the wonderful science of electricity, not only to peruse these proceedings, but to enrol themselves as members of this very excellent Society.

LECTURES ON CHEMISTRY, *including its Applications in the Arts.* By HENRY M. NOAD. London: R. Hastings, Carey Street; and Scott, Webster and Geary, Charterhouse Square. Nos. VI. and VII.

THESE two Numbers fully sustain the character acquired by the former ones, and bear us out in the high opinion which we have at various times given of this work.

II. CHEMICAL MANUFACTURES.

IMPROVEMENTS IN THE PROCESS OF TANNING.†

BY M. VAUQUELIN.

THE following is the substance of a report recently presented to the *Société d'Encouragement*, at Paris, by the Committee of Chemical Arts, on certain improvements in tanning, effected by M. Vauquelin.

The art of tanning constitutes a branch of industry of which every one appreciates the importance, but in which, nevertheless but little progress has been made for some years past. The skins when cleansed are usually thrown into lime-water, which facilitates the depilatory process; and then into larger vessels, where they are submitted to the action of the bark in a diluted and more or less acidulated state. The tanning process may be said to commence here, and it is finished in the tan pits, where the skins are exposed for months, and even for entire years, to the slow action of the tan, which is supplied by the bark of the oak, employed in its natural state, and interlaid, bed by bed, with the skins. It will be seen from

* *Comptes Rendus*, No. 25, Dec. 13, 1841.

† From *The Inventor's Advocate*, No. 164, Jan. 5, 1842.

this that tanning is a very tedious process, requiring the employment of large capital, and exposed to fluctuating chances; for it is difficult to foresee at what price an article will sell when it is necessary to purchase the materials which compose it two years before it is sent to market. Tanning may, therefore, more strictly be termed a commerce than an industry; as it requires on the part of him who pursues it the qualities rather of a *commerçant* than an *industriel*; the expenditure of fuel and motive power, which, in the majority of industries, play so prominent a part, being in this case replaced by the expenditure of time and money.

Among the different specimens of improvements in this art, produced at the last exhibition of the Society, a skin was shown by M. Vauquelin, of which the judges entertained so favourable an opinion that the Society resolved to assist the inventor in the experiments in which he was engaged, and which he would have found some difficulty in following up unaided. Owing to the support thus liberally afforded him, M. Vauquelin has been enabled to bring his experiments to a favourable conclusion, and to operate on a sufficient quantity of skins to afford the Society the necessary data to form a correct estimate of the value of the process. The object which M. Vauquelin had in view was to substitute for the loss of interest in money, occasioned by the tediousness of the process at present in use, an expenditure of power borrowed from some natural and economical principle. The Society will readily believe that its committee would not recommend it to sanction, by its approval, a revolution in recognised theories such as this, without having previously bestowed upon the subject the most careful and scrupulous examination. If the process of M. Vauquelin be brought into use, its value will speedily be tested. Should they be successful, tanneries, like paper manufactories, will be removed from the towns and established near watercourses and forests, so as that advantage may easily be taken of the natural and economical motive principle afforded by the former, and of the plentiful supply of bark furnished by the latter. So long as the tanning process is effected slowly, as at present, it will constitute in itself nothing more than a branch of commerce, where the management of capital will form the principal consideration, and the industrial part of the question will be in some degree lost sight of. Tanneries will continue to be established in the towns, so as to be close to the slaughtering-houses whence the skins are to be had, and they will be monopolised by those who have capital at command. Suppose, on the contrary, that capital should become of minor importance, and that the commercial chances are reduced to something like certainty by the increased rapidity of manufacture, the skins, instead of being the centre of attraction, will be sent to a distance from the towns to the places where economical manufacturing power, and a cheap and abundant supply of bark, are to be had.

The necessity of caution is therefore obvious, where an alteration is a question which threatens to subvert the entire economy of so considerable a branch of industry, and your committee have considered it their duty to submit to the Society such doubts, as a close examination of the subject has suggested to them. In doing this, the committee are, at the same time, bound to convey to the Society the favourable opinion which they entertain of the process of M. Vauquelin. The following is a description of the apparatus of which he makes use:—

1. A species of milling machine, of which the stock becomes displaced by an advancing and retreating movement, which successively draws the skins under the hammers of the machine, and which prevents the movement from reaching the same portion of the skin more than once, which would have the effect of injuring its quality by heating it. The hammers are armed with round knobs, which have the effect of thoroughly manipulating the leather.

2. A large vessel, traversed by an axle, furnished with long spokes, or arms, which, turning on the axle, raise the skins lying in the tub, renew their contact with the liquid, and thus favor the absorption of the reactive power which the liquid possesses. These vessels serve to cleanse the skins, and form a substitute for the stoves usually applied to this purpose; it is also used in the tanning process, the skins being submitted in it to the action of concentrated bark in dilution.

3. A machine, which has some resemblance to a shears, and which is used for stripping from the skin the parts which it may be necessary to remove, which is ordinarily effected by the hand with a currier's knife; this machine is intended to prepare the skin for the operations of the currier.

Without assuming that, in all respects, the processes of M. Vauquelin can lay claim to novelty, your committee, nevertheless, view the *ensemble* with lively interest. They are not ignorant, and it is their duty to state the fact, that M. M. Ogereau and Sterlingue have severally obtained interesting results, as regards the acceleration of the tanning process; they reserve, however, a more detailed notice of their improvements, until these talented gentlemen think proper to

bring them before the public. As regards M. Vauquelin and his process, the following facts may he said to sum up the experiments on which your committee found their opinion:—

1. A skin, presented to the exhibition of 1840, and preserved in a loft, to which the sun and air had free access, after exposure for nearly two years to both, underwent no sensible alteration, and, when dressed, sustained a favorable comparison with new skins.

2. The skins prepared under the inspection of your committee have stood the test of use, and been found equal in all respects to those prepared by the ordinary process.

3. A considerable number of the skins brought from the coast of Africa, which were not in a state to be worked by any tanner according to the ordinary process, have been tanned with the greatest facility by that of M. Vauquelin, to the great astonishment of every tanner in Paris.

"Your committee does not hesitate to pronounce the question relative to the possible introduction of mechanical processes in the art of tanning, as solved by the invention of M. Vauquelin. Considered as scientific experiments, the results obtained by the author are most important, and appear to leave no doubt on his mind as to their being calculated for general application. Leaving aside the question of economy,—for your committee have not such facts before them as would enable them to judge whether the expenditure of power that the new method requires will be above or below the loss of interest which the process at present in use renders unavoidable,—your committee entertain no doubts of its advantages in an industrial point of view, though, regarding it in a commercial light, they are not quite so satisfied as to its results. In the mean time, and notwithstanding the reserve which they have imposed on themselves as regards this invention, your committee are of opinion that the Society should grant to M. Vauquelin the highest recompense in their power to bestow. The zeal and perseverance of this *industriel*, and the sagacity which he has shown in the combination of his various processes, entitle him to this encouragement on the part of the Society, and in readily according it to him, they will probably give birth to a branch of industry which will be fraught with important benefits to France.

PREPARATION OF A LIQUID FOR COMMUNICATING A BEAUTIFUL CRIMSON COLOR TO FOILS OR TINSEL.

BY T. W. NAYLOR.

Foil is manufactured for the use of the jeweller to imitate the precious stones, and for the scene-painter to enrich and heighten the effect of those varieties of design intended to represent submarine grottoes, glittering fairy temples, and the like. It is also attached to theatrical costumes, banners, &c., and occasionally to fancy goods, toys, and pictures. All the processes and recipes I have found described in works relating to the subject of coloring foils, are for the most part ineffective. However well the colors may answer for such purposes as painting window-blinds, &c., it will, on trial, be seen that their partial opacity prevents the polished metallic surface from shining effectively through the coat of color laid thereon. I shall now describe a method of preparing a liquid which, on being applied to silver paper, produces the much admired crimson tint.

Take a small quantity of crimson drop lake, pulverise it, and put it into a bottle, adding about twice or thrice its weight of soda; hold the bottle over a slow fire till the liquid has gradually arrived at boiling; after this it should be kept at that heat for about five minutes, when it may be taken off and allowed to settle for about a day; after which it may be decanted, and the fluid mixed with thin glue or isinglass,* to which a little sugar is added. The sheets of silver paper, or plated copper, intended to be colored, are taken two at a time and placed back to back, in which form they are immersed in a suitable trough filled with the before-mentioned liquid, and then hung up to dry. The trough is not absolutely requisite, for the color can be laid on by means of a common varnish-brush, but the result is not quite so satisfactory, as the colored surface cannot possibly be of so even a tint as if it had been dipped, especially if the sheets be of large size.

BRONZE FOR ELECTROTYPE MEDALS.

To the Editors of The Chemist.

Islington, Jan. 14, 1842.

Gentlemen,—

For the information of such of your readers as are engaged on the Electrotype, I beg to forward you the description of a process which produces a handsome brown bronze at a very cheap rate.

Having thoroughly cleaned and polished the surface of the specimen, with a brush apply the common crocus powder, previously made into a paste with water. When dry, place it in an iron ladle, or on a common fire-shovel, over a clear fire for about one

* No gum arabic must be admitted into the mixture, as it would instantly spoil the color.

minute; and when sufficiently cool, polish with a plate brush.

By this process a bronze similar to that on tea urns is produced; the shade depending upon the duration of the exposure to the fire.

I have tried different qualities of the crocus, but find the commonest to answer the purpose best. I am, Gentlemen,

Respectfully yours,

F. E. L.

BURNING-GLASSES.

To the Editors of The Chemist.

December 20th, 1841.

Gentlemen,—

As burning-glasses are frequently employed by the experimental chemist in investigating the effects of heat on refractory substances, the fusion of metals, &c., I think the knowledge of an easy method of constructing them may be considered desirable.

The proposed plan is to take two circular discs of plate glass, of the requisite dimensions, and place one at each end of a shallow tube; an inch long will be quite sufficient for any size; they are kept in their position very firmly by means of screw clamps, in an analogous manner to the two lenses for showing Newton's concentric coloured rings. To the tube is fitted a short tube with a stop-cock attached: to the end of this tube a condensing syringe is fixed, and the cavity between the glasses filled with turpentine, varnish, bleached oil, or any other suitable substance of a high refractive power. When the glasses have attained the requisite degree of curvature, the stop-cock may be shut, the syringe screwed off, and the fluid lens (for such in reality it is) mounted for use. If you consider this piece of information of any utility, I shall feel obliged by your inserting it in your esteemed work.

I am, Gentlemen,

Your obedient Servant,

Lawrence Brunton.

Gateshead, Durham.

THE INVENTORS' ADVOCATE.

New Series. No. I.

It gives us great satisfaction to observe the improvements made in this excellent periodical, and we feel confident that it will continue to receive that support from the public which it has hitherto so deservedly and extensively enjoyed.

We would call the attention of our readers to a series of articles on the patent laws of foreign countries, which, it is stated, will shortly appear in this journal.

The Inventors' Advocate is published weekly, and contains abstracts of all specifications, &c.

III. PHARMACY, &c.

ON A NEW SYSTEM OF MEDICAL EDUCATION, GOVERNMENT AND PRACTICE.

BY CHARLES WATT.

In fulfilment of our engagement, in a previous number, to present to the readers of The Chemist the remaining portion of our Improved Method of Medical Education, Government and Practice, which has been delayed by the press of matter requiring more immediate attention, we now proceed to lay before them that which relates to the government and practice of the profession upon a plan which affords a total and effectual preventive of the errors and abuses, which now so disgracefully prevail in it, to its utter degradation as a body, and to the incalculable injury of that public for whose good it was intended, and in whose service it is its duty to labor.

To us it has long been a matter of the deepest conviction, and even of demonstration, that the medical profession can never be carried on with honor and dignity to its members, nor with satisfaction to the public, and, with regard to its protection from the dangers with which it is now on all sides surrounded, that it can never be freed from its monopolies, grovelling cupidities, truckling favoritism and partiality—that, in fine, it is hopeless to expect from it any thing but a continuance of empiricism, raving extravagancies, vagaries and delusions, calculated to produce no other impression on minds of any information, than that among its highest members there are many not a whit behind the most crafty and deluded of the witches of the darkest ages—so long as the present institutions hold the reins of medical government. The only remedy for these evils is the constitution of a governing body totally independent of medical prac-

tice, and secure against all the little selfish and low-minded feelings and jealousies which so conspicuously mark the members of this profession who are engaged in the active exercise of it. Such an institution must be founded on a sound, liberal, disinterested and satisfactory method of determining the qualification of candidates, and an open and accessible means of granting degrees and honors, giving powers to practice in any of the branches of Medical Science, from the Physician to the Chemist and Druggist, and of rewarding with suitable marks of distinction those whose talents or labors have been found to deserve them.

The power of a retrospect and experience of forty years affords us too ample an opportunity of judging of the inefficiency, fallacy, and utter uselessness of the Colleges, and that contemptible piece of public delusion, the Apothecaries' Hall, to create in us any desire to see their powers enlarged, or even continued; on the contrary, for the common good, we exclaim, "Down with them, even to the ground."

Of all the institutions which have hitherto existed for the government and regulation of the profession, the College of Physicians is by far the most entitled to respect and confidence, simply because of the superior education of its members in classical learning, and their more refined manners acquired at College; but in all other respects it is a sad specimen of all the abuses and imperfections of the other bodies. Its system of excluding all who are not educated at certain universities,* although their talents and professional acquirements are of the highest order; its gross display of partiality and favoritism to those who have served their terms at Oxford and Cambridge; its exhibition of deep concern for, and interest in, forwarding the views of such as are related to, or in any way patronized by, those employed by the Court, or who are in repute with the higher circles, are as notorious as they are shameful, and demand an immediate and effectual remedy. Let any young physician not allied to some of the select coterie, make any attempt to advance himself by trenching in the slightest degree upon them, and from that moment

* We recollect when the late amiable and celebrated Dr. Walshman, of Kennington, was obliged to leave his practice to the care of Dr. Blegborough, and pass his time for three or four years at Edinburgh, merely to obtain the license of a set of men whose interests it was to obstruct one so far above most of themselves in talents and acquirements, and fast outstripping them in the road to fortune. He returned with the due right to the forms of the College; and after making a large fortune, died at the advanced age of ninety years, respected by all.

In the case of the late Dr. Hector Campbell, a similar piece of *liberality* was shown, and the result was the treatment of the College by him in a manner so foul and disgraceful to himself, and contemptuous to that body, as to preclude its record here: at the same time we must remark the disgust it shows of an educated and gifted individual at a set of men who are vested with the power to ruin and destroy the prospects of one evidently surpassing them in public repute and in practice. There is no remedy but to submit to the same ordeal as that through which Dr. Walshman passed; and this often would be perfect ruin. Would this be the case, if there existed a body having the power to examine, and, upon the candidate being found qualified, to be vested with authority to grant a degree or diploma, as the case may require? Is it the object of the legislature that the candidate should possess the proper knowledge of his profession, or that he should have been educated at some particular College? It is easy to see the intentions it has; and so long as adequate qualification is obtained, those intentions are fulfilled; but on the part of those institutions to which the power of investigation is confided, a system of monopoly and exclusion is kept up, which calls for immediate extinction.

the spirit of intolerance, or the superciliousness of pride, awaits him from those who feel that they occupy the vantage ground; and well skilled in all the petty arts of showing it, and ever as ready to exercise their ingenuity in this respect are they, whenever opportunity occurs.

Until within a very few years, and, indeed, even now, hut very slight chances were or are to be found for a young physician not one of a certain medical circle. For the truth of these observations we have only to refer our readers to the times of the Baillies, the Pepyses, the Crofts, the Denmans, the Heberdens, and a score more, among whom any junior physician had, indeed, but a slender hope of success, if not related to, or the fortunate *protégé* of, one of them. But we object to the College of Physicians having the power to grant qualifications or diplomas, more especially because those who are entrusted with the office, are not independent of practice, and the method of determining the attainments of candidates is partial, imperfect, and erroneous.

The College of Surgeons is even much less worthy of confidence, nay, we might add, is contemptibly venial and inefficient, in all its acts and managements. It is defective in the whole system; exclusive, and ahundant with abuses and jobbing; a lumbering machinery, to produce what? Nothing, we reply, that is worth to the public one hundred per annum.

The examination, so far from heing calculated to ascertain the real amount of knowledge possessed by the candidates, is a piece of mere idle mummery, a perfect farce; but it is at the same time a dangerous one to the puhlic, inasmuch as it prevents the adoption of one which would compel all who are examined to be perfectly qualified.*

The spirit of monopoly, exclusiveness, and favoritism exists in this body in a far higher degree than in that of the physicians. For how many years have the affairs of this body been in the hands of the select few? We may say ever since the time of the immortal Hunter. Who had any chance in the times of the Clines, the Homes, the Blizards, the Coopers, and the rest of the clique, by whose agency a surgeon of the highest talents had no prospect, if destitute of their favors; and these he could little expect to gain, if not a pupil of, or related to, some of them.

Let any one reflect upon the manner in which this College disposes of matters. The examiners and council are chosen from the class of teachers, or, we should say, such as are paid for teaching, and hospital surgeons. Certain hy-laws have been passed, according to which no candidate for the diploma is eligible except he has heen educated at certain colleges or schools, and these are chiefly such as they either lecture or attend at, or both, so that they secure the fees very comfortably, and exclude a qualified man because he has not paid them for teaching him,—if teaching it is worthy to be called,—for wc hold the plan of teaching the sciences in such a mode as perfectly worthless, and we are not able to speak in higher terms of the performance of their duties by the pupils with regard to hospital practice; it is for the most part performed in a manner so slovenly, and the negligence of them so little heeded, as to make the whole a mere piece of show. There are no interrogations to the class, and no investigations of its advance in knowledge, and we are convinced that no method which rejects these can be of any value to students.

That such men should he highly interested in the welfare of the College is quite natural: they not only can take good care of them-

* We have often objected to any public and final examinations, especially upon the present system, first, because they are useless and afford no real test, and secondly, because they are ruinous to rejected candidates.

selves, but provide good berths and places for their sons and other relations for ages to come. They find, no doubt, that charity not only begins at home, but ends there also. How can we hope for any change for the better in the profession, while these men retain the power with which they are now invested. We reply, never: and the profession cannot advance in any conspicuous degree until the government of it is placed in the hands of disinterested persons.

Enough, we think, has been said to satisfy all persons of ordinary capacity, of the inutility and abuses of the College of Surgeons.

We shall occupy but little time with any remarks on the powers and examinations of the Apothecaries' Company.

The examinations present the same kind of errors as those of the College of Surgeons, only, as the more illiterate of the two, these, of course, are exhibited in a far higher degree. They have all the peculiar characteristics of that class which Shakespeare deemed "caitiff wretches," whose office of examiners we hope soon to see extinguished. They work on the same principles and to the same ends as those already stated.

After what has been advanced, it must be evident that it is hopeless to expect any effectual plan of Medical Reform to be adopted, so long as these corporate institutions exist, or have their present power; nor has any remedy been found in the Faculty of Medicine, established about three years back. Although it has power to confer degrees, as coming from such hands, they are by no means an example of the "laudari a laudato viro." It is composed of a most heterogeneous mixture: the votaries of quackery, mesmerism, homœopathy, and every other vagary which can suffice to give suitable temporary notoriety to its founders and disciples. This body is based on the same erroneous principles as all the rest, and has to divide its functions equally between "Stincomalee" and the parson's King's College affair in the Strand; no positive benefit to the public or the profession can arise from the exercise of the functions of this body, as the same system of monopoly, favoritism, and exclusion will still prevail, with the additional advantage of another means of effecting those objects.

We now proceed to submit to our readers the means by which a perfect and effectual remedy for all the evils which we have stated, and which will at the same time be the means of greater advancement and more extensive improvements in all departments of medical science, than any other which can be submitted to, or sanctioned by, the legislature, or be devised by any societies, which, for the most part, are conspicuous for nothing but the "vox et præterea nihil," of which distinction in a very short time the Pharmaceutical Society has reached the very climax.

Now, in the event of the adoption of the system of teaching, and more especially of examining, which we gave in THE CHEMIST,* it is quite evident that none could pass but such as are thoroughly and profoundly qualified in the science, of which they are about to exercise the practice; for if, as we propose, he is compelled to give in writing a perfect set of answers to the questions, which are put, not by oral means, but by printed forms, it is utterly impossible that he could fail to acquire a complete knowledge of the subject.† Hence, then, the whole idle and ridiculous paraphernalia of public examinations, and the squad who

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* No. XXIII.

† A board of commissioners, properly qualified, but not in actual practice, would be able to carry out these plans, and, by adopting the means suggested, of having a person present, effectually prevent the answers being taken from books. This board would be well able to judge of the knowledge of the candidate, as a written evidence must be the most convincing and perfect.

form the "dramatis personæ" in the farce, are at once rendered unnecessary, and their office superseded by a plan against which it is utterly impossible to urge a single objection. It will, also, be equally manifest that all persons, who have given such ample and written evidence of their qualifications, are justly entitled to such license to practice as is consistent with the department which they are destined to follow, and that, whether they have been educated at college, or whether they have been taught at this or that hospital or school, they may justly claim a right to their diploma, and we contend that no set of men have the right to exclude such from any professional privilege. The object of our forefathers, in establishing colleges and schools, was to ensure education; and we have no objection to them; we object only to their usurpation of a right of excluding all who have qualified without their aid, or at their places of instruction.

It must now be evident that in this too brief outline we have most completely provided for the thorough education of the members of every branch of the profession; and this being done, nothing more is required but to give a method of regulating its system of practice; and in doing this, we may ask, is any person not educated and properly qualified to exercise any part of the healing art? Our reply is No: IN NO CASE ought any one to be permitted to administer ANY REMEDY, unless he is FULLY QUALIFIED to judge of the fitness of its exhibition in respect to the state of the patient's case, and is PERFECTLY CONVERSANT with the action of the remedy which he advises; and it is the duty of the Legislature to PREVENT, by HEAVY PENALTIES, so DANGEROUS and UNFAIR a practice, and the great evil, also, which it produces, of preventing persons from falling into more competent hands.

If Chemists and Druggists are to give advice, society demands of the Legislature that they should be compelled to qualify, and that to such an extent as to protect the public from the present system, which we freely admit is too often practised to the greatest injury of the public. We do not exclude the uneducated and unqualified Apothecaries from these remarks.* We trust that, during the ensuing session of Parliament, an efficient measure of medical reform will be passed, and that each portion of the profession will have its distinct offices assigned to it, from which it will depart at its own peril. In such an enactment it ought to be compulsory on Chemists and Druggists to qualify in Chemistry, Botany, Materia Medica, and Pharmacy; the general practitioner, in these, and also in Medicine, Anatomy, Physiology, Surgery, and Midwifery, according to a plan much more strict and profound than the flimsy one now adopted; and the Physician, besides those sciences which he is obliged to study, should be necessitated to obtain a much more efficient knowledge of Chemistry, than he is ordinarily in the habit of acquiring; for we contend, these constraints are as essential on the part of the higher ranks of the profession, as is a suitable course of study on the part of Chemists and Druggists. That the present state of the medical profession is unfit to be any longer tolerated in a civilized community is too plain for any further delay in its amendment. We shall give our best aid in any measure likely to adjust all differences, and give to each his just right, and restrict his encroachment on that of his neighbour, or endangering the health of the public.

In the whole range of human knowledge, there are no subjects more important, or more beautiful, than those which relate to medicine, and its collateral sciences; and those must, indeed, be possessed of base, sordid, and poor minds, who are not desirous of reaching the highest degree of

* Coroners' inquests too often furnish us with melancholy instances of the applicability of these observations, both to Chemists and the lower grade of medical men.

attainment of them; none who are not imbued with a strong inclination for such studies ought to be engaged in the practice. We trust ere long to see none other occupying any department in the profession of medicine; and sincerely do we hope to see all the lower and illiterate, who now in such numbers disgrace it, exterminated from its ranks; and we have the consolation of knowing that the system of education and examination we have proposed would at once work out this much to be desired revolution; and if such success should result from our labors, we shall then, indeed, rejoice, aud rest happy in having conferred on society one of the greatest boons it can receive at our hands.

REVIEWS:—

Elements of Materia Medica and Pharmacy. By O'Bryen Bellingham, M.D., Lecturer on Materia Medica at the Richmond Hospital School of Medicine. Edited by Arthur Mitchell, M.D., Lecturer on Botany at the Richmond Hospital School of Medicine. Part I. Dublin: Fannin and Co.; Longman and Co., and H. Renshaw, London. 1841.

The want of an elementary work on Materia Medica and Pharmacy, which could be safely recommended to the student, has frequently been felt; not that there is any remarkable scarcity of elaborate Treatises on, or of Manuals of, Materia Medica; but a work was wanting, which, in filling up the deficiencies of the majority of compendiums and manuals, should include every thing of importance contained in the more elaborate treatises; should supply the omissions which occur in the latter; and which, without overloading the pages with unnecessary details on the one hand, or distracting attention by speculative or theoretical opinions on the other, should contain every thing really essential connected with these subjects.

This want is well supplied by the book before us: the arrangement as to classification is excellent, and the manner in which the subjects are treated is highly creditable to the eminent physicians to whom the profession is indebted for this work.

From the number of books which we have to notice this month, we cannot give a very lengthy extract from this. We, however, think that the following may prove interesting to our readers, and may serve as an example of the very excellent manner in which information is conveyed by this work:—

PROXIMATE PRINCIPLES OF VEGETABLES.

"Vegetable substances (as has been before observed) may be subjected to two different kinds of analysis: the object of one is, to separate their *proximate* or *organic principles;* and the other, to determine their *elementary composition*, that is, the number and exact proportion of the simple substances of which they are composed.

"*Definition.*—The proximate, or the immediate organic principles of a vegetable substance, are those compounds which exist in it, either in a state of mixture, or slight combination; and can be procured in a separate state, by chemical or other processes; and upon which, in most instances, the medicinal virtues of the vegetable substance depend. They are never simple substances, but are all formed by the union of two, three, or four elementary bodies (seldom more). The ultimate elements of vegetable substances are generally carbon, hydrogen, and oxygen. Animal bodies contain nitrogen in addition; and the following (in general terms) is the mode in which the analysis is effected:—

"*Analysis.*—The constituents of the vegetable principle are oxidised, by which the hydrogen is converted into water, and the carbon into carbonic acid; the nitrogen at the same time assumes the form of gas. The water produced is weighed, and the respective volumes of the resulting carbonic acid and nitrogen gases are measured; the amount of oxygen is best estimated by the loss. The composition of the substance submitted to examination can then be calculated.

"The process generally adopted, is the following:—A known weight of the vegetable principle mixed with black oxide or protoxide of copper (a substance which yields oxygen readily) is exposed to a red heat in a glass tube; its oxygen unites with the carbon of the vegetable substance to form carbonic acid, and with its hydrogen to form water. An apparatus containing known weights of caustic potash, and of chloride of calcium, is connected with the tube; the carbonic acid combines with the former, and the water is condensed by the latter. From the additional weight acquired by these substances, we calculate the quantity of carbonic acid and of water formed, and thus the amount of carbon and hydrogen in the vegetable principle. When nitrogen in addition is present, it assumes the gaseous form, and is collected and measured. The amount of oxygen is calculated by comparing the quantity of it in the carbonic acid and

water, with that lost by the protoxide of copper. If they contain more than has been lost by the oxide, the excess must have existed in the substance under operation. The protoxide of copper adapted to this analysis, is best prepared by dissolving copper in pure nitric acid, evaporating the solution to dryness, and then igniting the salt strongly in a platinum crucible. If the heat has been sufficiently strong, the resulting protoxide contains no traces of nitric acid.

"*Composition.*—By a series of experiments conducted upon these principles, Gay-Lussac and Thénard were enabled to establish certain general views.

"1st. When the oxygen which the vegetable substance contains, is to the hydrogen in a proportion greater than is necessary to form water—in other words, when there is *excess of oxygen*—the body is usually *acid.*

"2nd. When the hydrogen is in larger proportion than suffices to convert all the oxygen into water, or when *hydrogen* is in *excess*, the body is of an *oily*, *resinous*, or *alcoholic* nature.

"3rd. When the oxygen and the hydrogen are in proportion to form water, that is, when there is *no excess* of *either*, the substance is neither acid nor resinous, but *saccharine or mucilaginous.*

"To the correctness of these results, however, there are several exceptions. Those vegetable principles, which contain nitrogen, in addition to carbon, hydrogen and oxygen, as the alkaloïds, are not included.

"The proximate principles of vegetables which consist of but two elementary substances (termed binary compounds) are not numerous. Oil of turpentine, oil of lemons, oil of juniper, and oil of savine, are examples: they consist of carbon and hydrogen. Carbonic and oxalic acids, also binary compounds, consist of carbon and oxygen.

"The proximate principles which consist of three elementary substances (or ternary compounds) are the most numerous; these usually consist of carbon, hydrogen, and oxygen. Under this head are included most vegetable proximate principles, as sugar, gum, starch, lignin, vegetable acids, fixed and volatile oils, &c.

"The proximate principles which are composed of carbon, hydrogen, oxygen, and nitrogen, (termed quaternary compounds,) are usually of animal origin; however, several of the proximate principles of vegetables have also this composition, as the vegetable alkaloïds, &c.

"*Preparation.*—The proximate principles are obtained from the vegetable substances in which they exist, by different methods according to the nature of each particular substance. Some exude spontaneously from the growing vegetables; this is the case with gum: others are obtained by incisions in the root, trunk, or branches; as resins, gum-resins, &c.: and some are procured by mechanical means, as expression, of which the fixed oils are examples. In other cases the proximate principles are volatile, and can be separated by means of a moderate heat. In this way the essential oils, camphor, benzoïc acid, &c., are procured; and in many cases, the separation is effected by the aid of different solvents, the principal of which are boiling and cold water, alcohol, proof spirit, ether, acids, &c.

"Water dissolves many proximate principles, as gum, sugar, salts, coloring matter, &c. Alcohol dissolves others, as resin, essential oil, balsam, camphor, vegetable alkaloïds, &c. A mixture of alcohol and water, or proof spirit, (as it is called,) is frequently also employed as a solvent.

"Ether dissolves pretty nearly the same substances as alcohol; but it is also capable of dissolving some substances which are not soluble in that liquid. The acids, alkalis, volatile or fixed oils, are also often employed, either as solvents or re-agents, in detecting or isolating the proximate principles of vegetable substances.

"*Characters.*—The number of proximate principles which have been described by chemists is very large, and every year additions are being made to them. They are distinguished by the following characters:—

"They are all decomposed by a red heat, and are very prone to spontaneous decomposition, giving rise to the phenomena of fermentation and putrefaction. They cannot be formed artificially by the union of their elements. When heated in the open air, they burn; and the products of their combustion are carbonic acid and water. Exposed to heat, in close vessels, or to destructive distillation, (as it is called,) the products are water, carbonic oxide, carbonic acid, light carburetted hydrogen, and olefiant gases, acetic acid, and empyreumatic oil. When nitrogen, in addition, is a constituent of the proximate principle, ammonia, and cyanogen, with some free nitrogen, and sometimes cyanic acid, and hydrocyanic acid, are also given off.

"*Classification.*—The vegetable proximate principles, which require to be noticed here, may, for convenience sake, be described in classes, as they have been arranged in Thomson's 'Chemistry of Organic Bodies.'

"Class 1. Neutral Vegetable Principles.

"Class 2. Intermediate Bodies.

"Class 3. Vegetable Acids.

"Class 4. Vegetable Alkalies.

"This arrangement is rather intended for

the convenience of the student than to satisfy the theorist, for (as Brande has remarked) the published volume of Liebig's work upon this subject, so far as arrangement is concerned, does not appear at all calculated to aid the beginner; and the same objection applies to the fifth volume of Dumas' Elaborate Treatise. The nomenclature (he adds), which is creeping into organic chemistry, cannot be too strongly protested against by all who are engaged in teaching chemistry. Neither arrangement nor nomenclature are of much more importance to those who have advanced far into, and are familiar with, the more complicated details, &c. of the science; but to the student, the *capricious* and *hypothetical* terms, which are in vogue, are either *unintelligible*, or, what is worse, are calculated to *embarrass* or *mislead*."

This work will be found of the greatest utility to medical men and chemists, to whom we recommend it as being decidedly the best of the kind that has appeared for some time.

MEDICAL GUIDE FOR MOTHERS, *in Pregnancy, Accouchement, Suckling, Weaning, &c. &c., and in some of the more Important Diseases of Infancy. With an Appendix on the Successful Treatment of Consumption, by a New Remedy suggested by Dr. Ulric Palmedo, of Berlin.* By J. R. Hancorn, M. R. C. S. London: Smith and Elder, Cornhill. 1842.

This work is eminently entitled to our praise: all the subjects of which it treats are most ably handled.

The diseases to which females are subject are often of a nature to require as much, if not more, aid from the nurse, as from the physician. On the medical treatment of these diseases much has been written, while far too little attention has been paid to all that does not come quite within the province of the attendant. The care of the fairer portion of mankind is left, in youth, to ignorant mothers, who bring them up in such a manner, as to render them less capable of bearing those ills which every woman must go through; in riper age, imbued with the prejudices and notions inculcated by those parents, they are left to themselves, inexperience being thus superadded to ignorance; and, at those periods when they require most careful attention, namely, during confinement, to the twaddling old gossips, who, from the nature of their vocation, are called nurses,—to whose want of knowledge, many a woman has fallen a victim, and through whom many a sad bereavement has taken place.

By a perusal of this work, every mother will be taught the only rational way of rearing her progeny; every husband, the care which it is necessary that he should bestow upon his wife; every nurse, the proper exercise of the duties of her vocation.

The work is replete with observation; and the author has shown much skill in conveying information in such a manner as not to offend the delicate taste of even the most fastidious female. No maid, or wife, should be without it.

For the benefit of every class of our readers, we give the following extract; and we think that all will concur in our opinion of the correctness of the author's observations.

"GENERAL IDEAS OF WOMAN.

"Woman in the first years of her life, does not appear at first sight to differ from man. She has almost the same air, the same delicacy of organs, the same gait, the same tone of voice; but, as she advances towards puberty, woman seems to become less removed from her original, delicate, and tender constitution. She always preserves something of the temperament peculiar to infancy; the developement which age produces in all parts of her body does not give it the same degree of consistence as they acquire in man.

"Woman arrives at puberty before man, although the time for that revolution may be designated as from the age of thirteen to fourteen for the female, and from fifteen to sixteen for the male; that law is not general: puberty is subject to great variations, being, more or less, precocious, according to the temperature of the climate, and the manners of the inhabitants.

"The influence of puberty is not confined to the physical constitution; it works a manifest revolution in the moral. Woman feels much sooner than man this new influence; she is astonished at being endowed with new attributes; and this change produces modesty: those torments which, in a young man, are so impetuous and so difficult to be restrained, without being so violent in appearance, nevertheless produce effects as dangerous in the heart of the young girl.

"Considered anatomically, the female, besides the sexual and mammary organs, besides the catemenial discharge, is distinguished from man, by a lower stature, a smaller head, a shorter face, a longer neck, the chest prominent in front and compressed at the sides, the hips more extended, and the members shorter; she has the osseous system whiter, less voluminous than that of man; her clavicle is longer, and less curved;

her sternum is more forward, and shorter; her coxal bones touch one another in a smaller number of points; her sacrum shorter, broader, and less curved at the bottom, and thrown more backwards; the trochanters and upper part of the thigh bones more distant from one another: finally, the capacity of the pelvis is more extensive.

"The muscular system is less developed, more supple and weaker than in man. The nervous system predominates over the muscular in woman; it is more susceptible, more moveable. The blood-vessels are more numerous, more flexible, and the blood preserves more the characteristic of that of youth; the laminous tissue is more abundant, less close; its expansion, and the softness of the fat, which renders supple the organs of woman, contribute to the pleasing expression of her physiognomy, to the hemispherical form and marked projection of her bosom, which forms one of her principal ornaments, and of which artists represent the agreeable curves with so many graces; it gives also to those limbs that polished surface, that roundness, and that softness which those of men ought to have; the lymphatic vessels are more numerous, less furnished with valves in woman; the heart is less expanded; the lungs are smaller and more dilatable; finally, the cutaneous covering is of a finer and more delicate texture, and its whiteness and softness impress our eyes agreeably.

"The organs of sense in woman have an activity which depends much on the greater susceptibility of her nervous system; she has an eye more lively and more penetrating, a touch more delicate, a finer hearing and smell, and the taste more exquisite than man.

"Considered morally, woman, endowed with extreme sensibility, is submitted to a multitude of lively but evanescent impressions; her ideas succeed one another rapidly; she easily forgets past pains, and thinks little on evils to come; but, generally, she has a lively and quick imagination, the activity of which is partially derived from that temperament which, having little muscular force, gives the superiority by that means to the nervous system; hence it follows that woman is more capable of imitation than man, that she pays more attention to the physical impressions than to the chain of reasoning; that her imagination is more capable of being drawn away, more ready to be excited, and has also a greater influence over the body, and that she rather abandons herself to the feelings of the heart, than to cold and severe reason; it is physical vigour that renders man superior to these weaknesses; and accordingly the most masculine and robust temperaments are the least pliable, as well physically as morally. Who can feel those ecstacies, those ecstatic transports, if not the nervous woman! All the histories of fanaticism, of extravagance in sentiment, of enthusiasts in animal magnetism, of somnambulism, always show woman in the first rank. It is women who generally act as priestesses, and are addicted to sorcery. We have women who conjure with cards, sibyls, prophetesses, and gypsies, who believe in the truth of their art. They are, therefore, exposed to this superstition and credulity only by the radical weakness of their constitution; hence also women are more subject than men to nervous diseases, and are more affectionate and sensitive. When, indeed, we consider the delicacy of their fibres, the softness of their cellular tissue, and its developement, their pleasing and graceful forms, we must expect from them all the affections of humanity, compassion, tender charity, and conciliation, which sustain society, connect its different members, strengthen family ties, and form the sweetest attribute of maternity. Hence, woman is endowed with a lively sensibility, which renders her eminently fit to interest herself with infancy, which makes her surmount maternal pains by the sweet sentiment of pity, and renders agreeable to her the cares and details of housewifery; for this purpose, nature has given to this sex the desire of being mothers, a desire more powerful than life, and which renders it capable of all sacrifices. By her weakness, the female feels the necessity of loving, of attaching herself, of pleasing; the infant never implores her pity in vain; she braves all sufferings and meets all dangers for her child: many mothers have been known to precipitate themselves through fire and water for their children! all the unfortunate belong to woman; devoted to the oppressed, she shares his feelings, and takes upon her his sorrows; we see her go to death as a victim; and satisfied with those sacrifices, she desires no higher recompense than to be loved.

"Woman, it will be said, is a little too light, and too fickle in her actions; but if she wants that vigour of thought, that connection of reasoning, that abstract meditation, the lot given to her by nature for her share is nevertheless brilliant; all that is graceful, delicate, refined; that rapid and unerring taste, that tact that determines what is suitable, that exquisite sensibility in perception, that keen discovery of the ridiculous, that charming talent of conversation which divines at a glance, penetrates sentiments sought to be concealed, opens and interests the heart,—all this is given

only to woman in the highest degree. Accordingly, anger in woman, the desire of domineering, an air of violence and superiority, even of arrogance, break the bonds through which the strong is conquered by the weak. In this respect, however, much diversity exists, according to the constitution of each woman: she who is of a strong, dark, firm, vigorous, melancholic temperament, will exhibit more obstinacy, less inconstancy and levity in her sensations, than those of a soft, lymphatic constitution. But, however, although it is the same with man, woman is generally much more variable and changeable than he—

"Varium et mutabile semper
Fœmina.

"Weakness, say the detracters of this amiable sex, renders woman false and addicted to dissimulation. It cannot, however, be doubted that this timid and tender being may pass from the mildness so natural to her sex, to the greatest efforts of courage, and even extravagant excitement. The weakness of their nervous system, renders them susceptible of this profound agitation and this extreme excitation; every thing, in fact, exercises a powerful empire over that frail and tender organization, over slender fibres, capable of lively irritation. To complete our views of woman in general, which we have sketched, let us examine the differences which may exist between her who inhabits the country, and another born and living in the heart of a great city.

"It is generally known that country girls enjoy the advantage of being nursed by mothers who live a life conformable to the will of nature; scarcely have they come out of first infancy when they are already bent under the yoke of labour: frugality presides over their repasts; their education, too, free from minute precautions, insensibly familiarizes them with the various vicissitudes of the seasons; every day sees them grow and increase in strength; they pass without a storm and without danger, the epoch of puberty, so often fatal to the girls of great cities; they soon acquire their full developement, and may fulfil all the duties of these without experiencing inconvenience from pregnancy.

"Let us look into the heart of great cities; we there see most girls, whose parents are in easy circumstances, put after birth into mercenary hands, sucking strange milk, receiving no care but what is purchased; restored to their family, their weakness is increased and nourished; it is hoped to withdraw them from the laws of nature by removing from them her painful exercises; but soon the frail and feeble constitution of these girls becomes the sport of the seasons, and each of their revolutions is for them a fertile source of infirmities; susceptible of every thing, they show every moment the dart which wounds them. Entered into the world, women soon become the slaves of pleasures by sacrificing to them the hours of rest—tasting the sweets of sleep only when they are exhausted with fatigue; artificial appetites often make them have recourse to food more highly seasoned, which produces painful digestion; theatrical scenes which most excite their passions, are those to which they give their preference: for some, play is a deity to which they sacrifice; finally, they generally expose themselves to the severity of the season covered only with a mere gauze. The moment of conception arrives. Too little impressed with the new duties which nature imposes upon them, these women neglect to divide the period of digestion between the period of rest and moderate exercise, and a frugal life; some even forget themselves so far as to throw themselves into pleasures least suitable to their new state, and thus discover the secret of metamorphosing a very natural state into a fertile source of grave accidents and critical circumstances.

"How much caution and prudence, therefore, is required by the physician, in order to regulate the health of an organization so fragile and so active as that of woman in all states of her life! How many caprices are in her passions! How much play and elasticity in the springs of that inconstant sensibility! Into abysses of the heart must the physician descend, sometimes with discretion, at others with an imposing firmness!"

The author, after giving the above as introductory remarks, divides his work into the following heads:—Pregnancy; including treatment, diet, accouchement, &c. &c. &c.; Attentions required by the Infant; with all the requisite directions for rearing it, such as attention to clothing, sleep, food, suckling, weaning, and cleanliness: Medicine of Infancy; in which division all the ailments of infancy are treated of, as teething, thrush, worms, &c. &c., with remarks on vaccination. The work concludes with an Appendix on a new mode of Treatment of Consumption, in which the principal feature is the *oleum animale fœtidum*, which Dr. Palmedo and others have used with much success.

We draw the attention of chemists to the preparation of the *oleum animale fœtidum*, on the correctness of which much of the success in its application depends; and that of our medical readers to its administration in consumption.

An Investigation of the proposed Scheme of Medical Reform, in Reference to Chemists and Druggists: comprising an Analysis of Mr. Hawes's late Medical Profession Bill; a Review of the Pharmaceutical Society; and a Defence of the Druggists' "Counter Practice." Addressed to BENJAMIN HAWES, ESQ., M.P., and the Chemists and Druggists of the UNITED KINGDOM. By G. CROOK, M. P. S. London: R. Hastings, 32 pp. 8vo. 1841.

AN ably written and neatly printed *brochure*, by a druggist, on the subject of Medical Reform, as affects that branch of the medical profession to which he belongs, which has, in our opinion, greater claims on the attention of that class than most writings on the subject that have hitherto appeared.

It may be thought by many, that the period for the publication of such a work as this has gone by, and that, therefore, its appearance is uncalled for. A more dangerous fallacy does not exist. Those who have been lulled to repose by the hope that their rights will be sufficiently protected by the Pharmaceutical Society, will find, perhaps TOO LATE! that they have been labouring under a mischievous delusion, and that the efforts of that association have been calculated only to aggrandize certain assuming individuals, who have no object in view but the advancement of private interest; in witness whereof, we would remind our readers of the late monthly "show-off" in Oxford Street.

But Mr. Crook lays before his brethren a very excellent investigation into the subject; and we will back the individual efforts of himself and other equally praiseworthy writers on Medical Reform, against the wonder-working lucubrations of more pretending authors.

After some observations on the Society, the author says:—

"There is nothing in the foregoing observations opposed to the Pharmaceutical Society, further than is contained in the opinion, that the principles upon which it is established can only be advantageously carried out in connection with an act of parliament or charter, by which alone its regulations can be effectually enforced. It has repeatedly been urged, that a charter must be drawn up on certain fixed principles, and those principles ought to be practically tested before such an instrument can be obtained. I would inquire, if such actual demonstration of the successful working of the systems of other societies has been required previous to their obtaining the protection sought? If not, why should not the Pharmaceutical Society be equally successful? It would appear, the originators themselves mistrust the efficacy or propriety of their designs; otherwise, if the principles upon which the Society is based are just, and known to be practical and advantageous to society, why should its founders delay their application for that sanction and authority, which can alone crown their labours with success? Sufficient has already been compassed, to compel even our enemies to admit that we are at least influential as a body; and shall we neglect the *only* means by which we can become united? I fear, if we seek not the one until we are ostensibly the other, the objects of the Society will be no nearer advanced towards attainment half a century hence than now. Hitherto it has been customary to grant protection to societies, on the *principle* upon which they were established, regardless of the minor details of the systems of government. With respect to the Pharmaceutical Society, its laws having been promulgated, what further proof can be required, than that it will be alike beneficial to the trade, the profession, and the public? An institution founded upon the principles declared, requires no probationary period, its advantages being at once apparent. The very attempt to obtain legislative aid would tend to strengthen the bond of union, and ensure additional support. In defence of this hypothesis, I copy the following from a notice of the Pharmaceutical Society in the 'Medical Gazette' of 30th July: — 'It is not difficult to foresee, that a society of this kind, however laudable its objects, will be limited in its capabilities of doing good; possessing neither the advantages of a charter, nor the exclusive privilege of granting licenses, it cannot be expected to exert a general, much less an universal, influence; and it will be open to opposition from other sections of members of the same calling. In its present state, the Pharmaceutical Society is more analogous to the Medical and Chirurgical Society than to the College of Surgeons; and though, like the former, it may grant honour to its associates, it cannot grant even so much of privilege to them as the latter can. It is to be hoped, that its present is only an embryonic state of the Society, and that the designs of its founders may, ere long, be carried out. There can be no doubt, that 'the support of government, in completing the success of the measures,' which, it is said, may 'reasonably be anticipated,' should be one of the main and first things sought for. Nothing would be a better security against the dreaded evils of a reform forced upon them by others, than for the whole body of Pharmaceutists, through their Society, as a representative organ, to seek for leave and power to reform themselves, and to offer to

submit to laws which they can prove to be desirable for the public as well as themselves.' The question here may be propounded,—What is to prevent the future enactment of such a provision as that contained in clause 34 of the Medical Profession Bill? wherein it is set forth, 'no person shall have the power of granting licenses to practice medicine, or to carry on the trade of chemist and druggist, other than by virtue of this Act."

We heartily concur in the following observations—

"Having applied the several rules laid down for the investigation and solution of the question with which we set out, it now becomes necessary to state the result of the inquiry thus pursued.

"The Medical Bill has been shown to be a compound of good and evil. On a division of its elements, a very considerable alloy of base metal has been proved to exist; still there has been found, at least, an equivalent of the precious; so that, as a whole, the combination may be considered even valuable, doubtless much more so than the spurious imitation which succeeded, and others of a similar description which, probably, will follow. I conceive it, however, to have been clearly established, that it contains such a considerable proportion of good material, as, upon the abstraction of its baser components, to leave a metal of sufficient purity to be converted into standard coin for immediate circulation, from which would be derived some really tangible advantage. On the other hand, a review of the Society has shown it to be established on principles which will enable it to do much ultimate good; but, in its present condition, it is subjected to certain opposing interests, that tend to undermine its foundation, or, at least, to neutralize its operations, and, thus inert, to be scarcely capable of carrying out any *immediate substantial* advantage. Thus, it would appear, owing to its very limited power, the benefits to be derived are by far the greater part uncertain, and at best only in perspective.

"It was proclaimed by Mr. Cooper, at the meeting of the trade, held at the Crown and Anchor Tavern, on the 15th of February, that Mr. Hawes's Bill would confer a boon on the trade. I am not inclined to dispute this assertion, neither am I prepared entirely to concur with it; but I maintain, that with the amelioration of the one objectionable clause, viz. that prohibiting druggists from practising in the minor medical and surgical cases, and which, I think, must generally be allowed to be the only feasible objection that can consistently be urged against it,—it would realise more immediate benefit to the trade, than it is possible for the Pharmaceutical Society to confer, unless chartered or otherwise protected; at all events, the restrictive measures or other disadvantages, other than the clause referred to, would not be inflicted on the present generation.

"In drawing up this investigation, my endeavour has been, to show there were very many good points in the bill introduced to the notice of the legislature by Mr. Hawes, which, in the heat of debate, from the anxiety evinced to repel an unjust attack, and the impulse of the moment, was unfortunately overlooked, and to call the attention of all those interested in the question to their consideration; as I am of opinion, that much immediate advantage would be derived by the community in general, and the chemist and druggist in particular, on the passing of the said act, with the proviso herein suggested. Though, to pass the muster-roll of public opinion, and to obtain credit for its disinterestedness and impartial legislation, it must eschew all such dubious expressions and enactments, as those which relate to restraining those only who 'practice for remuneration or gain;' for, independent of such palpable disclosures of the real object sought, it is but too evident that individual advancement, self-interest, and pecuniary advantage, are the prime instigators of the proposed scheme of reformation in the government of the medical profession, rather than the *public weal*, however plausible the arguments advanced, or specious the garb in which they may be clothed.

"I will ask, in conclusion, if the present state of the trade is so satisfactory, and the whole body in such prosperity, as to require no immediate protection or relief? If such can be proved to be the case, I fancy we shall soon have the apothecaries resuming their original occupation. Whether the advantages to be conferred on succeeding generations by the Society, are considerations superior to the present, is a question, the reply to which, by the more affluent, is not difficult to guess; but, for an impartial answer, I appeal to the body of the trade. Whether those advantages will be superior to any that can be at present obtained, remains to be seen: and I will conclude with the suggestion, that it is possible, in legislating for the future, as entirely to overlook the evils of the day, as it is, in legislating for the present, to disregard the future."

We feel that we cannot too strongly urge a perusal of this admirable pamphlet: it should be carefully read by every druggist in the kingdom, and we hope that not one will fail to do so.

TWELVE PERSONS POISONED BY THE EXTRACT OF ACONITUM NAPELLUS.*

BY SIGNOR BOLARDINI.

On the 11th of June, 1840, twelve persons afflicted with scurvy, took, in mistake for the extract of cochlearia, ninety grammes each of extract of aconite.

A man, aged 60, was the first who exhibited symptoms of poisoning. Respiration was impeded; he was seized with vomiting. A medical man, who was called in, thought that it was an attack of asthma, and ordered a bath, a dose of castor oil, and a large blister on the sternum: no relief resulted, and the patient died in a few hours.

Two women, of 55 years of age, also afflicted with scurvy, and who took the same dose of aconite, became uneasy, convulsions ensued, then great prostration, and even like a kind of paralysis; and two hours after swallowing the poison, they died.

The nine other persons who had taken the aconite were also very seriously affected, and, without doubt, they would have died, but for efficacious remedies.

SYMPTOMS OBSERVED.

Great bodily weakness, with moral prostration. The face pale, and the countenance altered; great dilatation of the pupils; the eyes had lost their vivacity, and were surrounded by a bluish circle; vertigo, cephalalgia, especially towards the occiput; stomach tense and painful; greenish vomitings, and, in some, greenish diarrhœa. Oppression and anxiety, general sensation of cold, rapidly increasing, with lividness of the nails; cramps in the limbs; the pulse small, weak, and scarcely perceptible.

These symptoms were evidence of poisoning by an acro-narcotic substance, but the nature of which could not be determined. The substitution of the extract of aconite for that of cochlearia had not yet been discovered. In this uncertainty, an emetic was administered; then the general lowness was combatted by diffusible tonics, tincture of cinnamon, Hoffman's anodyne, wine, rum and aniseed in water, but not in sufficient quantity to cause intoxication; at the same time the limbs were submitted to alcoholic frictions.

The heat and the pulse were promptly restored, with the mental powers; the countenance resumed its natural aspect, and in a few hours the patients were completely cured.

EXAMINATION OF THE BODIES.

The bodies of the three persons who died from the effects of the poison were carefully examined. Externally, there was nothing remarkable.

Head.—Pia mater and tunica arachnoïdes considerably congested, and an abundance of serous fluid at the base of the cranium, and under the tunica arachnoïdes. No effusion in the ventricles.

Lungs.—Congestion of the lungs. The heart soft, and containing black blood; the large vessels distended with that liquid.

Abdomen.—Liver and spleen in the normal state. The stomach, distended by gases, contained a viscous liquid of a greenish color. Irregular congestion of the internal coat, especially at the greater curvature. The duodenum and the small intestines presented, here and there, red stains, and contained the same matter as the stomach.

* *Memoriale della Medicina Contemporanea.*

SCAMMONY AND JALAP: IDENTITY OF THEIR ACTIVE PRINCIPLES.

To the Editors of The Chemist.

Southampton, Jan. 14, 1842.

Gentlemen,—

It does not appear to have been noticed by any Pharmaceutical author, that scammony and jalap, which are two of the most important vegetable purgatives we possess, are identical, as regards their medicinal properties; but I am induced to conclude that such is the case, from the result of the following experiments, and will leave others to judge as to whether I am correct; for I believe it is quite time that some plan should be adopted for obtaining scammony at much less expense, as an inferior article is so often sold and purchased by those who are fond of gaining custom, by reducing their prices, which does not contain one half as much of the medicinal property as that imported from Aleppo.

To prove this, however, it is only necessary to digest one drachm of scammony powder in rectified spirit until all the resin is taken up, which is known by its turning milky with water so long as the resin is held in solution: mix the tincture with water, and, when the resin has entirely precipitated, pour off the clear liquid and dry the resin until it is capable of being reduced to powder; in this state it should weigh from forty-three to forty-five grains; but the inferior scammony will yield only from sixteen to twenty grains from the same quantity.

The purity of jalap may also be proved by the same process: two drachms of the powdered root, having a resinous fracture,

and of a brownish grey interior, gave ten grains of resin, and the light and white root nine grains.

To prove whether the scammony contained any more of the medicinal property after the resin was extracted, I took twelve grains of the residue, and no effect took place. A few days after, I took twenty grains of the extract of jalap deprived of the resin, and no action was produced. I afterwards took three grains of the pure resins of jalap and scammony in the form of a pill at different periods, and found them act as brisk purgatives, and without perceiving any difference in their strength. It may be necessary to state, that every dose was taken when the system did not require medicine, as a material difference in their effect would be the consequence if this caution were not observed, because the resins are rendered more nauseating and irritating by the presence of acid in the stomach: it is from this cause that children so often reject these purgatives. Alkalis, on the contrary, have the power of modifying the activity of resins by entirely suspending their griping effects—a fact which I proved by boiling them in a solution of soda until dissolved, and by taking a dose of each; I found nine grains of each resin, when held in solution, equal in effect to only three grains of the solid resins; their action was particularly mild, and they might be introduced into practice as a most agreeable purgative, since their taste was scarcely perceptible when taken in any of the mineral waters; immediately after they were swallowed, however, an irritation was excited in the throat, which formed an objection to their use. Moreover, the resin thus held in solution is liable to be precipitated in the stomach when it comes in contact with an acid, which would increase the effect twofold.

The irritation produced in the throat is caused by a volatile oil, and the griping, by an acid; both of which may be separated by distillation. Two drachms of the resins were put into separate glass retorts, and kept in a sand bath until all the acids and oils distilled over. The first portion, however, that came over, was only a little aromatic water, which was taken from the receiver without being allowed to dilute the acids. The oils thus produced were of a dark brown color, becoming white by redistillation, but soon returning to their former color. The acids, from the tests which I used, I believe to be nothing more than strong acetic acid impregnated with the odor of the oils. The acids and oils of both resins exist in the same relative proportions, but not in equal quantities: about one of the former to two of the latter is as near as I can recollect, having lost the memorandum since I performed the experiment —now two years since.

The fact of the alkalis diminishing the action of the resins, led me to suppose that the oils could not be very active, as the acids were separated from them, and as they could not be united and taken in the form of draught, owing to their being so exceedingly nauseous; I therefore took one drop in the form of pill; but even in that state, and being covered with silver leaf, it irritated the fauces so much, as to cause it to be rejected several times; but after it passed the throat, I only found a hot and nauseous taste, and after a short time a degree of warmth in the abdomen, with a sensation similar to that produced by a mild dose of medicine, without being sufficiently potent to carry itself off. In this experiment I was also unable to detect any difference in the strength of the two oils; and I am of opinion, if the quantity had been a little greater, the effect would have been the same as the alkaline solutions; but should it be doubted that the oils constitute the active principle of the resins, it may be proved by introducing a few drops in a capsule, in which manner it may be swallowed without inconvenience.

To procure scammony at a cheaper rate is the object of my publishing the foregoing; and I think nothing more would be necessary than to express the milky juice from the fresh roots of jalap, and allow it to evaporate spontaneously, which, I should imagine, would procure a larger quantity and equally pure as the scammony obtained by making an incision in the root, and allowing the juice to exude; however, this cannot be tried in this country, but I think it would be worth the attention of some individual who may have it in his power to try what I have now suggested.

I am,
Gentlemen,
Your most obedient Servant,
HENRY OSBORN.

ESTABLISHMENT OF A CHEMICAL SCHOOL ON THE NEW METHOD OF SCIENTIFIC EDUCATION.

IT is with great pleasure that we are enabled to announce to our readers the establishment of a CHEMICAL SCHOOL on the plan proposed in Nos. XXII. and XXIII. of this Journal. The OPENING ADDRESS was delivered by Mr. JOHN WATT, before a numerous and highly respectable audience, on the 27th ult. and by the time that this No. is published, the First Lecture on Chemistry, embracing

the subject of Heat, will have been delivered by Mr. Charles Watt, jun.

No endeavours will be wanting to give effect to the system; and we feel confident that by it the student will acquire more in three months than he would in as many years by the old one, and with greater facility.

The students in Chemistry applied to the Arts will be taught Chemical Manipulation, and all that bears more especially on his particular Art, under the personal direction of Messrs. J. and C. Watt, and will have the advantage of learning Experimental Chemistry under their eye, in the laboratory attached to the School.

The course of Chemistry applied to Medicine and Pharmacy is particularly advantageous to Chemists and Druggists, their assistants and apprentices. They will be taught every thing in Chemistry which relates to their vocation, as well as the Sciences generally.

REVIEW :—

An Essay on Diabetes. By H. Bell, D. M. P., One of the Librarians of the Faculty of Medicine of Paris. Translated by Alfred Markwick, late *Externe* to the *Hôpital des Vénériens*, Paris. London: Pigott, Kennington; Highley, Fleet Street. 1842.

It is with much pleasure that we give our testimony in favor of this little work, and we recommend its perusal to the student, and to the medical practitioner. It contains, in a condensed form, all the knowledge of which we are at present possessed relative to a malady, the treatment of which has always proved far from satisfactory to the physician. A number of books on one subject is a great and serious evil; and he is deserving of praise, who takes upon himself the labor of collecting in one the valuable information contained in each, thus laying it before the profession in a form most likely to be beneficial. In addition to the knowledge derived from other sources, the Author has given the world the benefit of his own extensive observations. Much also is due to the Translator for the able manner in which he has performed his very arduous task.

We quote the following from page 15 :—

"The saccharine matter of diabetic urine is identical with that of starch, and, like this, has the property to combine with common salt (chloride of sodium). It is generally sapid, its taste is of a sweet nature like honey, and this is generally sufficient to recognise it; but this method may sometimes lead us into errors, as diabetic urine is at times completely devoid of a sweet taste, although it may possess all its other qualities indicating the presence of sugar. In this state, if we boil it with a tenth part of sulphuric acid, it becomes perfectly sapid. This peculiarity of the presence of an insipid sugar in diabetic urine had been mentioned by Thénard and Dupuytren.*

M. Thénard described it as a peculiar kind of sugar. In a much more recent work, M. Bourchardat, having studied with care the properties of this sugar, found that its composition is exactly the same as that of sugar of grapes.† Since then this skilful chemist has continued his researches, and has discovered that the insipid sugar is merely a mixture of diabetic sapid sugar with the other elements of urine, such as lactate of urea, lactate of soda, and extractive matter.‡ The quantity of saccharine matter which this urine contains, varies considerably. M. Bourchardat has met with some containing only an eightieth part, while in others he found at least a seventh of their weight. (Loc. cit. p. 193). Vauquelin and Segalas have extracted from a diabetic urine, a seventh of its weight of saccharine matter.§ In an analysis made by M. Péligot, the saccharine matter was in the proportion of a tenth." ||

This extract is sufficient to show the manner in which the subject is treated, and we conclude our observations by earnestly recommending it to the notice of the medical profession.

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* *Annales de Chimie*, tom. LIX.

† *Révue Médicale*, June, 1839, pp. 327, 337, 338.

‡ Bourchardat, *Annuaire de Thérapeutique, de Matière Médicale, et de Pharmacie*, Paris, 1841, p. 199.

§ *Journal de Physiologie*, tome IV. p. 354.

|| *Annales de Chimie et de Physique*, tom. LXVII. p. 140. 1838.

TO CORRESPONDENTS.

Nota Bene.—*All Communications and Books for Review must be addressed, "To the Editors of The Chemist, Chemical School, 198, Strand, London." Communications must be prepaid, and sent before the 15th of each Month; Books for Review, before the 10th.*

THE CHEMIST.

I. CHEMISTRY.

DESCRIPTION OF SOME PROCESSES FOR ANALYSING ATMOSPHERIC AIR.*

BY C. BRUNNER,
PROFESSOR OF CHEMISTRY AT BERNE.

THE chemical analysis of the atmosphere has ever been a part of chemistry which has attracted most of the attention of men of science. Hence, methods, in great number, have been devised, one after another, to arrive at perfectly accurate results. The value of these methods is very differently estimated, as may easily be conceived. That which might be regarded as correct in the past century, may now appear insufficient, and we may expect to be judged in the same manner by our successors. However, all our efforts to establish on a better basis one of the most interesting points of chemical science appear to deserve attention, and must necessarily lead to greater degrees of perfection, even when they do not furnish more accurate results.

I shall not here undertake to discuss all the methods of analysing atmospheric air which have hitherto been devised; the object of my memoir being rather to collect certain processes which I have at different times described, and which are but partially known, because they are scattered in several scientific journals. I shall at the same time point out some improvements which I have made subsequent to their publication.

An illustrious French chemist [Dumas] having recalled attention to this subject, and having availed himself in his experiments of a process analogous to mine, I think it my duty, on my part, to furnish the materials which I possess. Perhaps they may assist in the continuing of these researches, to which an association of men of science of the first rank appears willing to devote itself.

When, by the efforts of all, a method of analysis of easy and certain execution shall have been found, we may follow, not only the variations which may occur in the composition of the atmosphere of one part of the globe as compared with another, and those which the different strata of the atmosphere may present, but likewise the changes which may take place in the course of a year, or longer periods.

All my experiments tend to show that these changes are very trifling: in all chemical works they are stated not to exist, at least so far as regards the two constituents which are looked upon as essential. There is nothing, however, to prove this absolute stability; for it would be easy to show that the methods hitherto employed are not accurate beyond the $\frac{1}{200}$, or even $\frac{1}{100}$ of the volume of the air operated on, and although a mean deduced from all the eudiometric experiments hitherto made might give a tolerably accurate result for the whole question, it is evident that it could not be satisfactory in particular cases.

Before describing the methods which I have adopted, I may be permitted to notice some defects common to all those which have heretofore been proposed.

The first defect which known methods present, is that of being applicable to only, ordinarily, a very limited volume of air. All the eudiometers which have ever been described, are capable of removing, by means of an oxidisable substance, the oxygen contained in but a very few cubic inches of air. It is easy to demonstrate the insufficiency of the determination of the absorbed volume, by comparing the quantity experimented on with the inevitable errors of gauging. I will say but a few words on this subject.

If a gas be measured in a water butt, (*cuve à l'eau,*) it is impossible to judge of its volume to $\frac{1}{200}$. Not only the contact of the gas and water, which, as is known, absorbs a small quantity of it, opposing

* *Annales de Chimie et de Physique*, Novembre, 1841.

the accuracy of the operation, but also the difficulty of determining the level of the liquid, owing to the adhesion of the water to the glass, the quantity of water which by wetting the interior of the vessel, diminishes the capacity of the space occupied by the gas, are so many sources of error which influence the result. I say nothing of barometric pressure, of temperature, of the tension of the water, the effects of which are known, and may be calculated. If, to obviate these inconveniences, a mercurial trough be used, new difficulties are presented. It is impossible to pass a gas through this liquid without some bubbles attaching themselves to the sides of the cylinder, or remaining engaged in some manner in the mercury: this is proved by the necessity of boiling this metal for the making of barometers. But a much greater source of error is that presented by the difficulty of keeping the two levels of mercury quite equal during the gauging of the gas. It is evident that the slightest difference between them must have very considerable influence on the volume measured. Besides, these operations over mercury are hardly practicable in travelling.

These inconveniences are less perceptible in the determination of the constituents of the atmosphere, which are regarded as variable and accidental,—aqueous vapor and carbonic acid,—since it is practised by entirely different means.

The methods which I adopted for analysing air rest on the employment of a very simple apparatus, and which is, I think, less liable to the errors just mentioned. It consists of a vessel of undetermined form and dimensions, filled with a suitable liquid, which, while flowing out by an orifice situated at the lower part of the vessel, is replaced by an equal volume of air entering into the vessel by an orifice in the upper part of the vessel. This apparatus drawing in the air, as it were by aspiration, I have called it an *aspirator* (*aspirateur*). By measuring the liquid which runs out, the volume of air drawn in may be estimated. At the time of the air entering into the vessel it is submitted to the influence of the proper reagents for ascertaining its nature, by making it pass through either Woulfe's bottles, tubes, &c.

By adopting this system, the following advantages are obtained:—

1st. That of operating, at will, on considerable quantities of air, which diminishes the errors of observation.

2nd. That of employing a variety of reagents.

3rd. That of obtaining results by a positive effect, either by increase of weight, or by formation of a precipitate, &c.

Before passing to the analytical processes employed, I may describe a form of aspirator, which is very well adapted for cases in which it is not required accurately to determine the volume of air operated on.*

It consists of two cylindrical vessels, of white iron or copper, of equal capacity, from $\frac{3}{4}$ to 1 cubic foot, for example, closed at each end, placed over one another, and joined by a stem of iron which crosses a transverse stem, on which the apparatus may turn as on an axis, in such a manner as that the two vessels may be made to change places. The upper vessel being filled with water, it is evident, that by making that liquid run out through a stop-cock into the lower vessel, the upper vessel will be filled with air by a small aperture at the top. The vessel being emptied of water, the aperture is closed, the apparatus is turned on its axis, and the operation is continued by making the water flow from what was the lower vessel into that which was the upper one, these having been made to change places. In order to make the air contained in the lower vessel escape, a stop-cock bored at a right angle is opened.

In the aperture used for the admission of the air are adapted the tubes or other apparatus through which the current of air is to be directed.

This apparatus is well calculated for evaporating, or drying substances in the sand bath, and for burning others in a current of air, &c.†

ANALYSIS OF AIR.

It is known that the atmosphere is composed of at least four substances, which have always been found in it in the state of mixture, and not in that of chemical combination. Two of these bodies, carbonic acid and aqueous vapor, exist in it in very small and very variable quantity. They are generally regarded as accidental, especially as we can in some measure account for their continual variation.

DETERMINATION OF WATER.‡

The estimation of the quantity of water contained in the atmosphere is commonly practised by indirect experiments, by means of hygrometers, which are described in all

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* The first description of this apparatus was published in *Poggendorff's Annalen*, Vol. XXXVIII. page 265.

† See its application to organic analysis, *Bibliothèque Universelle*, Juue, 1838, page 356; and February, 1839, page 405.

‡ The first description of this method was published in *Poggendorff's Annalen*, Vol. XX. page 274, Nov. 1830.

works on physics. It is thus performed by means of the aspirator :—

The aspirator employed is a vessel of any form,—a large bottle for example,—with an upper and a lower aperture, filled with water, which, in flowing out, is received into a bottle whose capacity has been accurately determined. In default of a vessel with two apertures, a syphon may be used. To fix the tubes hermetically in the aperture of the vessel, the following is the most convenient arrangement :—A leaden stopper is made to fit very accurately in the neck of the bottle, and is kept in by its edge. This stopper is bored with two holes, into which the tubes are admitted. The hermetic closing is effected by means of a lute composed of equal parts of ceruse and red lead, mixed with linseed oil previously boiled with $\frac{1}{10}$ of litharge. This lute is applied round the neck of the stopper, before putting it into the bottle, so that by placing it in its opening, and pressing on it a little, it is also hermetically sealed. The tubes, also furnished with lute, are likewise fixed by a slight pressure, when placed in their apertures.* A stop-cock may be used for closing the syphon as soon as the experiment is concluded.

Concentrated sulphuric acid is the substance used for absorbing the water contained in the air operated on. A sufficient quantity of it is put into a glass tube containing asbestos, and it thus remains fixed throughout the whole length of the tube. In order conveniently to charge this apparatus, previously dried asbestos is introduced into it, then the acid in sufficient quantity to moisten the whole of it, without running out at the inferior aperture.

It may be thought that the employment of chloride of calcium would be preferable to that of sulphuric acid : I have ascertained to the contrary. A tube, of 37 inches in length, and $\frac{1}{2}$ an inch in diameter, filled with chloride of calcium, dried a current of air saturated with aqueous vapor less completely than a tube of 8 inches containing asbestos moistened with 40 drops of sulphuric acid. A second similar tube, adjusted to the latter, did not increase in weight, whilst, when chloride of calcium was employed, the air after having traversed it, increased the weight of the second tube several milligrammes. A direct experiment proved to me that a tube containing 40 drops of sulphuric acid may absorb 2·5 grs. of water, before a second similar tube, traversed by the same current, is sensibly increased in weight.

* This kind of closing is applicable in many other cases, and it is infinitely preferable to corks.

In order to contain the sulphuric acid in the interior of the tube while weighing, it is necessary to introduce into the two extremities after having charged the tube with sulphuric acid, plugs of asbestos not moistened with acid. It is also requisite to close the two ends with corks, or metallic stoppers, to prevent the air from increasing the moisture during the manipulation.†

The operation itself consists in drawing off the desired volume of water, and in determining the increase of weight of the tube.

I do not here give the ciphers obtained by different experiments, for they are not of any scientific interest. I observe only that consecutive operations have furnished me results so agreeing with one another as to make me think that this method is applicable to all investigations of this kind. It is particularly useful in the verification of hygrometers.

DETERMINATION OF CARBONIC ACID.‡

The aspiratory apparatus is the same as that already described. The substance used for absorbing the carbonic acid of the air is slaked lime. The apparatus used for this purpose is composed of two tubes. One is a tube containing asbestos moistened with sulphuric acid, similar to that which I employ for the determination of water. The second tube consists of two parts of unequal diameter ; one contains lime, the other, asbestos moistened with sulphuric acid. The first tube, containing asbestos and sulphuric acid, is used for drying the air which enters into the apparatus, which would otherwise deposit its water in the tube containing the lime, whence would result too great an increase of weight ; the third tube contains sulphuric acid, so as to remove the moisture which the current of air derives from the lime. The space between the asbestos and the lime is filled with fragments of glass or porcelain, so as to prevent them from coming in contact. Between the first tube containing asbestos and sulphuric acid, and that containing slaked lime, a piece of damp sponge is placed, to facilitate the complete

† The apparatus for drying gases by means of sulphuric acid according to the principle of Woulfe's apparatus appears to me less efficacious than that which is described above. The gas, passing into it rather rapidly, traverses the liquid in bubbles which present to the liquid only a very limited absorbing surface.

‡ The first description of this process, although somewhat differing from that here given, was published in *Poggendorff's Annalen*, Vol. XXIV. page 569.

absorption of the carbonic acid. The lime employed is slaked and slightly damped, but in such a way as not to form lumps. For some time, I mixed it with moss to render it more permeable to the current of the gas. Experience has now shown it to be useless. But it should be lightly piled up in the tube, and it should be ascertained by aspiration that the current of air percolates it without impediment: during the weighing, and other manipulations, the tube is closed with corks.

I here likewise suppress the results of those of my experiments which were intended only for fixing the method. I may observe only that the results obtained were between the limits which M. de Saussure had determined by a very different method, and those which have more recently been fixed by M. Boussingault, who availed himself of my process.

DETERMINATION OF OXYGEN, OR EUDIOMETRIC PROCESS.

I employed, as an eudiometric substance, successively metallic iron and copper, at a high temperature, and phosphorus at the ordinary temperature.

I employed the two metals in the state of turnings mixed with the metal in powder, such as is obtained by reducing the oxide by a current of hydrogen gas at a high temperature. A glass tube containing the metal was heated to redness over a spirit lamp, or coals, while the current of air traversed it. The increase of weight of the tube indicated the quantity of oxygen fixed.*

I think it useless here to describe this apparatus in detail; it is very simple. The results which I obtained by using it presented differences of $\frac{1}{2}$ per cent. in volume of the air analysed, which caused me to abandon it. I attributed this want of agreement either to the property which powders possess of absorbing gases,—thinking that the metal employed might sometimes retain a little of the hydrogen used in preparing it, or air after the eudiometric operation,—or to an unequal temperature of the tube before and after the operation. Besides, this process required the employment of fire, which renders it less adapted for experiments on a journey.†

After many attempts to arrive at more accurate results, I finally succeeded by means of phosphorus; and the apparatus which I am about to describe appears to me to be best adapted.

It consists of an eudiometric tube and a tube for retaining the carbonic acid and water of the aspired air.

The eudiometric tube is charged in the following manner:—Into one part, a piece of phosphorus weighing about 1 gramme is introduced; it must be slightly heated, and, by turning the tube on its axis, the melted phosphorus attaches itself to its sides. The enlarged part of the tube is lightly filled with cotton, to a certain length. This tube, thus filled, is adjusted to the aspirator and to another tube by means of metallic stoppers sealed with sealing wax.‡ The aspirator is filled with olive oil.

I chose this liquid in preference to water, in order to avoid:—

1st. The effect of the tension of the vapor of the latter, which would necessarily affect the volume of the gas.

2nd. The possibility of the absorption of a small quantity of nitrogen gas by water.

3rd. Evaporation.

Before commencing the eudiometrical experiment, the phosphorus was fused while the oil began to flow out. The combustion of the phosphorus immediately took place, and the product was absorbed by the cotton

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* The description of this method appeared in *Poggendorff's Annalen*, Vol. XXVII.

† It appears that MM. Dumas and Boussingault have overcome these difficulties. In their experiments, they used the same process, at least as to principle, with the difference that the air analysed was first aspired by one exhausted flask, and that the quantity of nitrogen was weighed.

‡ The following is a detailed description of this mode of junction. If it be required to connect two tubes, one of which is drawn out to a point, the other is closed by a copper stopper sealed in the mouth of the tube by means of sealing wax. The pointed tube is introduced into the aperture of the stopper as far as its gradual increase of size will permit, and is fixed in it by means of fat lute, as above-mentioned. To connect two tubes, neither of which is drawn out to a point, both ends are closed by means of perforated copper stoppers, and there is introduced, into the two apertures, a small tube which has in the middle a disc soldered on the tube. After having put a little lute on the two surfaces of the disc, the two tubes are joined by gently pressing them together. This method of hermetically connecting apparatus is much more convenient and sure than by corks, and even than by caoutchouc tubes. I use it for all gases which are not decidedly acid. It is very useful in apparatus for organic analyses. To weigh the tubes before and after the operation, the lute is carefully removed, and the stoppers are closed by means of a ball of wax.

contained in the tube. This product was a mixture of phosphorous acid and a little oxide of phosphorus. When three or four ounces of oil had run out, the operation was stopped by turning the stop-cock of the aspirator, the apparatus is cooled, and the eudiometrical tube was weighed.

This preliminary operation was performed with the object of forming a small quantity of a compound of phosphorus, which being in itself very oxidisable, served, during the eudiometric experiment, to remove a small remnant of oxygen, which might escape the action of the phosphorus.

The eudiometrical experiment was made after having again adjusted the eudiometric tube, after having weighed and gently heated the phosphorus to fuse it, opening the stop-cock of the aspirator, and allowing the oil to flow out, which was received into a vessel measured with much care.* The current of gas ought not to be too rapid: I employ about an hour to run out $1731^{c.c.}687$ of oil. The combustion of the phosphorus, when once commenced, continued without intermission, without the assistance of external heat, producing the ordinary light which phosphorus gives in the dark. It is not the rapid combustion with a brilliant light, productive of phosphoric acid, that must be employed: the effect would be too sudden, and a great portion of the product would pass into the aspirator.

This operation terminated, the eudiometrical tube was weighed, and the increase of weight gave the quantity of oxygen fixed. This weight was converted into volume by calculation, taking into the account atmospheric pressure and temperature, and it was compared with the volume of the nitrogen represented by that of the oil run out.

It yet remained to be ascertained whether this volume of the oil run out really was equal to that of the nitrogen which had entered into the vessel. Berthollet was of opinion, as is known, that by using phosphorus as an eudiometric substance, the volume of the nitrogen remaining was increased about $\frac{1}{40}$, and that to have the exact quantity that amount must be subtracted.† Parrot maintained the contrary opinion, saying that the gas did not change volume.‡

In order to examine this point, I prepared nitrogen by passing a current of atmospheric air through copper heated to redness, and I collected it over mercury. A stick of phosphorus, placed in the gas, did not change its volume, so far as could be observed by the ordinary means of gauging, and observing the barometrical and thermometrical changes.§

Another experiment was made to ascertain whether an appreciable quantity of phosphorus would be removed by evaporation in the current of gas. A current of nitrogen, at the ordinary temperature, was driven through an eudiometric tube, charged as above described, and carefully weighed. $1731^{c.c.}687$ of dried nitrogen gas, passing in this manner into the apparatus, diminished in weight one milligramme in each result.

I give as examples the ciphers of some experiments made by this process. The volume of oil allowed to run out in each operation was $1731^{c.c.}687$. To calculate the volume of the hydrogen, it was admitted that a gramme of that body was equal to $698^{c.c.}14$, at 760 millimetres of barometrical pressure, and at 0° C. I took Rudberg's coefficient = 0.00365 for the increase of volume of the gas for each centigrade degree, commencing at 0°.‖

Date of Experiment.	Ther.	Bar. millig.	Oxygen condensed.	Volume of Oxygen in 100 Parts of Air.
July 20, 1841, at 11 a.m.	20° C.	718·5	0·575	20·867
20, ,, at 11 p.m.	19° C.	716·8	0·573	20·750
23, ,, at 10 a.m.	19° C.	718·5	0·576	20·790
23, ,, at 4 p.m.	20° C.	719	0·576	20·842
24, ,, at 7 a.m.	19° C.	719	0·578	20·812
24, ,, at 7 p.m.	19° C.	718	0·577	20·837
28, ,, at 4¼ p.m.	20° C.	719	0·575	20·818
		Mean		20·821

* It will of course be perfectly understood that the vessel which receives the oil must be carefully cleaned after each experiment, in order that its capacity may not be diminished by adhering oil.

† *Statique Chimique*, Vol. I. p. 514.

‡ *Gilbert's Annals*, Vol. X. p. 206.

§ I have not ascertained whether Berthollet's assertion is correct in operating over water.

‖ The author's results are here calculated with the density of oxygen 1·1026, and that of nitrogen 0·976; we have previously given the same results calculated with the density of oxygen 1·1057, and that of nitrogen 0·972.

In a series of experiments made in 1832 on air at the summit of the Faulhorn, in the high Alps of Bernese Oberland, I found as a mean, 20·915 of oxygen. The difference between this number and that which I obtained in the last experiments, might arise from a trifling error in the vessel which was used in my former experiments for receiving the liquid which flowed out. Unfortunately I am not in a position to ascertain this positively, as the vessel of 1832 is no longer in existence.

The process of analysis which has just been described is applicable, *mutatis mutandis*, to all chemical investigations concerning the atmosphere. It is easy to conceive the arrangement of the apparatus for submitting air to the influence of the most varied reagents. It is only necessary to arrange a suitable aspirator in order to pass the desired quantity of air through the proper solutions. It would not be uninteresting to use it on the banks of the sea, in marshes, and in places where the air is charged with deleterious matters, and miasms, as for example, in the *Maremmes* of Tuscany. I think that by this means we should arrive at more precise notions concerning those substances hitherto sought in vain, and whose very existence is admitted rather by induction, than from the result of experiment. It would be sufficient to fit up a large barrel as an aspirator, by means of which a continued current of air might be produced for whole days, even without the presence of an operator. By this means we might also easily prove the existence of salts which have sometimes been observed to be suspended in air or contained in atmospheric moisture. This would be a new kind of experiment which I recommend to those chemists who are in a situation to undertake it.

INVESTIGATIONS CONCERNING OLEÏC ACID.*

BY F. VARRENTRAPP.

Although oleïc acid is one of the most important of the fatty acids, it certainly has been studied less than any other. M. Chevreul, and more recently M. Laurent, are the only chemists who have paid any attention to it.

M. Chevreul, in his *Treatise on the Fatty Bodies*, entertains some doubts concerning the acid examined by him, as it was possible, in his opinion, that it might contain traces of margaric acid.

M. Laurent has since published investigations concerning oleïc acid, and he attributes to it a very different composition from that which had been adopted by M. Chevreul. But the acid which this chemist used had been previously submitted to distillation: now, by this means it cannot be freed from margaric acid; besides, it is decomposed by the action of heat, as I shall show in the course of this memoir.

I therefore resolved to undertake new investigations concerning this acid. In this memoir will be found the results furnished by the oleïc acid extracted from oil of almonds and beef fat, as well as the study of the products of its decomposition.

I had hoped to extract oleïc acid from the siccative oils which, for the most part, contain very little margarine; but these oils contain a very different acid, so that they could not be used for my purpose. Among the non-siccative oils, that which appeared to me to be most easy of employment, was oil of almonds, which contains very little margarine.

Oil of almonds was saponified with potassa, and the soap was decomposed by dilute hydrochloric acid. I digested the acid liberated with oxide of lead for several hours, at a temperature of 100° C. The mixture of margarate and oleate of lead which results was then re-dissolved in ether, which takes up only the latter salt, whilst a residue of margarate and basic oleate of lead is obtained.

The oleate of lead dissolved in ether was agitated in its own bulk of water acidulated with sufficient hydrochloric acid to convert all the lead into the chloride. The oleïc acid immediately separated, and floated on the liquid in solution in ether, while the chloride of lead was rapidly deposited in the aqueous solution.

The ethereal solution being filtered and evaporated in the sand-bath, the acid was presented under the form of a yellow, clear, oily liquid, possessing all the properties which M. Chevreul assigns to it in his *Treatise on the Fatty Bodies*. It is the most alterable of the fatty acids; concentrated sulphuric acid immediately turns it brown, as does hydrochloric acid gas in a short time; the dilute acts in the same manner after long contact.

In the mode of preparation which I have just pointed out, it is not necessary, in combining the fatty acids with oxide of lead, to employ exactly the quantity necessary for saturation; for all the margaric acid combines first with the oxide of lead, and ether more easily dissolves the oleate with excess of acid than the neutral oleate.

I submitted to the same treatment, crude

* *Annales de Chimie et de Physique*, Août, 1841.

oleïc acid extracted from beef fat. It was of a deep brown color, and lost but little of that tint after digestion with alcohol and animal charcoal. By the following process it is easy to prepare from it a slightly colored acid.

The greater part of the steäric acid having been separated by cooling at 0° C., the remainder was removed by oxide of lead and ether. The acid was liberated by means of hydrochloric acid, then saponified again. The soap was afterwards separated from its limpid solution with common salt; it was re-dissolved, and the treatment by salt repeated until the saline solution was no longer colored. It is as well to mix with the salt a small quantity of an alkaline carbonate, which assists in removing the coloring matter. In this manner a perfectly white soap is obtained, which, decomposed by tartaric acid, yields an almost colorless acid. The employment of tartaric acid is in this case preferable to that of hydrochloric acid, which, however dilute, alters the oleïc acid by long boiling; and because the soap of soda is not insoluble in the tartrate of soda, as it is in the hydrochlorate, so that there is not formed, as is the case with hydrochloric acid, an acid oleate which is decomposed only by long boiling.

The oleïc acid, prepared by these two methods, was submitted to analysis. The numbers which I obtained led me to adopt for the hydrated acid the following formula:—

$$C^{44}\ H^{80}\ O^5 = C^{44}\ H^{78}\ O^4 + H^2\ O.$$

In order to control this composition, I prepared oleate of silver by decomposing with nitrate of silver the salt of soda, crystallised twice in alcohol. Thus was obtained a very bulky, white precipitate, not entirely insoluble in water and alcohol, but which might be washed with cold water. In tepid water, this precipitate lost its bulky aspect, and was reduced, when further heated, to a plastic mass. When it was attempted to dry this salt, either by heat,—employing a heat below 100° C.,—or *in vacuo*, it was converted into a soft mass, which might be easily kneaded between the fingers. This mass with difficulty parted with its water, and turned brown by exposure to light.

This salt being more or less altered, I could not succeed in obtaining uniform results in the determination of the atomic weight.

Consequently, I prepared oleate of baryta by decomposing a solution of the salt of soda by chloride of barium. I thus obtained a precipitate of a fine white, light, very bulky, and easily agglutinating in warm water. After having been melted in the sand bath, and deprived of all the water which it contained, this compound was presented under the form of a yellowish transparent mass, brittle when cold, and viscous when warm, which attracts moisture from the air, becoming soft and opaque.

I also prepared this salt, either by decomposing oleate of ammonia retaining an excess of ammonia, by the chloride of barium, or by boiling oleïc acid with baryta water.

This salt being analysed, gave results which led to the formula

$$C^{44}\ H^{78}\ O^4,\ Ba\ O.$$

The anhydrous acid is, therefore, represented by the formula

$$C^{44}\ H^{78}\ O^4.$$

The metallic oxides are capable of displacing in the hydrate one equivalent of water; oleïc acid is, therefore, a monobasic acid.

OLEÏC ETHER.

I prepared this product by dissolving one part of oleïc acid in about three parts of alcohol, passing into it a current of hydrochloric acid gas. The mixture was heated, and the etherification was immediately effected. At the end of a few minutes all the oleïc ether separated from the alcoholic liquid, even before the alcohol was withdrawn from the acid, an excess of which must be avoided, as it might alter the product.

The ether thus prepared, separated from the alcoholic liquid, was agitated with ordinary alcohol, which dissolves its own volume of oleïc acid, and only a very small quantity of oleïc ether. Any alcohol that it may contain is afterwards removed by agitating with water: it is then decanted, and dried over chloride of calcium. The liquid thus prepared is limpid, with very little color: its density is intermediate between that of alcohol and that of water. Submitted to analysis, it gave me numbers which agree with the formula

$$C^{48}\ H^{88}\ O^5 = C^{44}\ H^{78}\ O^4,\ C^4\ H^{10}\ O.$$

This compound is altered by distillation; under such circumstances it furnishes alcohol and a hydrocarbon, and leaves a carbonaceous residue.

In distilling oleïc acid, there is obtained, besides a great quantity of permanent gases which are disengaged, in an uniform manner, during the whole operation, a liquid product, which is a powerful refractor of light, and which, on cooling, deposits a matter crystallised in fine needles.

If the operation be continued until the bottom of the retort becomes red-hot, a considerable quantity of carbonaceous matter is obtained.

The gas is composed of carbonic acid and carburetted hydrogen.

The condensed product contains a great quantity of a liquid hydrocarbon, mixed with

a little undecomposed oleïc acid, also containing a crystalline acid; this is sebacic acid. This last produet, being very soluble in boiling water, may be separated by that means from the other products. On cooling, it deposits almost entirely in the state of crystals very much resembling those of benzoïc acid. The formation of sebacic acid, by the distillation of oleïc acid, furnishes an excellent means of detecting the presence of oleïc acid in the fatty bodies.

In order to separate the hydrocarbon from the oleïc acid, we cannot resort to the treatment of the oleates by ether, as in the case of margaric or steäric acid, for these salts are much more soluble in the hydrocarbon. The best process consists in distilling the mixture several times with water, taking care to replace that which evaporates, until the whole of the hydrocarbon is volatilised. The operation always succeeds very well, if persevered with for a sufficient length of time.

I analysed the product thus obtained: I could not say whether the small proportion of oxygen which I found should be attributed to the presence of a body analogous to margarone. However, it was observed that the quantity of carbon found by experiment was to that of the hydrogen in the same proportion as in olefiant gas.

This hydrocarbon distils without leaving a carbonaceous residue: it cannot be regarded as an uniform product, for its boiling point varies greatly, and is raised from 160° to 280° C., and even higher.

The formation of these different products may be interpreted by the following formula:—

1 atom of hydrated sebacic acid	$C^{10} H^{18} O^4$
3 atoms of carbonic acid . .	$C^3 \quad O^6$
Hydrocarbon	$C^{71} H^{142}$
4 atoms of carbon	C^4
2 atoms of hydrated oleïc acid	$C^{88} H^{160} O^{10}$

The manner in which oleïc acid acts on being distilled, explains to us why the products of the distillation of fatty bodies, such as pork fat and olive oil, containing, besides margarine and steärine, oleïne, present greater consistence than the first matters; it is because the margaric acid is very little soluble in the hydrocarbon which is produced by distillation, while the margarine is very soluble in the oleïne.

DECOMPOSITION OF OLEÏC ACID AND ELAÏDIC ACID BY POTASSA.

MM. Dumas and Stas have lately, in a very remarkable work, drawn the attention of chemists to the action which potassa in fusion exerts on organic substances. Their investigations have thrown great light on the intimate constitution of many combinations.

I submitted oleïc and elaïdic acids to the action of the same agent: the following are the results at which I have arrived:—

I heated oleïc acid in a silver capsule, with a slight excess of caustic potassa and a few drops of water, so as to saponify it: I then added to it a quantity of potassa weighing twice as much as the oleïc acid, and I gently heated the whole, continually stirring, and fusing the potassa. If the operation be carefully conducted, the mass hardly blackens, and acquires only a brownish yellow tint. As soon as the mixture arrives at the fusing point of the potassa, a disengagement of hydrogen ensues, which is easily recognised by the pale flame which it produces in burning. When this gas is developed, the operation is terminated; the mass is quickly removed from the fire, and thrown into a hot solution of potassa. When a small quantity of water is used, the ley is so concentrated that the soap produced floats on the top, without being dissolved. The salt of potassa is separated from its solution in water, by means of common salt, and this operation is repeated until the small quantity of coloring matter produced in certain parts by too great heat is completely dissolved.

The fat acid of the soap which has thus been purified is separated by means of dilute hydrochloric acid. This fat is not liquid like that from which it is produced: on cooling, it concretes, forming crystalline layers. The crude acid, crystallised once in alcohol, fused at 56° C.; after several similar crystallisations, it fused at 62° C., a point at which it remained constant after being again treated by alcohol.

Elaïdic acid, submitted to the same treatment, produced an acid in every respect identical with this.

I analysed the crystallised acid, and obtained results which lead to the formula

$$C^{32} H^{64} O^4.$$

I prepared the salt of silver by mixing the alcoholic solution of the purified salt of soda with nitrate of silver, and thus obtained a very white, light precipitate, of a gelatinous aspect, when I operated without heat; with heat, this precipitate was granular, and more easy to wash. While it is moist, it must be kept from the action of light; but when dry, it is not sensibly affected by it.

The calcination of this salt, as well as its combustion by means of oxide of copper led to the formula

$$C^{32} H^{60} O^3, Ag O;$$

whence it follows that the composition of the crystallised acid should be

$C^{32}\ H^{60}\ O^3,\ H^2\ O$.*

The analysis of the salt of baryta perfectly confirmed these results.

I afterwards completely neutralised by sulphuric acid a certain portion of the liquid containing an excess of potassa, as well as the substances soluble in water which may be formed at the same time as the acid of which I have spoken. It disengaged only a trifling quantity of carbonic acid, and which could not consequently be regarded as a product of the reaction. By supersaturating the liquid by sulphuric acid, I observed a very distinct odor of acetic acid, and by distillation I obtained a considerable quantity of concentrated vinegar.

Another portion of the liquid was neutralised by hydrochloric acid and mixed with a solution of chloride of calcium; a precipitate was formed, which had all the characters of oxalate of lime. The oxalic acid thus obtained was found in very small quantity, so that it should rather be considered as a secondary product formed by the ulterior action of potassa at a more elevated temperature.

The formation of the great quantity of acetic acid by the action of potassa on oleïc and elaïdic acids may be very simply explained:—

1 atom of margaric acid	$C^{44}\ H^{78}\ O^4$
1 atom of the new acid	$C^{32}\ H^{60}\ O^3$
There remains	$C^{12}\ H^{18}\ O$

Supposing that the potassa yields 8 atoms of oxygen, that would make about 3 atoms of acetic acid.

With elaïdic acid, the potassa should yield 7 atoms of oxygen, and we should have:—

1 atom of elaïdic acid	$C^{72}\ H^{132}\ O^5$
	O^7
	$C^{72}\ H^{132}\ O^{12}$
2 atoms of the new acid	$C^{64}\ H^{120}\ O^6$
There remains	$C^8\ H^{12}\ O^6$

that is to say, three atoms of anhydrous acetic acid.

The simplicity of these reactions gives fresh support to the formula which I have adopted for oleïc acid, and established by M. Meyer for elaïdic acid.

Unfortunately, I have not yet been able to examine the red body which is formed besides elaïdic acid by the action of nitrous acid on oleïc acid. It is scarcely possible for me to explain in what manner this acid is produced. It appears, however, that in acting on oleïc acid, nitrous acid decomposes it into two bodies, one of which is elaïdic acid. By deducting from

2 atoms of oleïc acid	$C^{88}\ H^{160}\ O^{10}$
the elements of 1 atom of elaïdic acid	$C^{72}\ H^{130}\ O^7$
we have the body	$C^{16}\ H^{24}\ O^3$

which may perhaps combine with the nitrous acid, forming the red body so very soluble in alcohol and ether, and even in water. The examination of this red substance will, perhaps, clear up the mode of action of nitrous acid on oleïc acid. The small quantity of nitrous acid necessary for operating this conversion, by no means weakens our hypothesis, if we take into consideration the difference which exists between two atoms of oleïc acid and one of nitrous acid. The quantity of the latter should necessarily be inferior to that of the fatty acid.

* The properties and composition of this acid entirely agree with those of the ethalic acid which MM. Dumas and Stas obtained by the action of hydrate of potassa on ethal.

INVESTIGATIONS CONCERNING ELAÏDIC ACID.*

BY M. MEYER.

M. Poutet, a distinguished chemist of Marseilles, showed, some years ago, that olive oil becomes solidified by its contact with the deutonitrate of mercury, and, relying on this fact, he established a process for detecting the adulterations of olive oil with other oils.

Since then, MM. Lescallier and Boudet have demonstrated that other oils are susceptible of a similar conversion by the action of the deutonitrate of mercury: it therefore became necessary to renounce this mode of testing the purity of olive oil. (For a very good practical mode of detecting this adulteration, see *The Chemist*, No. VIII. August, 1840.)

In investigating this question, M. Boudet showed that the solidification of this oil must be attributed to neither the nitric acid nor the oxide of mercury, but to the nitrous acid which nitrate of mercury commonly contains; and he likewise proved that the deutonitrate, when free from this acid, does not in any way solidify oils. The same chemist also recognised, on this occasion, the formation of a peculiar body, to which he gave the name of *elaïdine*, and which he regarded as a combination of a new acid with glycerine. He ascertained that the fat oils are caused to become concrete by the action of nitrous acid, while

* *Annales de Chimie et de Physique*, Août, 1841.

the siccative oils do not undergo any alteration.

M. Laurent has lately resumed the examination of elaïdic acid, and has published the analysis of it.

He prepared this acid according to M. Boudet's process, and purified it by separating the margaric acid in the state of bimargarate of soda, crystallizing the elaïdic acid several times in alcohol, until it attained the melting point of 42° C. The analysis of this acid led him to the following formula:—

$$C^{70}\ H^{136}\ O^{8}.$$

By deducting from it the elements of two atoms of water, he found for the anhydrous acid the composition—

$$C^{70}\ H^{132}\ O^{6}.$$

In a subsequent work on the ethers of the fatty acids, M. Laurent found that the atom of elaïdic acid was double what it ought to be, and that it required to be diminished, so that the anhydrous acid became

$$C^{35}\ H^{66}\ O^{3}.$$

During some very extended investigations concerning the fatty bodies, made in Professor Liebig's laboratory, I resumed the study of this acid.

To prepare it, I followed the process pointed out by M. Boudet, putting very pure olive oil in contact with deutonitrate of mercury, and pressing the solidified mass between folds of blotting paper. I thus purified it from a small quantity of a liquid oil which adhered to it.

The substance thus obtained presented a yellow tint, owing to the presence of a peculiar oily matter. M. Boudet removed this by means of alcohol and by repeated pressure. This process did not succeed very well; I therefore substituted for it the following. By heating impure elaïdine with ether, this solvent removed a great quantity of that substance, acquiring a fine red tint; at the same time, a grey powder composed of metallic mercury was separated, which I removed by filtration.

The ethereal solution, cooled to 0° C., deposited a white, crystalline matter, of a pappy appearance. Washed several times in cold ether, it became exactly like pure steärine, obtained by treating beef fat by ether.

The uncrystallisable supernatant liquors separated, after resting some time, into two layers, one of which was lighter, more ethereal, and less colored, and the other, less fluid, had a deep red color. I separated them by means of a pipette. The lightest liquid left by evaporation a small quantity of impure elaïdine. The oleaginous and heavier liquid was first heated in a sand-bath until the ethereal odor entirely disappeared, and was then exposed to some degrees below 0°. At the end of some hours, it deposited crystals of elaïdine, which I separated from the liquid part by pressing them between folds of blotting paper.

The liquid which flowed from the press was transparent and of a red color. By a second treatment with deutonitrate of mercury, it solidified at the end of some time; but this solidification was not so complete as in olive oil submitted to the same treatment: treated by ether, with heat, the solidified mass deposited a white crystalline matter; the supernatant liquid was turbid, and did not become clear by repeated filtration.

I likewise tried to prepare elaïdine by passing into olive oil the nitrous vapors disengaged by a mixture of nitric acid and starch. The solidification was by this means effected in a very rapid manner; but it is necessary to avoid an excess of acid, for, instead of a concrete product, a mass little less fluid than the oil is obtained, which does not solidify.

Elaïdine prepared by either of these processes is essentially identical; in the pure state, it is perfectly white, dissolves in ether in almost all proportions, and is almost insoluble in alcohol. The alkalis saponify it without turning it yellow. It melts at 32° C.

Submitted to analysis this substance gave me results which do not lead to any rational formula, which will easily be understood, this substance being, as I have ascertained, a mixture, in variable proportions, of elaïdate, margarate, and perhaps also oleate of glycerine—a mixture from which the margarine cannot be separated.

Submitted to distillation, elaïdine gives a substance which has a very suffocating odor, the *acroleïne* of Berzelius, as well as elaïdic acid, hydrocarbons, and probably also sebacic acid. However, the formation of the latter remains involved in some doubt, since I could not obtain it in the crystallised state, nevertheless, the boiling water with which the distilled product was treated, gave, with the salts of lead and mercury, precipitates which resembled those furnished by pure sebacic acid.

By boiling with the alkalis, elaïdine is decomposed into glycerine, and an alkaline margarate and oleate. If the soap thus obtained be decomposed with dilute sulphuric or muriatic acid, with heat, the mixture of margaric and oleïc acids separates under the form of an oil which floats on the surface, and which concretes and solidifies by cooling. The melting point of the impure acid thus obtained is about 38° C.

By crystallising elaïdic acid several times

in alcohol, and by removing each time the portions which crystallise first,—margaric acid being less soluble than elaïdic,—it was impossible to obtain an acid whose melting point was 44°, as M. Boudet indicated. I found it constantly at 42°, and it remained the same after further crystallisations. Hence I concluded that this acid was pure, since it possessed exactly the same melting point as the acid analysed by M. Laurent.

With this idea, I prepared the salt of silver by double decomposition. The precipitate was collected on a filter and preserved from the contact of the light, while being washed, by means of a black paper: it was dried in a sand-bath.

Two combustions of this salt led me to the following numbers for the atomic weight of the acid:—3327 — 3316·7. Although these determinations perfectly agree, the salt on which they were made gave by combustion results which it was impossible to make coincide either with the composition of the free acid, or with the composition found by M. Laurent, although these results coincide very well with each other.

It is evident, therefore, that the acid obtained by the process indicated is not a pure substance. I therefore had recourse to another mode of preparation, and I passed nitrous acid into oleïc acid. The liquid was very little colored, and, after passing the gas into it for about five minutes, I surrounded the vessel with cold water. In half an hour the oleïc acid was completely solidified and crystallised in fine laminæ. I treated the mass several times with boiling water, in order to remove all the nitric acid taken in by the current of gas, or formed by the reaction of the latter. The water acquired a light yellow tint; its taste was slightly acid. Saturated by ammonia with great care, it gave a yellow precipitate with acetate of lead, and a white, light precipitate with the protonitrate of mercury.

The acid separated from water was dissolved in its own weight of alcohol, and left to settle. The next day the liquid was traversed in all directions by fine pearly tabular crystals. Separated by filtration from the reddish supernatant liquor and redissolved in water, these crystals were obtained in the state of perfect whiteness.

In the course of my investigations, I have continually made use of the oleïc acid purified by Varrentrapp's process, and I prepared elaïdic acid by passing into it nitrous vapors. The elaïdic acid obtained was dissolved in alcohol, and left to crystallise.

Prepared by this method, elaïdic acid melted between 44° and 45° C; it readily dissolved in alcohol, and was deposited from concentrated solution in superb crystals, resembling benzoïc acid. It dissolved less easily in ether. The solutions possessed an acid reaction.

Submitted to distillation, the elaïdic acid passed over partly without alteration; part was decomposed into carburetted hydrogen. The impure acid gave fine crystals of sebacic acid. By distilling, on the contrary, pure elaïdic acid, and by treating the distilled product by boiling water, I could not extract from it any crystalline product; however, the salts of lead and mercury occasioned in the aqueous liquid precipitates resembling those of sebacic acid.

By treating elaïdic acid by a great excess of potassa, at a high temperature, a peculiar acid was obtained, fusible between 60° and 62°, and which is identical with that obtained by M. Varrentrapp, by submitting oleïc acid to the same treatment. When this acid was boiled with alkaline carbonates, a slight effervescence was produced, owing to a disengagement of carbonic acid gas, and a thick and limpid soap was formed. The soap of soda, dissolved with heat in alcohol, gave, by cooling, crystals of the neutral elaïdate, of a silvery lustre, resembling those of the free acid, only being much finer. By dissolving the neutral elaïdate of soda in alcohol, and by decomposing it by a neutral solution of nitrate of silver, a white and bulky precipitate of elaïdate of that base was obtained. This salt was easily dissolved in ammonia. The ammoniacal solution, left in a cool place for some time, deposited the greater part of the salt of silver in the state of small, white, prismatic crystals. The salts of lead and baryta were obtained, like the salt of silver, by double decomposition.

I prepared elaïdic ether by passing a current of muriatic acid gas into an alcoholic solution of elaïdic acid; the ether produced was agitated with water to remove the muriatic acid, and then with alcohol, to separate from it any elaïdic acid not converted into ether, and finally with water, to free it from this alcohol. Dried *in vacuo* this ether was presented under the form of an oily liquid, colorless and inodorous when cold, and possessing only a very slight odor when heated. It is lighter than water, insoluble in that liquid, and soluble in alcohol and ether. It is decomposed by distillation.

Submitted to analysis, crystallised elaïdic acid gave me numbers which led to the formula

$$C^{72}\ H^{136}\ O^{7}.$$

Four combustions of elaïdate of silver gave as a mean the number 3419·5 for the atomic weight of the acid.

The elementary analysis of this salt showed that elaïdic acid is a bibasic acid,

and that its atomic weight should be doubled. The formula deduced from analysis is

$$C^{72}\ H^{132}\ O^{6},\ 2Ag\,O.$$

Hence the composition of the anhydrous acid is

$$C^{72}\ H^{132}\ O^{6}.$$

I likewise analysed the elaïdates of soda baryta, and lead, which confirmed the above formula.

The analysis of elaïdic ether gave numbers which lead to the formula

$$C^{73}\ H^{144}\ O^{7},$$

which may be decomposed in the following manner:—

$$C^{72}\ H^{132}\ O^{5},\ C^{4}\ H^{10}\ O + H^{2}\ O.$$

The great difference which exists between my results and those of M. Laurent may be easily explained. That chemist probably operated on impure substances; while those which I used in my experiments were, for the most part, crystallised, and no doubt as to their purity can exist. However, the formation of elaïdic acid still remains unexplained. It is only the oleïc acid of oily bodies that furnishes elaïdic acid under the influence of nitrous vapors. Margaric acid is not modified by this means.

According to M. Laurent, oleïc acid is converted into elaïdic acid, by acquiring one atom of oxygen; this interpretation presents little probability, inasmuch as the composition which that chemist assigns to oleïc acid is incorrect, for he analysed an acid submitted to distillation; and Varrentrapp's experiments have shown that it is decomposed under such circumstances.

The conversion of oleïc ether into elaïdic ether by the deutonitrate of mercury cannot be quoted as an argument in favor of this hypothesis. It is not shown, however, that elaïdic ether is produced only at the expense of the oxygen of the nitrous acid. Indeed, by treating by an alcoholic solution of potassa the product of the action of deutonitrate of mercury on oleïc acid, and by decomposing that solution by means of dilute hydrochloric acid, we obtain, it is true, by cooling, a concrete acid whose crystalline form agrees with that of elaïdic acid; but, at the same time, this acid is impregnated with a peculiar red body; it is precisely this red matter which, I think, will one day furnish us with a clue to the theory of the formation of elaïdic acid.

ON THE LIGHT WHICH APPEARS DURING CRYSTALLISATION.*

BY H. ROSE.

The production of light during the crystallisation of certain salts has long since been noticed; but this phenomenon has been only accidentally observed; it could not be produced at will. I showed† that a hot solution of vitreous arsenious acid in hydrochloric acid gave, while cooling, very beautiful appearances of light, at the moment of the formation of the crystals. I found that arsenious acid in its opaque modification, or that which is deposited from a solution of vitreous or opaque arsenious acid, did not present this phenomenon when dissolved in hydrochloric acid. From these remarks, I concluded that the production of light was owing to the arsenious acid passing from the vitreous to the opaque modification.

Several authors have already announced that they have sometimes observed light during the crystallisation of sulphate of potassa. But it is only after a great number of fruitless efforts that I have been enabled to produce this phenomenon at will. By dissolving in hot water the crystallised or recently fused salt, I have never been able to observe the phenomenon of phosphorescence. I was not surprised at this, because fused sulphate of potassa itself is perfectly crystallised, and presents the same cleavage as the salt crystallised in the humid way. The sulphate of potassa is obtained in the state of a vitreous mass, without any appearance of crystallisation, when it is fused after having been mixed with sulphate of soda. The mixture is much more fusible than either of the component salts.

A mixture composed of one equivalent of each of these salts fused in a platinum crucible, gave a vitreous mass, which split in all directions while cooling; which at first gave rise to the belief in a crystalline texture, which, in reality, does not exist.

The fused mass was dissolved in boiling water, and the liquor was filtered as quickly as possible. The solution left to cool slowly in the dark gave the same luminous appearances as the solution of vitreous arsenious acid. The formation of each rudiment of crystal is indicated by a spark.

The crystals which are thus obtained do not present a similar phenomenon when redissolved.

The crystals which are formed with light, being removed from the solution, again become phosphorescent when rubbed, or even when slightly pressed. But, in this case, the light is weaker than that observed during the crystallisation. At the end of some hours, the crystals no longer become phosphorescent by friction. The crystals of arsenious acid which are deposited from a hot

* *Annales de Chimie et de Physique*, Octobre, 1841.

† *Annales de Chimie et de Physique*, Vol. LXI. p. 288.

solution of the vitreous acid in hydrochloric acid retain this property much longer.

The crystals of sulphate of potassa which are formed in this case present, however, the ordinary crystalline form of that salt; so that the production of light must be attributed to the same cause that gives rise to phosphorescence during the crystallisation of arsenious acid. Sulphate of potassa fused with sulphate of soda is in the vitreous state. The salt, dissolved in boiling water, separates in the crystallised state only when it is deposited during cooling.

To observe the phosphorescence of sulphate of potassa during crystallisation, it is necessary to dissolve the fused mass in water, a short time after its cooling; if the solution be made only four and twenty hours after cooling, no light is perceived except with a few crystals, and if the solution he delayed for several days, no phosphorescence at all is presented; the fused mass then appears to have passed to the crystallised state.

In those experiments which did not give light during crystallisation, it was easily perceived that the salt which was deposited from the liquor presented a different aspect than when phosphorescence took place during crystallisation. In the latter case, no hydrated sulphate of soda was observed among the crystals of sulphate of potassa, or, if there were any, only a few, which were very slowly deposited. When the phenomenon of phosphorescence has not taken place, it is always observed, that there is deposited with the sulphate of potassa a large quantity of hydrated sulphate of soda.

A great numher of experiments have shown me that the crystals which are deposited with phosphorescence are not formed of pure sulphate of potassa; that they contain a very considerable quantity of sulphate of soda, in the anhydrous state; and that it is a true double salt, which presents not only the same crystalline form as the sulphate of potassa, but likewise most of its physical properties.

In several experiments, I found the douhle salt to be composed of two atoms of sulphate of potassa and one atom of sulphate of soda; in others, on the contrary, I found the proportions of three atoms of sulphate of potassa and two atoms of sulphate of soda. I will not decide whether the salt which is deposited with phosphorescence has a definite composition, and whether the differences in the analyses are owing to the definite douhle salt heing mixed with pure sulphate of potassa, or, indeed, whether the two sulphates, like two isomorphous hodies, are capable of combining together in any proportions.

Thus the phenomenon of phosphorescence in this case seems to he owing to a douhle sulphate of potassa and soda,—not to pure sulphate of potassa,—in the vitreous state after fusion, which dissolves, retaining the same state, and which passes into the crystalline form only at the moment when it separates from the liquor. The phosphorescence is therefore always presented under the same circumstances as with arsenious acid.

But as the two salts which form the double sulphate comhine by virtue of only a very weak affinity, it often occurs that they separate in the solution, and that they crystallise when thus isolated, one in the anhydrous, and the other in the hydrated state. But when this happens, phosphorescence is not observed at the time of crystallisation.

The sulphate of potassa of commerce now almost always contains sulphate of soda, as I have ascertained hy many analyses. This suhstance is evidently in the state of a douhle salt. In this douhle salt, the potassa is isomorphous with the soda, which is not ordinarily the case, for native anhydrous sulphate of soda, (Thenardite) has not the same crystalline form as the sulphate of potassa. It appears that when a salt of potassa is mixed with a salt of soda formed hy the same acid, the comhination always takes the form of the salt of potassa when the liquor contains a greater number of atoms of this salt than of that of soda.

The double salt of potassa and soda is also produced under other circumstances than by the fusion of a mixture of the two sulphates: thus, it is obtained by fusing the sulphate of potassa with chloride of sodium: it would appear that in this case the double salt is formed more easily than hy the fusion of the mixture of the two sulphates. The phosphorescence is more regularly presented, and crystals of hydrated sulphate of soda are not deposited.

The same result may he arrived at by fusing together sulphate of potassa and carhonate of soda, or chloride of potassium with sulphate of soda.

I ascertained, hy a great numher of experiments, that the sulphate of potassa never emits light during its crystallisation when it has been fused with salts which do not contain soda.

Equal atomic weights of neutral chromate of potassa and of anhydrous sulphate of soda gave, after fusion, a mass which acted like that obtained hy fusing the two sulphates. The substance, dissolved in warm water, gave, while the liquor was cooling, with the emission of light, yellow crystals presenting the form of chromate of potassa, which is, as is known, the same as that of

the sulphate. Analysis showed me that these crystals contained sulphuric and chromic acids, potassa and soda. The oxygen of the bases was one third of that of the acids.

Moreover, the double chromate of potassa and soda, not containing sulphate, presents a very vivid phosphorescence. This salt may easily be obtained by fusing together bichromate of potassa and carbonate of soda. The double salt, crystallised with an emission of light, presented the same crystalline form as sulphate of potassa, and it was found to be composed of one atom of chromate of soda, and three atoms of chromate of potassa.

The high price of selenium prevented me from making so great a number of experiments on the seleniates as on the sulphates. Pure seleniate of potassa did not present phosphorescence during crystallisation any more than the pure sulphate. A mixture of one atom of seleniate of potassa, and one atom of sulphate of soda having been fused, the mass boiled with water gave, during the cooling of the liquor, a very brilliant phosphorescence, and crystals of the same form as sulphate of potassa. These crystals were composed of selenious and sulphuric acids, potassa, and soda.

The phenomena of light which are manifested during the crystallisation of certain bodies are owing, as is evident from the above, to a salt passing from one state into another isomeric state. These transitions are very often accompanied by phenomena which seem to be of the same nature as the phosphorescence during the crystallisation of certain salts: among the most common of which may be mentioned the incandescence undergone by certain oxides, — oxide of chromium, titanic acid, &c. &c.,—or certain minerals, as gadolinite. Before this incandescence, these substances are easily soluble in, or attackable by, the acids, and, after it, they become insoluble, or, at least, they are dissolved only with considerable difficulty.

In the two isomeric states of arsenious acid, a difference is found in the density and in the solubility. The minerals which I have just mentioned likewise present a difference in density before and after incandescence. But the density is not always, as might be supposed, greater after incandescence than before. This circumstance led me to ascertain whether there is a disengagement of heat during crystallisation, as well as during the incandescence which certain minerals present. In none of my experiments could I perceive a disengagement of heat either during the crystallisation of arsenious acid, or during the incandescence of oxide of chromium.

OBSERVATION OF A CLAP OF THUNDER, ACCOMPANIED BY HISSING.—GENERAL EXPLANATION OF THE NOISE OF THIS METEOR.

BY M. TESSAN, CIVIL ENGINEER.*

In the night of the 18th to 19th of September of last year (1840), towards midnight, being on the road from Avignon to Remoulin, not far from the latter town, the carriage in which I was travelling sunk into and remained fixed in the mud, at the top of an ascent, and there was assailed by a violent storm, which passed and repassed three times over the spot occupied by the carriage. The flashes of lightning, of a terrific brilliancy, succeeded each other with extreme rapidity, and were almost instantly followed by tremendous claps of thunder, which were themselves succeeded by deluging showers.

I prudently divested myself of all the metal I chanced to have about me, under the anticipation that the carriage might be struck by the lightning, both on account of its elevated position, and of the great quantity of iron that entered into its construction. On a sudden, a flash of lightning much more vivid than the preceding, gave rise to a very powerful hissing sound, accompanied by a frightful crash of thunder. The flash, the hissing sound, and the crash, appeared to me simultaneous, as well as to the two individuals who were with me in the *coupé*; I think, however, that it is precisely in the above order that the sensations succeed each other. I doubted not that the carriage had been struck; but, as it rained in torrents, I unfortunately could not verify the fact.

However, no damage to the carriage could be perceived when the baggage of the travellers was unpacked at Nîmes. Did the lightning find in the carriage a sufficiently good conductor? or rather did it strike some neighbouring tree? I cannot say. The newspapers, however, of the locality, (Avignon, Nîmes,) in giving account of the abundant devastation caused by the storm, in every instance alluded to the lightning's having struck many points, in the neighbourhood of that which we occupied.

This observation of the hissing sound of the thunder appears to me very important, inasmuch as it comes to the support of the objections (already very strong ones) which

* *Comptes Rendus de l'Académie des Sciences*, May 5, 1841; and *Proceedings of the London Electrical Society*, Part III.

the theory of Robert Hook raises with respect to the noise of thunder, . . .

Most philosophers, and M. Poisson in particular, in his *Mathematical Theory of Electricity*, admit, that it is the *mutual repulsion* of the particles of electric fluid which induces them to arrange themselves at the surface of conducting bodies ;—that the molecules of these bodies are no further concerned in the phenomenon than by the *free circulation* they allow to the electricity ;—that the molecules of conducting bodies exercise directly neither attraction nor repulsion upon the particles of the free electric fluid ;—that there is no adherence between the fluid and them. They admit that electricity is retained at the surface of conducting bodies only by the resistance which the surrounding non-conducting body (dry air) opposes to the indefinite expansion of electricity.

It follows from this that the conducting body is no further concerned in the distribution of the electric fluid which occurs at its surface, than by the form which this surface makes the surrounding non-conducting body assume ; that nothing would be changed if we were to succeed in annihilating the conducting body, simply preserving the form of its surface. The distribution of the electricity would remain the same, as well as the pressure which it exercised, from within, outwards upon the surrounding non-conducting body, which [latter] could not restrain its expansion but by opposing from without a pressure inwards equal to the electric pressure exercised in the contrary direction.

If, then, non-conducting bodies, of precisely the same form, are placed in dry atmospheric air, at the ordinary pressure, and one of them is electrized, this latter will experience at each point of its surface a less pressure than will the other, which remains in the neutral condition. It will be less by all the force with which the electricity presses the air, on these same points, from within outwards.

The atmospheric pressure which supports the electrized body must then be considerably diminished ; it may even become nothing, which is the case with points, whence electric sparks escape. Since there is no action, nor reaction, nor adherence between the electric fluid and the molecules of the conducting body, it follows that the surface of this body is actually relieved from all the part of the atmospheric pressure which the electric fluid supports.

This diminution of pressure produces no very sensible effect upon ordinary conducting bodies, which are all either solid or liquid ; but it must of necessity produce very great effect upon a gaseous conducting body, such as a cloud. In fact, such a conducting body must needs dilate indefinitely in proportion as it is electrized ; since the pressure, to which it is subjected externally, *diminishes*, in proportion as the electric charge *increases*, and as hence the elastic force of the interior conducting gas is no longer kept sufficiently *in equilibrio* by the exterior pressure. The dilatation of the conducting gas must be always equal to its own elastic force, added to the force of expansion of the electric fluid,—that is, in each point, equal to the exterior atmospheric pressure.

There can be no doubt that a cloud is an exceedingly good conducting body, since the hygrometer maintains itself in it at about 100 degrees ; nor can there be any greater doubt that the surrounding transparent air is a bad conducting body, since it permits the accumulation of considerable quantities of electricity in clouds.

The clouds must dilate, (and that sometimes to an enormous extent,) when they become electrized. From this dilatation must result *cold*, and a very sensible diminution of *density ;* two things very useful for the theory of *hail*, and the *suspension* of clouds. Dilatation of necessity taking place at the points where the electric charge is greatest, and electricity carrying itself principally towards points, it must hence result that the cloud will always elongate itself in the same direction, which may very reasonably be the cause of the pyramidal form of storm clouds, and of the formation of tranquil tornadoes.

If a cloud is charged with electricity, almost to the point of giving sparks, its dilatation will be very considerable ; since, in certain points, the force of expansion of the electricity will be almost equal to the pressure of the exterior air, and since consequently the proper elastic force of the cloud will be almost nothing. If, under these circumstances, a powerful electric spark actually passes, the cloud will be, at least partially, discharged of its electricity, and the exterior air, being no longer retained by the expansive force of the electric fluid, which kept it in equilibrio, will rush from all parts towards the cloud, especially towards the point whence the spark darted, which must produce, *in the region which it occupies, a very strong and deep noise*, and determine also a great precipitation of vapor. Is not this the cause of the noise of thunder, and the torrent of rain which follows it ?

The electric conditions of the different clouds which compose a storm being closely connected (*solidaires*) with each other, the discharge of one of them must often bring

on the discharge of many others, more or less distant. Besides, the differences of density between the exterior air and that of the clouds being very considerable, on account of the dilatation of these latter, there must result from this, in the region which they occupy, very powerful reflections and refractions of sound, which will cause both the flashes of lightning and the rolling of thunder.

This explanation of the noise of thunder thus appears to me perfectly to accord with the five qualities which the ear recognizes in it, (distance, direction, character (*timbre*), intensity, and tone;) and as it is based upon one of the properties of electricity most universally recognized, it seems to me perfectly admissible.

If a great extent of air, not situated in the region of the clouds, but at the surface of the earth, should be found in an electric condition, analogous to that of a storm cloud, (which may happen without the temperature of this air being disturbed, since it is sufficient that it be highly charged with the vapor of water in solution,) and this mass should suddenly lose its electricity, the surrounding dry air will rush from all parts towards the space occupied by the expanded moist air; and hence, I think, will result a true hurricane-whirlwind.

The electric condition of the gaseous masses of the atmosphere, rendered more or less conductors by the visible or invisible vapor of water, seems to me of necessity to occupy an important place in the causes of accidental winds, by the sudden and considerable variations of pressure and of density which must result from it.

MALARIA AT THE MOUTHS OF TROPICAL RIVERS.

To the Editors of The Chemist.

Gentlemen,—

As a constant reader of your valuable journal, though not one of the craft, it is with some diffidence that I hope for a place in your pages. My sole object, however, being to excite the attention, and call forth the pens of those who are better qualified by practical education to enlighten the public mind, my humble hints may elicit some useful information on the subject with which I have headed my letter.

Having read with considerable interest the account of the Analysis of the Waters of the Coast of Africa, &c. by Professor Daniell, given in your second volume, p. 72, I could not but feel deeply impressed with the obligations which humanity would be under to science, were her admonitions oftener and more practically followed.

No man of reflection can fail to be struck with alarm at the lamentable tone of the accounts recently received of the probable failure of the "Niger Expedition," at its very ENTRY into the field of its destination. The first thing that recurred to my mind was the paper drawn up on the subject by Professor Daniell. Are the labours of scientific investigators pursued merely for the sake of recording them in philosophical journals—there to repose without any practical applications? Yet, consider how often this is the case with valuable expositions of natural phenomena in your own journal!

A leading daily paper, commenting on the political inexpediency of the dispatch of this expedition to the shores of Africa under ANY circumstances, incidentally takes occasion to scout the idea of malaria at the mouths of rivers, &c.; the Editor must have had a professional prompter, surely, for that assertion; otherwise, must we say, "*ne sutor*," &c.? Now look at the following, from the *Bombay News*, recently arrived:—

"The causes of the disease which produced such fatal effects among the men of the Queen's 17th Regiment, when quartered at Colabah, and rendered it expedient for that regiment to be sent to sea, have been investigated by a committee of Bombay medicals, who ascribe the extensive mortality to 'malaria of a most deadly nature, generated by the salt water coming in contact with the stunted trees which grow between high and low water mark on the western shores of the island.'" !!!

I trust I have now stated sufficient grounds to call upon some of your intelligent correspondents for an explanatory discussion of this matter; for who shall decide when doctors disagree? If my remarks shall be doomed to pass *sub silentio*, let it at least be recorded to the honor of chemical science, that though in one case, her warnings being disregarded, disaster and death ensued; yet in the other science and humanity prevailed in the restoration of health and safety.

Yours most respectfully,

Aliquis.

LONDON ELECTRICAL SOCIETY.

Tuesday, Jan. 18th.

The secretary communicated to the Society an account of a method of exciting voltaic pain, so as to produce a constant action, which was adopted some months since by Dr. Leeson, and which is attended by several advantages. It consists in making

a solution of bichromate of potassa, and adding to it sulphuric acid. Amalgamated zinc and copper, with this solution, and without a diaphragm, are very effective for electrotype purposes; they have been successfully employed in the practical applications of the art. There are several interesting particulars, connected with the action of this solution, which will be laid before the Society as soon as possible, after they have been more fully developed.

The other papers read were, "Further Account of the Hydrostatic Galvanometer," by Mr. Iremonger. "Apparatus for regulating the Voltaic Current," Prof. Jacobi, and Mr. Weekes's "Register" for December.

"ELECTRO-LACE."

TUESDAY, FEBRUARY 15th.

The secretary communicated to the Society some recent additions to the manipulation and application of electrotype. By means of a leading wire, abutting upon any portion of a plumbagoed mould, on which the copper has delayed to deposit, this object is readily effected; and considerable facility is, by this simple means, given to the manipulation with wax and composition moulds.

Some specimens of "electro-lace" were then submitted to the society, and excited considerable interest, from the very perfection of their metallic appearance; the imagination could almost conceive that the thread lace had, by a magic touch, been converted into metal. They were produced by covering lace or net with wax and plumbago, and then depositing on it copper, and were prepared as negative plates, for the "Improved Smee's Battery," described in Electrotype Manipulation, by an experimentalist, in Cornwall, who was unable to obtain copper gauze for that purpose. But this beautiful application of the art is not limited to this solitary case; for all the delicate fabrics of lace and gauze may in their turn be used, as the base of deposits, and, after being silvered or gilded, will afford a material admirably adapted to the construction of fancy-work, baskets, &c. &c.

Among the other papers read was one from Geo. Mackrell, Esq., which promises to lead to interesting results. From the description given at the last meeting of the bichromate of potassa battery, this gentleman made a series of experiments, using various exciting liquids for the copper element, and has found that nitrate of soda or salt, used for the first time, produces an action more constant than that from the bichromate, or even the sulphate of copper. The tabulated results of the experiments are well worth examining; and lead to the conclusion that this mode of exciting the battery will, especially for electrotype purposes, be very generally adopted.

The other papers were—one from Mr. Pine, on the "Connection between Vapors and Vegetation;" and Mr. Weekes's Register for January.

REVIEWS:—

Letter to the Right Honorable George, Earl of Aberdeen, K. T., &c. &c., Principal Secretary of State for Foreign Affairs, Chancellor of the University and King's College, Aberdeen, On the State of the Schools of Chemistry in the United Kingdom. By WILLIAM GREGORY, M.D., F. R. S. E., M. R. I. A., Professor of Medicine and Chemistry in the University and King's College, Aberdeen. London: Taylor and Walton, Upper Gower Street.

THE foregoing letter contains much valuable matter. We perfectly agree with the able author, that the difficulties which surround the pupil in his endeavors to acquire a perfect knowledge of Chemistry in this country are so great, that many are deterred from its pursuits, or are driven to study at the schools of France and Germany. Perhaps few journalists have taken such pains or have devoted so much time as ourselves to the improvement of the method of education, and particularly in the science of Chemistry. Like Dr. Gregory, we place but little confidence in lectures. It is in the laboratory and by frequently repeated demonstrations alone that students can learn all its facts and doctrines.

We are, indeed, well aware of the want of facilities and economy in prosecuting this most important study. With such conviction deeply implanted on our minds, we opened a School of Chemistry, where, after a lecture of the usual duration, demonstrations and examinations are carried on, upon a system of instruction completely ensuring a thorough knowledge on the part of the pupil, and on a scale of economy bringing the attendance easily within the power of every one wishing to join the class. We much regret to say that, with the exception of a few gentlemen connected with chemical manufactures, none seemed disposed to attend; and of all classes, those who by business and pursuits are or ought to be especially interested in the science, we mean Chemists and Druggists, evinced the least desire. This circumstance cannot fail to convince the writer of the above letter that but little love of science actuates this class of men, and that some act of the legislature

is necessary to impose a greater extent of knowledge for the good of the public. As a proof of the value of this publication to those interested in tbe propagation of Chemical Science, we may quote the following passage:—

"It is generally in fear and trembling that they follow my advice, which is, to devote their whole attention, not to imitating manufacturing processes in the laboratory, but to learning how purely scientific problems are to be resolved. Their intellect soon and easily learns how to find the best means: it is they themselves who modify and shape their means according to circumstances. Every operation, every analysis, which serves to clear up a given question, or which must be performed in order to discover the conditions essential to the resolution of the problem, has a specific object. Each process thus acquires a certain charm tbat effectually wards off fatigue; and when the problem is once resolved, the student has learned the means of solving all similar problems. I am acquainted with many of my former pupils who are now at the head of every variety of chemical manufacture. Without having ever practised these in the laboratory, they became, in the first half hour, perfectly familiar with the whole process; and the next half bour commonly produced a number of well-devised improvements in the manufacture. They had acquired in the laboratory the most exact knowledge of the properties of the materials which they had to employ, and were accustomed, as the only way of avoiding errors, to subject the products of chemical reactions to a searching investigation in regard to their composition; and they thus at once discovered the sources of error, the means of avoiding loss, and the best methods of improving the apparatus, or perfecting the process. All this is not learned, when the student enters a laboratory for the purpose of practising a given process by recipe."*

We are bound to admit the truths stated so fully, particularly as to the want of facilities and the expense of apparatus.

Dr. Gregory has, in his statement, treated of so many important subjects, and so fully and ably set forth all the desiderata, that we must refer the reader to the original as the best source whence to derive information so important and conclusive.

II. CHEMICAL MANUFACTURES.

NOTICE OF A NEW MODE OF GILDING METALS BY THE HUMID WAY AND THE VOLTAÏC CURRENT.

BY M. LOUYET.

[We are indebted to M. Louyet for a pamphlet, in the French language, on the above subject, which it is our intention to divide into two Nos., as the translation would occupy more space than we can spare in one.]

Among the arts and occupations which should attract the attention and solicitude of science, as well with respect to their pernicious effects on the workmen who practice them, as considered in an economical point of view, that of the gilders by fire and mercury occupies a conspicuous place.

Now, it is only by a complete renovation of the processes of gilding at present generally employed, that we can hope to bring about any real amelioration of this dangerous state. However valuable, indeed, the furnaces invented by M. D'Arcet may be, in preserving the working gilders from the mortal influence of the vapors of mercury, we see that the greater number of these artisans persist in not employing it, whatever advantage they might derive from it as regards health and economy. It is because *progress* has always been very difficult to instil into the ignorant and inferior classes, as a substitute for established routine; and, when danger is not immediate, when the evils arising from deficiency of precautions do not suddenly manifest themselves, but proceed by a slow and continuous action, which attacks and silently consumes the principles of life, it is almost impossible to persuade workmen to avoid the causes of disease, by employing simple precautions.

A substitute for the process of gilding by fire has long been a desideratum; most of the attempts which, hitherto, have given results sufficiently satisfactory to give any hope of the possibility of arriving at the desired object, namely, the complete abandonment of mercury, were made with gold held in solution by different vehicles, which has caused all these processes to pass under the general denomination of *processes by the*

* *Ueber den Zustand des Chemie in Preussen*, by Dr. Justus Liebig, Professor of Chemistry at Giessen, 1840.

humid way. Among these, that is most distinguished which was invented, some say, by M. Bonnet, others, by Mr. Elkington, although it would appear that the priority belongs to the former, and which consists in treating a solution of bichloride of gold, by a certain quantity of carbonate of soda, or potassa dissolved in water, boiling for a certain time, and steeping objects of copper, brass, or silver, previously cleaned, in the liquid, from which they are taken out perfectly gilded. Whatever may be the superiority of this mode of gilding, which may be called Anglo-German, it appears, however, that it does not give results completely analogous to those of gilding by fire and mercury, for the latter method seems to be still used when a careful gilding is required. The Anglo-German process is seldom made use of for covering with a thin coat of gold vessels or instruments which ought not to be subjected to much friction, and in which the gilding should be looked upon only as an accessory of little importance, as well on account of the trifling thickness of the layer of gold, as from the incapacity of the gilded bodies for supporting the necessary operation, called *put in color* (*mise en couleur*), which consists in boiling them with different oxidising and cleansing salts, which purify the surface of the layer of gold, oxidising and dissolving the copper which is mixed with it. The same object may be attained by covering the surface of the gilded metal with a pulverulent mixture of nitre, sal ammoniac, sulphate of iron and verdegris, either alone, or combined with melted wax; when it is known by the name of gilding wax, and by heating it until the mass begins to fume. By the putting in color alone, we are able to give to gilded objects the beautiful color required in the arts, instead of the dull, yellow, brass-like tint which they always have, when the surfaces are not previously polished, on leaving the fire, in the mercurial process, and sometimes, when removed from the boiling bath, in the Anglo-German process.

Most of the scientific journals gave, a few months ago, the description of another process of gilding by the humid way, invented by M. De la Rive, of Geneva, and communicated by him to the Academy of Sciences of Paris. (See THE CHEMIST, No. VI. p. 180, where this process is accurately described.) M. De la Rive availed himself of the decomposing influence of voltaïc currents on the solutions of different salts, especially on those of the easily reducible metals. In this process, a glass vessel, containing water very slightly acidulated by sulphuric and nitric acids, (five or six drops in a glass of water of the ordinary size,) receives a bag of bladder or goldbeater's skin, which contains a dilute neutral solution of bichloride of gold; this bag also contains the substance to be gilded, previously scoured, and a piece of pure zinc is steeped in the acidulated water contained in the glass vessel. The sheet of zinc may be in the form of a hollow cylinder surrounding the bladder. The acidulated water may be put into the bladder; this arrangement is convenient for gilding the interior of a silver vessel. To establish communication between the object to be gilded, and which is immersed in the solution of gold, and the zinc, M. de la Rive employs a silver or platinum wire, which communicates by the medium of a thick copper wire, with the piece of zinc, which is attached by its other extremity to the object to be gilded. In this process, the electric current should be very weak; otherwise the particles of gold which are deposited on the object detach themselves as soon as they are precipitated. At the end of a few minutes the object is taken out to be washed; it is dried with a fine cloth and again submitted to the action of the electric current. This operation is repeated a third time, to obtain a very solid coat of gold. In these operations, it is necessary to collect the gold which adheres to the cloth and to the bladder, as well as that which is reduced in the solution.

This process, which was said to give very satisfactory results, could not, however, entirely substitute the gilding by means of mercury; for, like the preceding, it gives a coat of gold which does not resist the action of energetic acids, nor the putting in color.

When I was acquainted with the above described mode of gilding, I had already long been endeavoring to find out some advantageous means of doing away with the use of mercury in gilding. By repeating and varying the experiments of M. De la Rive, I have been led to that which forms the subject of the present notice. I may here remark that I had been informed that a process of gilding had been put in practice in England, where it had given very good results, and that the cyanuret of potassium was used as a solvent of the compound of gold.

Guided by these different data, I continued my experiments, which finished by giving me very satisfactory results.

I first tried to dissolve in a concentrated solution of cyanuret of potassium, different compounds of gold, such as the bichloride, the biniodide, the iodide, the oxide, and the bisulphuret: the last produced the best effects.

I will not here enumerate all the difficulties which I met with before arriving at the following process: suffice it to say, that it

is the result of a great number of experiments, in most of which, theory was of but little utility to me.

My process differs from that of M. De la Rive in the following particulars. I substitute for the bichloride of gold a solution of bisulphuret of gold in cyanuret of potassium; instead of acting on a very dilute solution, I employed a very concentrated one, having observed that this mode of operating gave the best results with the compounds in question. Finally, I caused the electricity to act directly, by the aid of voltaïc pairs, amalgamated zinc and copper, of size varying with the extent of surface to be gilded.*

Bisulphuret of gold may be prepared in the ordinary manner, namely, by passing a current of hydrosulphuric acid gas into a solution of bichloride of gold, taking care to continue the operation for a long time, in order to precipitate the last portions of metal. This compound may also be obtained in a very simple manner, by pouring into the bichloride a solution of hydrosulphate of ammonia. The sulphuret is collected on a filter, and washed several times in warm water, in order to remove a small quantity of hydrosulphuric acid, which it may retain from its preparation, and which might impede the success of the operation, especially when it is required to gild objects of silver. Care must be taken not to perform the washings with boiling water; for at this temperature, a small quantity of the bisulphuret of gold is decomposed; to dissolve the sulphuret, it may be sprinkled, even on the filter, with a concentrated solution of cyanuret of potassium,† and collecting the liquid in a glass vessel; the solution of cyanuret is poured on until the filtering paper, blackened by the sulphuret, is entirely decolored, which proves its total solution, and afterwards washing the filter with a little warm water; this washing water should be kept, in order to extract from it by burning any portions of gold which it may retain. The solution has a light golden yellow color; it is diluted with water until the tint becomes a very faint straw color; it may be preserved in close vessels for a long time without alteration. The gold is contained in it in the state of sulphuret and not of cyanuret, as might at first be supposed; for, if it be decomposed by the voltaïc current, the negative reophorus of

* It may here, perhaps, be objected that a process analogous to mine has already been published; indeed, in *L'Institut*, of August 13, 1841, No. 398, in the report of the sitting of the Academy of Sciences, we find the following paragraph:—

"M. H. de Ruolz addressed a notice of a new process of gilding and silvering all the metals employed in commerce. This process is based on the decomposition, by the aid of a powerful pile, with a constant current, of the cyanides of gold and silver, dissolved in a given proportion of cyanuret of potassium."

To demonstrate in an evident manner that my experiments on gilding by the humid way, and the voltaïc current, were prior to this notice, it will be sufficient for me to remark that I called the attention of my pupils, about eight months ago, in the course which I was then delivering at the central school, to the process of gilding, which I had discovered, exhibiting the experiments to them. Some of them even repeated these experiments. If I did not publish this process at that time, it was because I had not made sufficient trials to make it rest on certain data.

† The solution of sulphuret of potassium may be prepared in the ordinary way, namely, by calcining ferrocyanuret of potassium for three quarters of an hour in a covered crucible or stone-ware retort, luting the apertures during the cooling, treating the carbonised residue by a small quantity of cold water and filtering: or else by using the process described by Wiggers (*Annalen der Pharmacie*, XXIX. 65), which consists in introducing into a retort a mixture of two parts of ferrocyanuret of potassium with one and a half of sulphuric acid, previously diluted with one and a half part of water, and cooled; connecting the retort with a receiver containing a colorless solution of one part of pure hydrate of potassa, in three or four parts of alcohol at 90 per cent. The receiver and the retort ought to be tubulated and furnished with safety tubes. The receiver must be kept as cold as possible, and the distillation must be very gently conducted, on account of the great heat which is developed in the receiver by the condensation. When the ebullition in the retort is disturbed by swelling up, the operation is stopped; for it is a sign that most of the hydrocyanic acid is driven off, and, if it be continued, the liquor in the receiver will be diluted with water. This liquor is converted into a pap of precipitated cyanuret of potassium held in suspension by the undecomposed alcoholic solution of potassa. The precipitate is collected on a filter, and freed from the supernatant liquor by washing with alcohol; then it is pressed and dried in the same filter, on a hot plate of iron. Two ounces of ferrocyanuret of potassium treated in this manner gave six drachms of cyanuret.

copper is covered with gold, while sulphur is deposited at the extremity of the positive conductor. This solution is not decomposed by silver, copper, or brass, when steeped in them, without being submitted to a voltaïc current: I think that the permanent state of alkalinity in which this solution is found on account of the presence of cyanuret of potassium, has a favorable influence on the results.

NEW AND USEFUL APPLICATION OF THE ART OF ELECTROTYPE.

To the Editors of The Chemist.

February 8th, 1842.

GENTLEMEN,—

I take the liberty of forwarding to you an account of a new application of the electrotype, which I hope you will insert, should you think it interesting to the admirers of that beautiful art which is at present making such rapid strides,—every day bringing forth fresh instances of its important applications to the useful and ornamental arts.

Among the variety of objects which I am in the habit of copying, are the beautiful embossed backs of some of the modern bound books usually done in "cloth." The back itself is metallised in the ordinary manner, and is used as a mould to the decomposition cell,* where it should remain until the deposited copper appears to be of sufficient thickness. The process may be so modified as to produce a plate or tool capable of impressing the work with ornaments in relief, instead of intaglio, producing a novel and pleasing effect; the only variation is in taking a fusible metal cast† of the cover, and depositing the copper on it in lieu of the latter, which is thereby preserved unaltered and attached to the book. The plates were found on trial to produce on a plain cloth cover all the elegance of appearance possessed by those from which they had been copied.

I am not aware that the electrotype art has ever before been employed for the purpose above mentioned: at all events I have never either heard or read of its being so applied.

I remain, Gentlemen,
Respectfully yours,
M. J. BRAZENDALE.

* The backs or covers are consequently rendered unfit to be again used as such on the book, but they can be easily had of any bookbinder, who removes them as useless from the works, when sent to be bound in a stronger and more substantial manner.

† Wax may be substituted if thought fit, but the surface of the cast then requires to be metallised.

PORTABLE OXYHYDROGEN MICROSCOPE.

To the Editors of The Chemist.

February 9th, 1842.

GENTLEMEN,—

I have at present an extremely portable oxyhydrogen microscope, which is, I think, particularly adapted for travelling; so much so, that it may be carried with the greatest convenience under the arm. The brass work, lenses, lime-light apparatus, &c., are packed in an oblong wooden box, at the bottom of which is a drawer, sliding outwards, and intended for the reception of that part of the instrument which encloses the lime-burner and jet, the four sides being joined at the corners by sliding into grooves; the top and bottom parts being afterwards affixed thereto when required. The top part is made to fit into the interior of the lid of the packing case, and the lower or bottom part, which is a square metallic box (forming a compression blow-pipe), is placed along with the moveable sides in the sliding drawer above-mentioned. At the end of the brass work, where the object glass is placed, a plane mirror is affixed at an angle of 45°, the magnified image being thereby projected upwards instead of horizontally: the advantage of this is that the instrument may be exhibited with facility in such places as a parlor, or, indeed, in any room whatever possessed of a flat white ceiling; altogether dispensing, therefore, with the clumsy screens, frame-work, gasometers, &c. Moreover, the whole apparatus can be readily carried about in the hands from one room to another when in full action.

I am, Gentlemen,
Your humble servant,
M. J. BRAZENDALE.

METHOD OF CASTING ELECTROTYPE MEDALS IN ONE PIECE.

BY Z. J. ROCKLINE.

IN the process of casting medals by the electrotype, it has hitherto been the practice to operate on two separate moulds, one of which is for receiving the obverse, and the other the reverse, halves of the medallion, the two parts being afterwards soldered together, constituting a *facsimile* of the original. By following the method described below the object is copied at once, and in one solid piece, no kind of cement or fastening being required. The two halves of the mould are first cast in Paris plaster, and

when they have become dry and hard, rub down the superfluous edges and prominences, until they both accurately coincide, when fitted together, leaving a hollow cavity or space in the centre of the exact size and form of the original medal. Thus neatly adjusted, they should be steadily fixed in that position by means of a clamp and screw: then, with a pointed bodkin pierce a small hole in the mould, in such a manner that a wire attached to the zinc arrangement of the apparatus may be rather tightly inserted therein, and pushed about half an inch into the hollow matrix: it is then fastened in its place with a piece of melted sealing wax dropped on the outside. The mould is now complete, and ready to be set in action in a proper apparatus. On account of the small surface of copper exposed on the negative side of the circle, the action of the acidulated water on the zinc ought to be extremely weak at first, but as the deposit increases in the matrix the energy should be proportionally strengthened.

III. PHARMACY, &c.

MEDICAL REFORM.

It is with the deepest interest that we anticipate, from the announcement by Sir James Graham, of his intention to lay before the House of Commons a Bill for the better Regulation of the Medical Profession, that the removal of those abuses which have so long endangered the public health, and disgraced one of the noblest avocations which can engage the attention of civilised man, will at length be accomplished by an efficient and final enactment. To what objects ought this measure to be most especially directed? Is the present monopoly of the Colleges to be continued? Are none, however well qualified, to be admitted to Degrees in Medicine, Diplomas in Surgery, or Certificates to practice as Apothecaries, except they have been educated at particular and exclusive Schools, and these such as the examiners alone have the pecuniary benefit of, by fees for instructions by far too expensive, and scandalously imperfect, superficial, and ill conducted?

Are Apothecaries still to be allowed to exercise the art of Surgery, without any Diploma? Or, which is of far greater importance, without proper education? Are they to continue to unite the trade of the Chemist with the semi-professional occupation of the general practitioner? Are their present slender, nay, scandalously deficient knowledge and attainments to suffice for the safety of the public, and more strict examinations into their qualifications than have hitherto existed not enforced? Are Chemists to be allowed to continue in the full enjoyment of the lucrative and unrestricted exercise of counter practice, and indeed even of visiting patients, without any education beyond such as they acquire of persons of their own trade, and totally without any knowledge of Chemistry, (although they assume the name of Chemists,) Botany, Materia Medica, and Pharmacy? Is quacking still to exist in all its horrors, depravity, and filthy abominations, which perpetually contaminate the young and disgust the old? Are the demoralising and indecent advertisements and bills of the base wretches who by nature and by character are fitted for no better occupation than the dissemination of immorality and the encouragement of licentiousness, to be allowed to meet the eye of youth, and especially of females? Is the deficiency of the revenue still to be supplied through this vile and pestilential source of fraud, falsehood, and vice.

We ask these questions, important, indeed, to mankind; to a civilised, enlightened, Christian country, indispensable; and we leave the answers to these questions to common sense, and that best of all bonds of society—morality: and sincerely do we wish success to the efforts of the talented Secretary of State, who has undertaken the onerous task, and we trust that he will at once strike at the root, and not skim the surface

of the evil. Any services we can render shall cheerfully be devoted to bring about a result so necessary to the common good. In the measure in contemplation, we may expect to see introduced a thorough and final settlement of the different departments and provinces of the profession, but at the same time we must, in the most forcible manner, urge the necessity of some compulsory clause to enforce education and qualification in each class of practitioners, suited to the rank which they are destined to occupy. Our views and method of education and examination have already, in full, been laid before the public, and the more our minds are called to the subject, the more forcible become our convictions of their importance and value, to the total extinction of the ignorance, malpractice and degrading conduct with which it now teems, from the very humblest to the highest ranks of the profession.

In legislating for the two higher orders of practitioners, physicians and exclusive surgeons, there will be but little trouble or difficulty, as the objects to be gained for their regulation are but few, and are, indeed, confined to such as rather restrict and confine their powers than enlarge them. All that is required is the complete annihilation of the present monopolies, and exclusiveness of these bodies, and the admission of all properly educated and qualified persons,—whether students of any school whatever, or of no school but that of personal application, and diligent and attentive industry in the pursuit of knowledge. We trust that the enlightened functionary who has undertaken the improvement of the Medical Profession, will mow down all such impediments to the spread and increase of scientific research, as these corporate bodies have so long and so effectually placed in the way. For the medical profession to rise in value, importance, and public estimation, all degrees, diplomas and honors must be thrown open to ALL WHO ARE QUALIFIED, and not, as now, reserved for the members of some particular and select schools. All men of talent and acquirements are deserving of the rewards due to their attainments; and to debar them from such because they are not disciples of any particular places of instruction, and such instruction as we have proved they afford, is intolerant, tyrannical, and infamous. The public is interested only in the qualification of Medical men, and not in the emolument of those who try to keep to themselves the benefits of conferring Degrees and Certificates upon them. We therefore hope to see the sphere of qualification enlarged, and the monopoly of the office not only diminished but abolished. The next improvements which we are anxious to see effected in these ranks of Medical men are—compulsion to acquire a thorough knowledge of Anatomy, Physiology, Pathology, Chemistry, Materia Medica and Surgery, &c. and that the examinations by which their attainments are tested should be made in writing, and the answers given in the same form, and carried out to such an extent as thoroughly to probe the real amount of knowledge, and of the application of the pupil: these examinations should also be private, and not public, as the latter are often destructive to the success of the party in after life. With the above exceptions, little amendment can be made in these departments of the Medical Profession.

We now come to the real point of contention—the main objects of any Medical enactment—and these are the adjustment and determination of the rights of Apothecaries, or general practitioners, as they call themselves,—and those of Chemists and Druggists. That a line must here be drawn is evident; that a restriction, upon both, from any unfair encroachments upon the province of each other, is much wanted, cannot be doubted. That Apothecaries should be jealous of the extensive practice of Chemists, of their interference with Medical and Surgical cases,—and even visiting the sick, is but natural to men who have received a far higher degree of education, and devoted much of their time and money to study. Chemists, on the other hand, are equally jealous of the attempt of Apothecaries to restrict what they consider as their right to give advice, in minor cases, when applied

to; and they seem to think that for this right and privilege, which they have ever exercised without molestation, they ought not to be compelled to obtain any education or qualification beyond what they acquire as Chemists and Druggists. They are likewise aggrieved at their trade being injured by the open shop and retail of the Apothecaries.

These constitute the great objects of Medical reform, and the main obstacles to the final adjustment of the long perturbed state of the Medical Profession, and a bar to the regulation of its departments, on a basis satisfactory to all parties, and conducive to the safety of society.

The difficulty and the justice of the question of Chemists practising behind their counters appear to involve the following considerations: —The excessive charges of medical men, which totally preclude the poor from apply to them for advice, and the almost certainty that if they do and cannot pay, their doom be a prison, through the instrumentality of the Court of Requests. Hence it will be evident that they are the sole or chief cause of that evil of which they so loudly and so continually complain, and the poor are therefore driven to seek the best advice they can get from Chemists, rather than incur debts, or apply to charitable institutions or take gratuitous attendance, which means nothing; such is the state of the needy with relation to medical assistance. The open retail shops kept by Surgeon-apothecaries constitute another source of grievance between them and Chemists, whose business suffers as much as, or even more than, does that of these men, from the counter practice of Chemists; therefore, so far as justice between them is concerned, we can observe no ground of complaint. But with the public the case is far otherwise. Are persons totally without any medical education to be permitted to practise? Are the poor to be left to the charge of men ignorant of disease and its remedies, merely because of their poverty? Chemists say, that if they are prevented from giving advice, they must stand as mere ciphers behind their counters, and that they will be liable to penalty for giving a dose of medicine. Now the whole of this argument points only to one inference, that is, that without the least qualification they desire to be allowed to practise. This cannot—ought not, to be suffered. They must be compelled to obtain a due and respectable knowledge of Chemistry, Botany, Materia Medica, Pharmacy, &c. &c.; if they are to be suffered to exercise the privilege of advising in medicine, it must be with knowledge properly to do so, and not, as at present, without the least pretensions to science. By an enactment enforcing on Chemists such studies and examinations previously to their having certificates, all the difficulty is at once removed, in the only way in which it can be with justice to general practitioners, with credit to themselves and with safety to the community.

It is not without the deepest regret that we observe on the part of Chemists so little desire to increase their stock of scientific knowledge; but we have too ample evidences of the fact to be ignorant of it, and too great an interest in the welfare of the body of the trade not to comment upon so melancholy a dereliction of duty and negligence of their best and greatest interests. Little, indeed, has the constitution of the Pharmaceutical Society done to enlighten them, or arouse in them the ambitiou of knowledge and the emulation of excelling. In the course of our experience it never has occurred to us to see any undertaking so badly carried out, so misconducted in every movement, in fact, so completely ruined, as this Society has been.

When we say that there is, on the part of Chemists, no desire for obtaining knowledge, we speak because it has been made evident to us in a striking degree. A School of Chemistry in its application to Pharmacy has lately been established in the Strand, intended more particularly for the class of Chemical students, and what was the amount of encouragement given to it by those parties for whom it was instituted, and to whom it offered, of all other seminaries, such peculiar and paramount advantages, not only of lectures and demonstrations, but of daily and minute exami-

nations and explanations, calculated to insure to them a perfect and indelible acquaintance with the science? Our answer is, that our students were composed wholly of gentlemen imbued with a love of science, and such as are engaged in those arts and manufactures which are founded in Chemistry, without one single exception to the general aversion to become pupils in Chemical Science, exhibited by the whole London Trade. Was it any objection to the teachers? Was it that we had not stood foremost in their cause when they most wanted our aid, and with a sincerity and success far exceeding their own wonder-working Society? No: the carelessness on the part of masters, apprentices, and assistants, as to the increase of the knowledge of their body, is the sole and disgraceful cause. We suppose that they and their Society view "a little learning" as so "dangerous a thing," that they are determined to get none at all. We wish them to look before it is too late. Such a state of things cannot remain; and we shall be no advocates of the perpetuity of ignorance in so serious a concern as that of medicine. Thanks to the talent and sagacity of the enlightened Secretary of State, who has commenced the work of improvement, the scythe will, we hope, soon mow down the weeds and leave room for the springing up of a good and wholesome crop.

We have from the first advocated the best interests of this class, and it is with equal concern for its well being that we are solicitous of creating in them an ambition to excel in those departments which have reference to their avocations; for their sake we trust that the Legislature will interfere, and cause them to acquire that knowledge from necessity, which they, we sadly fear, are unwilling to do from choice or zeal for study.

That no delay may take place in the passing of this measure, we trust and believe, and if it effects the desired objects, we wish it every success, and are devoted to its progress, and ready to give every assistance that may lie in our power, or conduce to the perfection and completion of its various details.

CASE OF POISONING BY ARSENIURETTED HYDROGEN.

BY CHARLES O'REILLY, M.D., DUBLIN.

We are indebted to the kindness of Dr. O'Reilly for the details of this highly interesting case, which we consider so important that we give it almost entire. In previous cases of poisoning by this gas, only two of which are on record, *viz.* those of Gehler and Beard, but little has been made known as to the symptoms and pathological appearances.

"On the 23rd of October, 1841, Mr. Brittan, a druggist and chemist, aged 31, previously in good health, inhaled, at different periods, about 150 cubic inches of impure hydrogen gas, believing that if pure it would not prove injurious to the constitution, and that the only symptoms arising from it would be a change of voice, a shrillness following such inhalation; and that he might record the result in a book which he was about publishing on chemical manipulation.

"Immediately after inhaling the last portion of gas, he was seized with giddiness and faintness, followed by shivering, accompanied by an evacuation from his bowels; about two ounces were discharged without pain from the urethra; shortly after he felt pain in the lower extremities, more particularly in the right, and numbness of the superior, and afterwards of the inferior extremities, followed by a tingling sensation. On the subsidence of these symptoms, which continued for two hours, he was seized with slight pain in the loins; during this time he had constant vomiting, every drop of fluid was rejected—soda water, aërated lemonade, saline mixture, &c.; this violent vomiting continued from three o'clock, being about half an hour from the reception of the poison, until seven o'clock, P. M. When I saw him, there was nothing remarkable in his appearance; he complained of weakness (referring it to the distress occasioned by the constant vomiting), and a bitter taste in his mouth; pulse 90, but feeble; temperature of the surface lowered, and the voice whispering, which alteration, his assistant stated, took place while inhaling the gas; dull pain, on pressure, in the epigastrium; the matter vomited was greenish, and about two quarts in quantity. To allay the irritability of the stomach, and produce reaction, the following remedies were employed; a draught with opium and ammonia was directed, in orange flower

water, to be taken every third hour; the feet were immersed in warm water, and an emollient injection administered. On my return at ten o'clock, P. M., the draughts being repeated, eight leeches were applied to the epigastrium, followed by a poultice; and four drops of the black drop, with three grains of mercury with chalk, were directed to be given every second hour.

"24th. Vomiting continued every hour during the night; somnolency during the interim; the vomited matter presented the same greenish character; bowels freed by the injection; no urine; face presented a copper colour, or rather a dark reddish yellow; remainder of body greenish yellow; conjunctivæ of the same colour; white objects, however, do not appear colored; pulse 80, and strong; the slight tenderness in the epigastrium still continues, but he is free from pain; has had troublesome hiccough. Same remedies, with diluent drinks; also ten leeches to the epigastrium, and a mustard cataplasm (after bleeding had ceased) to be applied for twenty minutes.*

"*Evening.*—The vomiting greenish fluid, not so frequent, now every two hours; has passed about a table spoonful of blood from the urethra, which I did not see; is perfectly intelligent, and has been so from the commencement; does not complain of pain. Opium omitted; a blister was applied.

"25th. Vomiting less frequent; pulse 76; occasional hiccough; no evacuation from bowels; no urinary discharge; no pain in the epigastrium; an aperient mixture composed of sulphate of magnesia and infusion of roses; also a domestic enema; coffee, tea, chicken, or beef tea for drink, were ordered.

"26th. Stomach has retained mixture; jaundice disappearing; bowels freely evacuated; discharges loaded with bile; has vomited three times during the day and night; no urine. The following draughts were ordered: bicarbonate of potass, five grains; syrup of roses, one and a half drachm; water, six drachms, every third hour; cream of tartar water; the carbonated lemonade omitted; iced water as drink, with occasionally chicken broth, and wine and water.

"27th. Jaundice nearly disappeared; no fever; considerable weakness; no pain; has vomited once during the night the same colored fluid; exceedingly restless; great thirst; bowels natural; no urine; face somewhat œdematous; slight ammoniacal odor perceptible from the breath; pulse 80; somnolency during the day. To be put into a hip-bath for twenty minutes; continue diluent drinks and the following draughts every third hour: nitre, ten grains; syr. one drachm; sweet spirit of nitre, twenty drops; water, six drachms, with occasionally a glass of claret.

"28th. Has had a restless night; face slightly œdematous; tongue somewhat enlarged; on its right side a deep irregular ulcer has appeared; breath having an ammoniacal odor; no urine found in bladder, although catheterism was employed; the bowels twice moved during the night; complains of nausea and fulness in the epigastrium. At the suggestion of Dr. Graves, a liniment of vinegar of squills, and muriated tincture of iron, was freely rubbed on the lumbar region; he was also put into a hip-bath.

"*Evening.*—Somnolency increasing; loss of memory; face œdematous. At the suggestion of Sir H. Marsh and Dr. Stokes, equal parts of lime water and milk were given; wine occasionally.

"29th. Nine o'clock, A. M., has slept occasionally; bowels seven times moved; an exceedingly small quantity of urine has passed, depositing a little blood; has taken only a pint of drink; has had a more comfortable night; pulse 102; skin natural; no pain; odor from breath the same; tongue exhibiting a second tubercle, pointed, and of a dark color; complains of palpitation. Twelve o'clock, great weakness, but in other respects better; pulse 76; complains of no pain; œdema increased; Seltzer water and claret were ordered. Four o'clock, has commenced sinking; musk draught, also brandy and water were given. A little before seven he expired.

"POST MORTEM EXAMINATION, THIRTY-SIX HOURS AFTER DEATH.

"I was assisted by Drs. Woodroffe, Neligan, Thomas Mitchell, and Henry. Universal anasarca; integuments of the abdomen of a slight greenish color, particularly at the sides; abdomen greatly distended with gas; on opening the chest, we found the lungs completely collapsed, natural in structure, but containing little air; a fluid, to the amount of two pints, of a reddish brown color, without odor, was effused into the chest; heart pale and flabby, not containing blood, nor changed in structure; a little fluid in the pericardium.

"ABDOMEN.—Liver of a deep indigo color, not increased in size; the gall bladder distended with bile; the kidneys of a

* The blister, in sixteen hours, did not produce a single vesicle; blistering plaster from a second establishment, in twelve hours, produced scarcely any alteration of the surface.

deep indigo color all through, the left particularly large, the internal structure resembling much the spleen in appearance; the right smaller and firmer; the stomach empty; two distinct patches of inflammation were observable in the greater curvature; the mucous membrane easily separated; the bladder empty and natural.

"HEAD.—Dura mater healthy; arachnoid somewhat vascular, containing air bubbles underneath; substance of the brain bloodless; no fluid in the ventricles.

"In this case, we have many points for consideration:—First, what were the impurities which the hydrogen contained? For the elucidation of this question, I obtained the bottle from which the sulphuric acid was procured, and also a portion of the zinc employed by the deceased in the generation of the gas; and having obtained a double-necked bottle, with two tubes, one, a bent one, for the transit of the gas, and the other, a safety tube, which dipped into the fluid; and having added water to the acid, in the requisite proportions; I passed the mixture into the bottle, having first introduced the zinc, when immediately hydrogen was evolved, from which I obtained a large portion of arsenious acid and metallic stains. I then heated the tube to redness, and in the cool portion of it a large quantity of the metal was deposited, forming the well-known *metallic crust*, and crystalline appearance. The arsenious acid was then subjected to the fluid re-agents, hydrosulphuret of ammonia, ammoniaco-sulphate of copper, and ammoniaco-nitrate of silver, all of which exhibited their characteristic colors. A portion of it was also introduced into a tube with charcoal, and the metal was instantly produced by heat, all of which, with the foregoing results, were exhibited to Sir H. Marsh, and Drs. Graves and Stokes. The second question for consideration was, in which of the ingredients was the arsenic to be found, or was it in both? In order to ascertain this fact, I sought for some pure sulphuric acid; and after subjecting six different specimens from different respectable establishments in this city, both of the pure and impure, according to the Pharmacopœia, I found all to contain arsenic in large quantities.* However, through the kindness of Professor Kane, I was enabled to obtain *a pure acid*,† from which I could not obtain a trace of the metal, although using the ordinary zinc of commerce, as well as a portion of that which I had obtained from the deceased's establishment, and a part of which he had used. So far, then, it is quite evident that the sulphuric acid was the immediate source from which the arsenic was eliminated; and I would wish to caution the public, generally, from employing such acid for medicinal purposes, and also those exposed to the fumes of the acid during its concentration, or using it extensively in the arts, lest injurious consequences may result. According to Orfila, Vogel, Vakenrodder, and Barthel, all the sulphuric acid prepared in leaden chambers contains arsenic in considerable quantity. I have found that the fuming acid of Germany is free from arsenic, at least, as far as my experience enables me to form a judgment. Having used iron turnings in place of zinc, the arsenious acid was produced in a similar apparatus, from the British acid, but none from the German. Third, the fluid found in the chest, amounting to twenty-two fluid ounces, of a reddish brown color, feebly alkaline, specific gravity 130°, was filtered. Ten ounces of this were then evaporated in a sand-bath to the consistence of a jelly; when weighed it amounted to four ounces; to this two ounces of German sulphuric acid were added and exposed to heat in a sand-bath, when it became carbonized. In this state I dissolved it in distilled water and filtered; after which I placed it in the same apparatus with zinc and pure sulphuric acid. The gas given off was found to contain arsenic from the *stain* produced, and also the garlic odor emitted at the extremity of the tube. This experiment I consider highly valuable, as it is a confirmation of Orfila's statement that arsenic may be obtained from the bodies of persons poisoned by this substance, no matter in what way introduced into the system, although a trace of such may not be found in the contents of the stomach or intestinal tube. In this instance, although inhaled in the form of arseniuretted hydrogen, and though the quantity of arsenic received into the system was exceedingly small, merely sufficient to *destroy life*, still sufficient evidence was afforded of its presence.

"In order to ascertain with exactness the

* All our sulphuric acid is now formed from the sulphur obtained from *iron pyrites* and in leaden chambers. It will be in the recollection of our readers that, in No. XIX. of THE CHEMIST, page 207, we commented on the danger attendant on this mode of manufacture, and it is more than probable that we shall at some future period recur to it. At all events, we shall carefully note all cases in which its injurious effects are manifested.

† This was the German acid prepared from sulphate of iron.

quantity of arsenic contained in the acid, I took two hundred grains of the sulphuric acid mixed with four hundred of distilled water, and passed a stream of sulphuretted hydrogen through it for a considerable time, when a copious yellow precipitate was formed. I then exposed this to heat in a flask, to get rid of the excess of sulph. hydrogen, then filtered, collected the precipitate, which weighed ten grains, dissolved it in water of ammonia, and got rid of the sulphur, which was thrown down with the orpiment, and subsequently weighed six grains; the equivalents of arsenious acid and orpiment being nearly the same, and calculating the amount of acid used in the separating of the gases half an ounce, I consider the amount of arsenious acid inhaled to have been about twelve grains, as nearly as I could calculate, from the statement made by Mr. Brittan's assistant of the quantity used."

The author then comes to the question—"Were these symptoms those which are stated to arise in cases of poisoning by arsenic, or its compounds? The symptoms of arsenic, according to Christison, may be advantageously considered under three heads.

"In one set of cases there are signs of violent irritation of the alimentary canal, and sometimes of the other mucous membranes, accompanied also with excessive general depression, but not with distinct disorder of the nervous system. When such cases prove fatal, which they generally do, they terminate for the most part in from twenty-four hours to three days.

"In the second form the cases are singular; scarcely any sign of irritation in any part of the alimentary canal exists, perhaps trivial vomiting or slight pain in the stomach; sometimes neither. The patient is chiefly or solely affected with excessive prostration of strength and frequent fainting; death is seldom delayed beyond the sixth hour.

"In a third class of cases life is commonly prolonged, at least six days, sometimes much longer, or recovery may even take place. After a serious illness, the signs of inflammation in the alimentary canal are succeeded or become accompanied, about the second or fourth day, or later, by symptoms of irritation in the other mucous passages, and more particularly by symptoms indicating a derangement of the nervous system, such as palsy or epilepsy.

"In the division of symptoms here mentioned, we have those which accompany almost every form of poisoning by arsenic.

"The case, however, of Mr. Brittan cannot be considered as belonging to any one of those forms; accurately speaking, some of the symptoms in his case are to be met with in the first form, namely, that of urinary suppression, as in a case related by Guilbert of Montpellier, in which this symptom continued for several days. The rapidity with which the symptoms of poisoning set in, namely, vomiting and weakness, &c., may be considered as allied to this form. We have an instance of this in the case of Gehlen, who, when occupied with M. Rubland in researches on the reciprocal action of arsenic and potass, inspired a small portion of the arseniuretted hydrogen; and who, at the termination of one hour, was seized with continued vomiting, shivering, and great weakness, which increased until the ninth day, when he died. Now, the early symptoms in Mr. Brittan's case were similar in a great degree, but diminished after the first day, nor did he suffer considerably towards the close, as it is stated Gehlen did. We find that the second form furnishes us with many of the symptoms which were met with in the case of Mr. Brittan. First, those of slight irritation in the alimentary canal, and towards the close somnolency, and loss of memory, to which may be added numbness of the extremities and tuberculous state of mouth; but we are distinctly told that such symptoms are only met with in cases where the arsenic has been taken in very large doses, in masses, or solution. I am happy, however, to find, that Dr. Taylor, in a paper already alluded to, states, that the chief symptoms observed by him, in those who were poisoned by wine, which contained arsenious acid in solution, were violent vomiting, and prostration of strength. I merely quote this in confirmation of the statement of several authors, not as applicable to Mr. Brittan's case, but as showing that the same symptoms may arise even where the dose is small. Several other examples of death, occurring without pain, by this poison, might be adduced. In this form we are presented also with the fact of œdema of the face, occurring previous to death, as related by Schlegel of Meiningen. I have, however, to record the following symptoms in the case of Mr. Brittan, which I have not seen in any authors, namely, the jaundice supervening on the second day, and the ammoniacal odor of the breath.

"With respect to the latter symptom, we know that urine allowed to stand for some time becomes quite alkaline, and effervesces with acids, and that it is employed in some of the arts as a solution of carbonate of ammonia; this alteration being produced by the urea, a soluble principle, which is so composed, that, in adding to its elements water, we obtain carbonate of ammonia. In like manner, in treating urea by the acids,

alkalis, heat, or ebullition in water, it acts almost in a similar way to carbonate of ammonia, and when left in solution is transformed into this salt. This change may take place in the kidneys in typhus fever. So far, the case resembled one of narcotico-acrid poisoning, in which the cerebro-spinal system appeared to have been engaged, as is evident from the great derangement of the vital functions, and the total loss of sensibility of cutaneous surface. As to the pathological appearances, they, in some degree, differed from those usually stated by authors as occurring in the various forms of arsenical poisoning; particularly the indigo color which pervaded the entire structure of the liver, and also the state of the kidneys, but the alimentary canal agreed in appearance with the cases already mentioned. The brain, heart, and lungs may be considered, also, not to have presented unusual appearances under such circumstances.

"In respect to treatment, I consider, with others, that in cases of poisoning there are two periods. In the first, we endeavour to expel the poison, or withdraw and neutralize by chemical action whatever of it may have remained behind. In the second stage we try to counteract and subdue the symptoms which may arise, whether of inflammation or of debility and exhaustion, of excitement or of stupor. In this case, the former consideration was totally set aside, and the second, as well as my judgment could dictate, carried into effect.

"In conclusion, I shall state, from the manuscript left by the deceased, his own instructions respecting the mode of inhaling the gas; at the same time I may observe, that society has lost an excellent and useful member, chemical science an ardent admirer and cultivator of its phenomena, and the students of Dublin a friend and instructor. The following are his directions:—'Fill a bladder with hydrogen; this is done by tying a bullock's bladder to a porter cork, through which a hole has been made sufficiently large to admit a small cock, which contains in its turn the bent delivery tube. The bladder must, of course, be exhausted of air by being squeezed together; when it has become distended with gas, take out the small cork, stopping the orifice by the thumb, until the experimenter is ready to breathe it. After taking but one inspiration the voice will become remarkably shrill, the effect, however, passes off in a few seconds, nor is there any danger connected with it.'

"The experiment of Mr. Brittan has, however, afforded melancholy evidence that it may have a contrary and fatal effect."

ABUSES OF THE DRUG TRADE.

Letter I.

To the Editors of The Chemist.

Gentlemen,—

Among the many impositions daily and hourly practised, and which have been at various times held up to public scorn in the columns of your ably conducted journal, I know of none more grossly unjust, and certainly none more deserving of exposure, (than which a greater public service cannot be performed,) than the disgraceful and injurious system pursued by the majority of the medical profession,—members of either College and of the Society of Apothecaries alike,—towards their patients and their semi-professional brethren, the chemists and druggists, *viz.* that of recommending particular establishments at which they wish their prescriptions to be compounded. Some less scrupulous, if not less honorable members, not content with this partial recommendation, absolutely *desire* their various panaceas and specifics to be prepared at some favored shop, and on no account elsewhere, accounting for such flagrant abuse of professional influence, after the following fashion:—"I have written for a particular preparation, which can be obtained at such a shop only, or at least there only of a genuine quality:" which assertion of an eminent professional man cannot be doubted by the confiding patient, he or she not being supposed to know that which is well known to every member of the profession, *viz.* that the trade are in continual communication one with the other, whereby every article in pharmacy becomes available even to the most humble compounder of pills and potions. As to the genuineness of the drugs, &c., it is well known, that the price of the generality of them, together with the small quantity employed in dispensing, is such as to render adulteration useless; a state of things which probably exists more in the imagination of such self-constituted judges of the integrity of "better and more honorable men," than in practice, and which, if existing, could not possibly have been brought about otherwise than by the venality and iniquitous practices of such wiseacres.

Then another will say, "I have ordered a preparation of my own, the formula for which I have imparted to such a person only, therefore let that person make it up, as it cannot possibly be compounded accurately elsewhere." Certainly not, unless this sapient M.D., or other professional with his boasted nostrum, who thus unwittingly pronounces himself neither more nor less than an arrant quack, would take the trouble to write the recipe, or at

least insert where the said article might be procured, which would scarcely be giving too much for his guinea.

Others content themselves by making the sweeping and self-important declaration, "that you cannot depend upon the quality of the drugs kept by the generality of the shops, but may rely upon having the recipe prepared with the greatest nicety and correctness by such a party or at such a shop:" of course, for the very weighty and obvious reason (which, by-the-bye, the wily professional takes care not to let transpire,) that he has an interest in the favoured party or establishment,—it being a notorious fact, that in ninety-nine cases out of a hundred, the parties thus imitating the Jews of Holywell-street, in their touting propensities, have either direct or indirect interests in their recommendations, which they would fain make the patient, already too prone to prejudice, believe was in his or her welfare, and owing to their anxiety for his or her restoration to health. Moreover, the greater portion of such *honorable* members are annually in receipt of a per centage on the amount realized by such recommendations; the unsuspecting dupe scarcely imagining, when handing over the one, two, three, or five guinea fee, that the hungry professional is endeavouring to grasp at a share of the profits arising out of the dispensing of the recipe for which he has been so amply paid, the cost of which seldom averages more than half-a-crown, the profits being very disproportionate between the vilified dispenser who supplies the Elixir Vitæ and the licensed swindler who prescribes it. Here I think it would be well to enlighten a prejudiced public a little respecting those monopolists of the good will of the medical profession, the proprietors of these favored and most immaculate establishments: the public, at all times easily humbugged, will, nevertheless, it is hoped, after this *exposé*, not be backward in discriminating between honest persevering industry, (with fair remuneration for labour for its motto,) and that species of duplicity, which, under the above pernicious influence, obtains notoriety for selling good and cheap drugs. Common honesty dictates the propriety of advising the economising of a penny or twopence on a box of pills or a mixture, when in daily receipt of at least one of Queen Victoria's bright sovereigns. The patient no doubt finds the familiar pressure of the professional hand has most irresistible influence upon her purse strings. Of course a gentleman who has his patients' interest so much at heart as to proffer his *disinterested* views of economy gratuitously, cannot possibly have any sinister design upon it himself.

It is presumed that the public will be able to judge which establishments they are, whose proprietors are enabled to give sumptuous entertainments annually to the above description of professional worthies, and also to supply their much respected patrons' tables with game and choice fruits, such as pines and hothouse grapes, thereby enabling the aforesaid to maintain a species of grandeur in their entertainments towards their less fortunate, but, perhaps, more honorable professional brethren.

It will be evident to all that this class of persons is not indebted to the honest and ill-used trader (whom they defraud of that support which locality and strict attention to business entitle him to) for their grand dinners, and "the luxuries wherewith they grace their boards."

In the former part of my letter I have made use of the word majority, in alluding to a very general practice of the members of the profession; should that word appear to take too wide a range, let they whom the cap fits wear it; for, broad as the assertion may be, it is but too true, as the far greater proportion are, more or less, addicted to the same; but with what motives I leave to their own reflections, and which are probably best known to themselves, although I doubt not they can, according to their own idea of human excellence, account for such injustice plausibly enough.

I am, Gentlemen,
Respectfully yours,
PHARMACOPOLA.

FORMULÆ FOR MAGNETIC OXIDE OF IRON, EAU DE COLOGNE, AND ARQUEBUSADE.

To the Editors of The Chemist.

GENTLEMEN,—

Having noticed in No. XXV. of your interesting periodical, the requests of your correspondents "A Subscriber," and "Enquirer," to be furnished with formulæ for the Magnetic Oxide of Iron, Eau de Cologne, and Arquebusade, I beg to offer the following from the second edition of the Pocket Formulary, which I hope will be ready by the 1st of February.

I am, Gentlemen,
Your obedient Servant,
THE EDITOR OF THE
POCKET FORMULARY.

Ferri Oxydum Magneticum. Dr. Jephson's formula. Ferri sulph. cryst. cont. ℥ xxiss, aquæ Oij, acid. nitrici fort. f ℥ iv, vel q. s. Heat in an earthen vessel at 180°,

stirring frequently, and adding the nitric acid gradually, till the solution no longer yields a blue precipitate with the red or *per*-prussiate of iron. When quite cool, add *suddenly* to this, a solution of ℥ x¾ ferri sulph. in Oiij of water. Dissolve by heat, in a large iron pan, ℥ xl soda sub-c. cryst. in aquæ Oiij; add to this, gradually, the mixed solution of iron, stirring them well together. Boil briskly for half an hour; remove, settle, pour off the liquid, add Ovj of water, boil for half an hour, decant the liquid, wash the precipitate repeatedly, drain in muslin, and dry carefully.

Aqua Coloniensis. P. *Eau de Cologne.* Ol. bergamii ℥ iij, ol. limonis ℥ iij, ol. cedrat. ℥ iij, ol. rosmar. ℥ iss, ol. neroli ℥ iss, ol. lavand. ℥ iss, ol. cinnam. ℥ vj, sp. rect. Oxxiv, sp. melissæ Co. Oiij, sp. rosmar. Oij; mix, and after 8 days distil Oxxiv.

Spiritus Vulnerarius. P. *Arquebusade.* Fol. rec. basilici, calaminthæ, hyssopi, origani, melissæ, menthæ, marjoranæ, rosmarinæ, salviæ, thymi, serpylli, absinthii, angelicæ, fœniculi, rutæ, satureiæ (*savory*) sing. ℥ j, sum. flor. hyperici ℥ j, lavandulæ ℥ j, spir. ten. Oiij, macerate 6 days and distil Oij.

[This article was received at the early part of last month, and put in type, but omitted in No. XXVI. owing to want of space. The Pocket Formulary has since been published, *vide* page 96.]

ON THE PREPARATION OF MEDICINAL WATERS WITH MAGNESIA.

" Ingenuas didicisse fideliter artes
Emollit mores, nec sinit esse *feros*."

To the Editors of The Chemist.

Gentlemen,—

When you did me the honour of inserting my observations and opinions on the above subject in the December number of The Chemist, a diffident feeling respecting my inferences led me to anticipate that further remarks would be made on the subject in good feeling, not with a degree of vindictiveness which would have been discreditable even to a violent politician.

On receiving the January number of The Chemist, and on beholding so long an article on the same subject, my first idea was that some valuable remarks might be contained in it; but I felt extreme surprise to find that the chief part of it was nothing but angry charges and gross sarcasms, while the remaining very small portion relating to the subject is so vague, and unsupported by either proof or inference, that if I chose to follow "G. C.'s" evil example, I might justly charge him " with propagating gross fallacies, and with an ostentatious display of pretended chemical knowledge."

" G. C.'s" remarks may please the splenetic; but the feeling of the individual who reads for information will be, that

" Like the baseless fabric of a vision,"
it
" Leaves not a wreck behind."

That my statements were not so very absurd, is proved by your correspondent himself, (if his statement is of any value,) for, doubting the correctness of my conclusions, he tried some experiments ! ! before he could form an opinion on the subject; and Mr. Phillips considers ferri iod. incompatible with lime water and the alkalis; and that magnesia is incompatible with metallic salts; therefore, although magnesia is nearly insoluble in water, a little passing through coarse filtering paper, or spilt in pouring, would be sufficient to decompose the ferri iod.

Your correspondent, speaking of waters prepared by trituration with magnesia, says, " As regards strength, they may be increased *ad libitum*." Your readers will readily agree that this is a mere dogma of his own, unless he uses alcohol.

He denies that distilled waters keep better than those prepared with magnesia. On this point my opinion was given from experience, and not from mere theory. The first few years of my life in business were spent in an eminent London establishment, where we drew off large quantities of medicated waters for our own use, and the supply of many surgeon-apothecaries, and I never knew or heard of an instance in which any of them became deteriorated; whereas I have seen waters prepared by trituration, and only by a quart at a time, completely putrid. Your correspondent seems to have some misgivings on that point, for he adds, " when prepared with aqua distillata," which is rarely, if ever, the case.

" G. C." asserts that the precipitate " is merely surplus iodide of iron." Let him, if capable, subject it to a careful analysis, and then give the results. This would soon convince him of the absurdity of his conclusion; for the ferri iodidum is not merely soluble in water, but is deliquescent. Phillips says, " When exposed to the air it attracts moisture, and is very soluble in water and alcohol. In order to prevent the deposition of sesquioxide of iron by the absorption of oxygen, the solution should be kept with a piece of iron wire in it;" therefore if Mr. Phillips or Mr. Brande are any authorities, the deposit which " G. C." obtained from distilled water was sesquioxide, and not surplus iodide.

Why he should quarrel with the term ferri sesquioxydum, I know not; for had he

referred to the last Pharmacopœia, he would have found, in the column headed "Nomina Nova," "Ferri Sesquioxydum" opposite "Ferri Subcarbonas," in the one headed "Nomina Priora;" but your correspondent reads with an angry jaundiced eye, and quarrels with every thing.

When the iodide of iron is kept in bottles, corked, or not well stoppered, and even by exposure to light, it becomes changed, and, according to its state, is more or less soluble; it seems probable that "G. C." performed his experiments on some thus impaired, which accounts for his otherwise unaccountable theory of "surplus iodide" and "granulation."

It would be out of place in a controversy to make any remarks on the probable effects of tannin, lignin, and cinnamic acid on the ferri iod.; merely remarking, that the deposit occasioned by the absorption of oxygen seems of a browner color than the ferri sesquioxydum of the Pharmacopœia; as also is the substance itself, when injured by keeping, probably because the ferri sesquioxydum contains a small portion of carbonic acid, which would be the case with the deposit I obtained.

It would be trespassing too much on your pages, and on the patience of your readers, further to notice "G. C.'s" mis-statements; I therefore leave the matter with your readers, respectfully soliciting their judgment whether my short contribution, even if erroneous, merited a reply, written by a person in such a temper, that he seems to forget the subject on which he writes, to abuse the writer he unprovokedly attacks.

I am, Gentlemen,
Respectfully yours,
J. S.

Peterborough,
7th Jan. 1842.

REVIEWS:—

THE POCKET FORMULARY; *and Synopsis of the British and Foreign Pharmacopœias: comprising Standard and Appointed Formulæ for more than* 2000 *officinal and extemporaneous Compounds employed in Medical Practice: arranged in Alphabetical Order.* By HENRY BEASLEY. Second Edition. London: Sherwood, Gilbert, and Piper, Paternoster Row. 1842.

THIS is by far the most complete and useful work of its kind: it contains 1500 more formulæ than the first edition. It is of very portable size, and we may here repeat what we said of the former edition, namely, that every medical practitioner and chemist and druggist should carry it about him. It gives the formulæ and mode of preparing all the compounds treated of.

MEDICAL STUDENT'S GUIDE; *containing the Latest Regulations of The Universities, The Colleges of Physicians, The Colleges of Surgeons, The Army, Navy, Ordnance and East India Medical Boards, &c. &c. &c.* Dublin: Fannin and Co. London: Renshaw, Strand. Edinburgh: Machlachlan and Stewart.

THE title of this work is sufficiently indicative of its utility. We consider that no medical student, nay, no medical practitioner, should be without it.

BOOK RECEIVED.

AN Investigation of the Present Unsatisfactory and Defective State of Vaccination; and the Several Expedients Proposed for Removing the now Acknowledged Defects of the Jennerian Practice. In a Series of Letters addressed to Dr. George Gregory, Physician to the Small Pox and Vaccination Hospital, London. By THOMAS BROWN, formerly Medical Practitioner in Musselburgh. Edinburgh: Machlachlan, Stewart, and Co. London: Whitaker and Co. 1842. pp. 139.

[This work will be noticed in our next.]

TO CORRESPONDENTS.

VARIOUS, indeed, are the questions from month to month asked of us, and many of them are of a nature totally unconnected with our work. But we have now to reply to a question which outstrips all the others. "A SUBSCRIBER" inquires of us whether the plural of "spoonful" is more correctly spelled "spoonsful" or "spoonfuls." We give our opinion in favor of the latter, but space will not admit of our giving the reasons for such decision. We suppose that "A SUBSCRIBER" is desirous of spelling it correctly on his labels. We may here remark, that the last syllable is not spelled with double *l*, a fact of which he seems to be unaware.

MR. THOMAS WARBURTON is requested to apply to our publisher.

A SUBSCRIBER, Brighton.—By adding carbonate of soda to muriate of lime.

ERRATUM. No. XXVI. page 63, col. 1, line 29 from bottom, for "mineral," read *aromatic*.

NOTA BENE.—*All Communications and Books for Review must be addressed, "To the Editors of The Chemist, Care of Mr. Hastings, Publisher, 13, Carey Street, Lincoln's-inn Fields, London." Communications must be prepaid, and sent before the* 15*th of each Month; Books for Review before the* 10*th.*

THE CHEMIST.

I. CHEMISTRY.

INVESTIGATIONS CONCERNING INDIGO.*

BY M. ERDMANN.

INDIGO and its derivatives have been made the subject of very attentive study on the part of M. Erdmann. In three very remarkable memoirs, that chemist has made numerous observations on the composition of indigo, as well as on the transformations which this body undergoes under the influence of oxygen and chlorine. The facts contained in these memoirs now offer new support to the theory of types and substitutions, and are connected in a manner so simple, that they may be considered as definitively fixed.

M. Erdmann at first took as the basis of his calculations the old atomic weight of carbon, which led to erroneous formulæ; but he rectified them without delay, having first ascertained, by experiments in which he was assisted by Marchand, the correctness of the new atomic weight proposed by Dumas and Stas. (For Erdmann and Marchand's experiments, see page 109.)

We here give a summary of his principal observations.

COMPOSITION OF BLUE INDIGO.

In addition to the difficulty of obtaining indigo in the pure state, this body has the inconvenience of burning very difficultly, which explains the differences remarked between the first analyses of M. Erdmann and those of M. Dumas. However, the products of the decomposition of indigo leave no doubt as to the formula which should be adopted for this body; M. Erdmann now admits the formula

$$C^{32} H^{10} N^2 O^2,$$

established by M. Dumas.

A new analysis gave him, for 0·586 gr. of matter, 0·206 water, and 1·590 carbonic acid; which makes, per cent.,

Carbon . . .	73·99
Hydrogen . .	3·80

The preceding formula would make

Carbon . . .	73·18
Hydrogen . .	3·80

It is probable that the excess of carbon obtained by experiment arises from a slight impurity.

ISATINE.

A concentrated solution of chromic acid immediately destroys blue indigo, especially by the aid of heat, disengaging from it much carbonic acid, and separating oxide of chromium. On the other hand, a dilute solution of chromic acid dissolves it, giving a yellowish brown liquid, which remains limpid when left for a long time.

When indigo is heated with a solution of chromic acid, of a proper strength, to near its boiling point the liquor, being afterwards filtered, on cooling, deposits the body which M. Laurent calls *isatine*.

The preparation of this interesting product does not always perfectly succeed. An essential condition is to dilute the chromic acid properly. If it be too concentrated, there is observed, on heating the liquor, an effervescence of carbonic acid, and then it deposits little or no crystals. On evaporating the liquor, it is remarked that it becomes turbid, depositing a brown powder, composed partly of oxide of chromium. The liquor deposits crystals of isatine only by spontaneous evaporation in the air, or *in vacuo*. This means must be made use of when the liquor does not give crystals, from having been treated with too dilute an acid.

The crystals obtained are purified by crystallisation in water and in alcohol.

Isatine sometimes forms large prisms of a deep yellow; sometimes, and especially by rapid crystallisation, small yellowish red

* *Annales de Chimie et de Physique*, Novembre, 1841.

prisms, which present much lustre when they are produced in an alcoholic solution.

This body is not very soluble in cold water, and more soluble in boiling water. Its alcoholic solution communicates to the skin a disagreeable odor, which remains for some time. When isatine is heated in a small closed tube, a small portion is sublimed, while the greater proportion is decomposed, leaving an ash which is difficult to burn.

Isatine contains

$$C^{32}\ H^{10}\ N^2\ O^4:$$

it is evident that it is indigo oxidised—indigo which has acquired two atoms of oxygen without losing anything.

A solution of potassa dissolves isatine, acquiring a deep purple color. When hydrochloric acid is added to the solution, a crystalline and reddish yellow powder is precipitated, which appears to be unaltered isatine. If, on the contrary, the red solution be heated, or if it be left for some time to itself, it is discolored, becomes of a dark yellow, and furnishes by evaporation a crystalline salt, soluble in alcohol, and crystallising in small colorless and transparent prisms. This salt of potassa contains an acid, *isatic acid*, which is represented, in the free state, by isatine plus two atoms of water:—

$$C^{32}\ H^{10}\ N^2\ O^4 + H^4\ O^2 = C^{32}\ H^{14}\ N^2\ O^6.$$

The salt of silver of this acid contains

$$C^{32}\ H^{12}\ N^2\ O^5\ Ag\ O = C^{32}\ H^{12}\ N^2\ O^6\ Ag.$$

Isatic acid is decomposed by heat into isatine and water; which proves that the above composition is very accurate.

To isolate isatic acid, its salt of lead is treated by sulphuretted hydrogen. The colorless solution gives, by evaporation *in vacuo*, a white, flocculent, and scarcely crystalline powder, which is easily decomposed into isatine and water.

With the aid of heat, isatine completely dissolves in hydrosulphate of ammonia, and the solution deposits, on cooling, a yellowish and not very crystalline powder; after several washings, it is entirely free from sulphur. By drying in the sand-bath, it assumes a reddish tint. M. Erdmann gives it the name of *isatyde*, and assigns to it the following composition:—

$$C^{32}\ H^{12}\ N^2\ O^3.$$

This formula requires to be confirmed by new analyses.

When sulphuretted hydrogen is passed into an alcoholic solution of isatine, the liquor gradually becomes of a pale yellow, and becomes turbid, at the same time depositing sulphur. If a hot solution be employed, the sulphur often separates in distinct crystals. The filtered liquor turns milky on the addition of water, and deposits yellowish white flocks, which contain sulphur in chemical combination; Erdmann calls them *sulphisatine*, and expresses them by

$$C^{32}\ H^{12}\ N^2\ O^2\ S^3.$$

This composition requires to be verified by new analyses.

ACTION OF CHLORINE ON INDIGO.

In the dry state, chlorine and indigo do not act on one another, nor at a low temperature, nor at 100° C.

If, on the contrary, blue indigo be moistened with water, so as to form a paste, and if a current of chlorine be passed into it, the blue color of the indigo gradually disappears; the mass assumes a greyish green tint, and finally becomes yellow. The hydrochloric acid which is formed at the same time, remains almost wholly dissolved in the liquor. During this reaction, neither carbonic acid nor any other gaseous product is disengaged.

The crude product which results from the action of chlorine on indigo, is a mixture of several substances. Submitted to distillation, it gives an aqueous liquid, charged with a solid and volatile matter, which is deposited in greater quantity on the sides of the retort and in the receiver, under the form of bracteæ or white needles.

Erdmann calls this body *chlorindopten*. We shall speak of it further on.

The crude product chiefly contains two chlorous substances, which may be obtained pure by crystallisation in alcohol. The first of these substances is *chlorisatine*, or monochloruretted isatine,

$$C^{32}\ H^8\ N^2\ O^4\ Cl^2;$$

the other is *bichlorisatine*, or bichloruretted isatine,

$$C^{32}\ H^6\ N^2\ O^4\ Cl^4.$$

The first is least soluble in alcohol, and crystallises the first. These two substances may also be formed by the direct action of chlorine on isatine.

Bichlorisatine is always produced, even when the action of the chlorine on the indigo has not been complete, in greater quantity, and perhaps even double that of chlorisatine.

It is evident that chlorine, in acting on moistened indigo, first decomposes the water, oxidises the indigo at the expense of the oxygen of the water, and afterwards produces bodies derived from the type of isatine.

CHLORISATINE.

If the crude product be dissolved in boiling alcohol, on cooling, reddish yellow crystals, chiefly composed of chlorisatine, are obtained. Sometimes the crystals preserve a brown tint, even when several times re-

dissolved; this color is owing to a small quantity of resinous matter which is formed with the chlorisatine. For this reason, the formation of large crystals must be as much as possible avoided, by stirring the solution while it is cooling. Chlorisatine may be obtained perfectly free from resin by means of chlorisatate of potassa, which will be described further on.

Chlorisatine crystallises in quadrilateral prisms or in orange yellow lamellæ, transparent, and of great lustre. It is inodorous, and has a bitter taste. Its dust irritates the olfactory organs and excites sneezing.

Heated in the air, it melts into a brown liquid, and exhales yellow vapors, whose odor resembles that of indigo when it is burned. It burns with a luminous flame, leaving a difficultly inflammable ash, which imparts a green color to the flame of spirit of wine. It may be heated to 160° C., without being decomposed, and without losing water. At a higher temperature, it is partly sublimed without alteration, under the form of orange yellow needles; however, the greater part turns black, enters into fusion, and is decomposed, swelling up considerably. It is almost insoluble in cold water; it dissolves in boiling water with an orange yellow tint; it is also very soluble in alcohol. Its solution communicates to the skin a disagreeable odor, which endures some time.

Concentrated sulphuric acid dissolves chlorisatine, acquiring a red brown color; water precipitates it from the solution, to all appearance without alteration. Fuming nitric acid decomposes chlorisatine, producing from it peculiar compounds.

The new formula by which M. Erdmann represents chlorisatine is, as we have already stated,

$$C^{32}\ H^{8}\ N^{2}\ O^{4}\ Cl^{2};$$

that is to say, isatine, in which 1 equivalent of hydrogen has been replaced by 1 equivalent of chlorine.

Chlorisatine may also be formed by the direct action of chlorine on isatine.

Chlorisatic Acid.—When a solution of chlorisatine is treated with caustic potassa, and when the liquid is heated, the latter is decolored, and deposits, on cooling, shining bracteæ of a salt of potassa.

This salt contains a peculiar acid, and is composed of

$$C^{32}\ H^{10}\ Cl^{2}\ N^{2}\ O^{5},\ PO.$$

The acid which it contains is *chlorisatic acid*, that is to say, isatic acid in which 1 equivalent of hydrogen is replaced by 1 equivalent of chlorine.

By repeated crystallisations in alcohol, chlorisatate of potassa is obtained in the state of purity.

This salt forms pale yellow bracteæ of great lustre, or flattened quadrilateral needles. It readily dissolves in water, and possesses a very bitter taste.

Chlorisatic acid cannot be isolated, for if a stronger acid be added to the solution of the above salt,—as pure hydrochloric acid,—as soon as the two liquids come in contact, the formation of a yellow precipitate is remarked; but this is immediately dissolved, and, at the same time, the liquor acquires an orange yellow tint, becomes turbid, and very soon deposits chlorisatine under the form of crystalline bracteæ. If the mixture of hydrochloric acid and chlorisatate of potassa be heated, it immediately acquires a deep orange tint, and gives, by cooling, crystals of chlorisatine.

When cold, acetic acid does not alter the solution of chlorisatate of potassa; but if the mixture be heated, chlorisatine separates from it. This reaction furnishes the surest means of obtaining chlorisatine in the free state.

By means of chlorisatate of potassa, many other chlorisatates may be obtained by double decomposition.

The formation of chlorisatate of lead is accompanied by a very curious phenomenon. If a solution of acetate or nitrate of lead be added to a solution of chlorisatate of potassa, a gelatinous and shining yellow precipitate is obtained, which, at the end of a few minutes, and especially if the liquid be stirred, becomes flocculent, and acquires a superb scarlet color, almost as fine as that of periodide of mercury. This transformation is often instantaneous, and very frequently the red coloration begins only at certain points, being gradually propagated throughout the mass. If this phenomenon be observed by the aid of a microscope, it is remarked that the yellow precipitate is pulverulent, without any indication of crystallisation, and that, as it disappears, it makes way for red dendritic vegetations which suddenly appear. Sometimes these are not produced, and the precipitate is converted into larger crystalline grains, agglomerated and presenting only here and there indications of regular faces. These changes of color are evidently only a simple effect of the crystallisation of the salt.

The formation of chlorisatate of copper is accompanied by a similar phenomenon.

BICHLORISATINE.

This compound is contained in the alcoholic solutions of chloruretted indigo, from which chlorisatine has already been crystallised. The alcohol is removed by distillation, occasionally interrupting the operation to remove the crystalline deposits

formed by cooling; they are composed of a mixture of chlorisatine and bichlorisatine, the latter in less and less proportion. This is continued until the deposits present an uniform composition. In this case they then contain pure bichlorisatine. In order to purify it, it is recrystallised in weak alcohol, or it may be converted into bichlorisatate of potassa, which is decomposed by an acid.

This body contains

$$C^{32}\ H^{6}\ N^{2}\ O^{4}\ Cl^{4};$$

it is isatine in which 2 equivalents of hydrogen are replaced by 2 equivalents of chlorine.

Bichlorisatine is presented under the form of small yellow needles, possessed of great lustre. Sometimes quadrilateral prisms are distinctly recognised in it.

Heated in a close vessel, it is partly sublimed, and the remainder fuses into a black and carbonaceous mass. It is rather more soluble in water than chlorisatine; it is much more soluble in alcohol than the latter body.

Bichlorisatic Acid.—When cold, bichlorisatine dissolves in caustic potassa, and communicates to it a deep red tint. When the mixture is heated, this color disappears, the solution becomes yellow, and, on cooling or evaporation, it assumes the form of most brilliant yellow bracteæ, which are purified by pressing and crystallisation in alcohol.

These crystals constitute the salt of potassa of *bichlorisatic acid;* they contain

$$C^{32}\ H^{8}\ Cl^{4}\ N^{2}\ O^{5}\ PO.$$

If the aqueous solution be decomposed by any acid, without heat, bichlorisatic acid is precipitated in the state of a yellow powder.

This acid is very soluble in water; it is more stable than chlorisatic acid; but, for all that, it is very difficult to prepare it in a proper state of purity, for it is very easily decomposed into bichlorisatine and water, when it is dried *in vacuo*, even at a low temperature.

Free bichlorisatic acid is therefore

$$C^{32}\ H^{10}\ N^{2}\ O^{6}\ Cl^{4},$$

that is to say, isatic acid, in which 2 equivalents of hydrogen are replaced by 2 equivalents of chlorine.

The bichlorisatates present properties entirely analogous to those of the chlorisatates.

CHLORINDOPTEN.

This body is obtained, as we have already stated, by distilling the crude mass obtained by the action of chlorine on blue indigo. It is difficult to procure it in large quantity; for although it is very abundantly produced, the distillation does not succeed well towards the end, owing to the swelling up.

The solid product furnished by the first distillation is purified by a second distillation and by careful sublimation; it is thus obtained under the form of white, delicate, and very friable scales, of a peculiar, disagreeable odor. Alone, it does not very easily volatilise; but it does so with facility with the vapor of boiling water.

It is very sparingly soluble in cold water, but dissolves, in large quantities, in boiling water, and separates from it, on cooling, in crystalline flocks. Alcohol very readily dissolves it.

Erdmann thinks that chlorindopten is a mixture of two matters; indeed, if it be put in contact with caustic potassa, the disagreeable odor which it at first presented, instantly becomes very pleasant, like that of fennel or chervil. If the operation be carried on in a retort, there condenses in the retort a small quantity of a neutral body, to which Erdmann gave the name of *chlorindatmite:* it contains

Carbon . .	36·89 (old atomic weight).
Hydrogen .	2·23
Chlorine .	53·28

The formula

$$C^{24}\ H^{8}\ Cl^{6}\ O^{2}$$

requires

Carbon . .	36·76
Hydrogen .	2·00
Chlorine .	53·22

The salt of potassa which remains as a residue in the retort acquires, by cooling, the form of a pap composed of colorless, delicate and transparent needles. The solution of these crystals is precipitated under the form of white flocks which resemble chlorindopten, but whose odor is less disagreeable; they powerfully redden limtus. Erdmann gave them the name of *chlorindoptic acid.*

Chlorindoptate of potassa gives with nitrate of silver a very voluminous brown yellow precipitate, sparingly soluble in hot water and entirely insoluble in cold water; in acetate of lead it occasions a white precipitate; in sulphate of copper, a violet purple one.

Analysis gave, for the salt of silver, the formula

$$C^{24}\ H^{4}\ Cl^{6},\ Ag\ O.$$

Chlorindoptic acid, separated from the salt of potassa by sulphuric or hydrochloric acid, and dried *in vacuo*, presented the following composition:—

$$C^{24}\ H^{4}\ Cl^{6}\ H^{2}\ O.$$

Analysis gave

Carbon . .	38·00 (old atomic weight).
Hydrogen .	1·77
Chlorine .	54·53

Calculation would require

Carbon . .	38·48
Hydrogen .	1·57
Chlorine .	55·73

We may observe that M. Erdmann's results may be expressed by

$$C^{24} H^4 Cl^6 O, H^2 O,$$

which would make his acid identical with M. Laurent's chloro-phenesic acid.

ACTION OF BROMINE ON INDIGO.

The products to which blue indigo gives rise under the influence of bromine are completely identical with those obtained by the action of chlorine. If the white mass, which is produced when moistened indigo is treated by bromine, be distilled, bromindopten results.

Bromisatine contains

$$C^{32} H^8 N^2 O^4 Br^2;$$

Bibromisatine likewise contains

$$C^{32} H^6 N^2 O^4 Br^4.$$

These two bodies are derived from the isatine type, like the corresponding chloruretted bodies above described.

Potassa converts these two bodies into two peculiar acids—*bromisatic* and *bibromisatic acids.*

Bibromisatate of potassa may easily be obtained in the crystallised state; it consists of straw-colored brilliant needles, less soluble in water and alcohol than bichlorisatate of potassa. In the dry state these crystals contain

$$C^{32} H^8 Br^4 N^2 O^5 PO.$$

Bibromisatic acid is easily decomposed into bibromisatine and water.

The reactions of bibromisatate of potassa are entirely identical with those of bichlorisatate of the same base.

ACTION OF SULPHURETTED HYDROGEN AND HYDROSULPHATE OF AMMONIA ON CHLORISATINE, BICHLORISATINE AND BIBROMISATINE.

When a current of sulphuretted hydrogen is passed through a solution of chlorisatine, bichlorisatine, or the corresponding combinations of bromine, the liquid is decolored, and at the same time a white deposit is formed, composed of a mixture of sulphur and of a new combination which contains the elements of chlorisatine or bichlorisatine plus two atoms of hydrogen.

Hydrosulphate of ammonia gives rise to the same products.

The following is the manner in which the latter are formed:—

By chlorisatine:

$$C^{32} H^8 N^2 O^4 Cl^2 + H^2 S = C^{32} H^{10} N^2 O^4 Cl^2 + S;$$

By bichlorisatine:

$$C^{32} H^6 N^2 O^4 Cl^4 + H^2 S = C^{32} H^9 N^2 O^4 Cl^4 + S;$$

By bibromisatine:

$$C^{32} H^6 N^2 O^4 Br^4 + H^2 S = C^{32} H^8 N^2 O^4 Br^4 + S.$$

The author gives these bodies the names of *chlorisatyde, bibromisatyde, &c.*, to show that they are derived from chlorisatine, bibromisatine, &c. It is evident that they present the same relations with the latter compounds as that which exists between alloxan, alloxantine, benzoïn, and benzoïle, &c.

These new bodies are very sparingly soluble in water; boiling alcohol dissolves them readily, and deposits them on cooling, in white crystalline crusts, without any regular appearance.

ACTION OF HEAT ON CHLORISATYDE AND BICHLORISATYDE.

Under the influence of a temperature of about 200° C., the above-mentioned combinations disengage water and leave a brownish violet residue; at the same time there is sublimed a certain quantity of chlorisatine or bichlorisatine, according to the matter employed.

The residue is composed of a peculiar body, which the author calls *chlorindine.* Its formula is

$$C^{32} H^{10} N^2 Cl^2 O^2.$$

In like manner, bichlorisatyde and bibromisatyde give *bichlorindine* and *bibromindine.*

Chlorindine is presented in the state of a pale violet powder, insoluble in water, alcohol and hydrochloric acid, and soluble, with a yellowish color, in caustic potassa. The alkaline solution, decomposed by hydrochloric acid, gives a yellow precipitate, which dissolves in boiling water with a yellowish color.

ACTION OF POTASSA ON CHLORISATYDE AND BICHLORISATYDE.

The experiments of M. Erdmann do not satisfactorily account for what takes place in the action of potassa on chlorisatyde and bichlorisatyde. He obtained, under these circumstances, a violet body, which he presumed to be chlorindine, chlorisatic acid, and an acid isomeric with it, *chlorisatydic acid.*

This new acid, although it has the same composition as chlorisatic acid, gives, with bases, salts which, in their physical properties, greatly differ from the chlorisatates.

The author has likewise examined the action which ammonia, nitric acid and protosulphuret of potassium exert on chlorisatine and bichlorisatine, but his researches in this respect do not present any analytical data.

PROLONGED ACTION OF CHLORINE ON CHLORISATINE AND BICHLORISATINE.

Neither chlorisatine nor bichlorisatine appear to undergo any alteration when moistened with water and subjected to the action of a current of chlorine for several hours; Erdmann tried in vain thus to convert chlorisatine into bichlorisatine. Direct solar light did not appear to have any perceptible action.

If, on the contrary, chlorisatine or bichlorisatine be dissolved in alcohol, and if a current of chlorine be driven into this solution, a decomposition is speedily effected. The products are the same, whether chlorisatine or bichlorisatine be employed, or both mixed. Beside the chloruretted products of the decomposition of the alcohol, there are found, under these circumstances, three peculiar bodies :—*chloranile*, *chloruretted chlorindopten*, and a *resinous matter*. The latter appears to be the principal product of the reaction.

Chloruretted Chlorindopten.—This body is obtained by distilling the resinous product of the action of chlorine on chlorisatine, from which the chloranile has previously been extracted by means of cold alcohol.

It is sublimed in the neck of the retort in the state of white needles.

The crystals dissolve in potassa and thus give rise, fixing one atom of water, to a new acid which presents all the physical characters of chlorindoptic acid, but in which four atoms of hydrogen are replaced by four atoms of chlorine, as the following formulæ indicate :—

Chlorindoptic acid.	The same chloruretted.
$C^{24} H^4 Cl^6, H^2 O.$	$C^{24} Cl^{10}, H^2 O.$
Salt of silver.	The same chloruretted.
$C^{24} H^4 Cl^6. Ag O.$	$C^{24} Cl^{10}, Ag O.$

The salts of this new acid give, with metallic solutions, precisely the same reactions as the chlorindoptates.

Chloranile.—This body is extracted, by means of cold alcohol, from the product of the action of chlorine on chlorisatine. It is sometimes deposited, in the state of shining bracteæ, in the alcoholic solution into which the chlorine is driven.

Its composition is

$$C^{12} Cl^4 O^2.$$

It is insoluble in water and but sparingly soluble in cold alcohol. In boiling alcohol it dissolves with a yellow color, and crystallises on cooling, in shining bracteæ which much resemble iodide of lead. The solution is neutral to test paper. The acids have no action on it, even at the boiling temperature. The alkalis convert it into *chloranilic acid.*

It does not contain hydrogen. With heat, chloranile easily dissolves in a dilute solution of potassa, giving a purple liquid. This solution deposits, on cooling, brownish purple crystals, whose formula is

$$C^{12} H^2 Cl^2 O^4.$$

It is chloranilic acid.

This new acid is formed in chloranile in the same manner as benzoïc acid is derived from chloride of benzule.

$$C^{12} Cl^4 O^2 + H^4 O^2 = C^{12} H^2 Cl^2 O^4 + H^2 Cl^2.$$

The formula of chloranilate of silver is

$$C^{12} Cl^2 O^3, Ag O.$$

That of potassa is represented by

$$C^{12} Cl^2 O^3 PO + Aq.$$

Chloranilic acid is partly decomposed when heated in a tube: another portion is sublimed.

ACTION OF AMMONIA ON CHLORANILE.

If chloranile be heated with aqueous ammonia, it is slowly dissolved in it, without effervescence, giving a liquid of a deep blood red, which yields, on cooling, small flattened needles, of a chestnut color, and very brilliant. These crystals constitute *chloranil-ammon*,

$$C^{12} Cl^2 H^6, N^2 O^3,$$

which may be regarded as a combination of anhydrous chloranilic acid and anhydrous ammonia :—

$$C^{12} Cl^2 O^3 + N^2 H^6.$$

This body is analogous to the anhydrous sulphate of ammonia of Rose. Potassa slowly decomposes it into chloranilate of potassa which separates in the crystallised state, and ammonia which is liberated.

Chloranil-ammon dissolves in water with a purple color. The acids do not occasion any precipitate in its dilute and cold solution; they do not alter its color, which essentially distinguishes this body from chloranilate of potassa.

The solution of chloranil-ammon gives, with several metallic salts, precipitates which seem to resemble those produced by chloranilate of potassa, but which, nevertheless, essentially differ from them.

The concentrated acids attack chloranil-ammon, and produce a new body, crystallised in black needles which contain less nitrogen and hydrogen than chloranil-ammon. This composition is represented by

$$C^{24} Cl^4 H^6 N^2 O^6.$$

To furnish them, two atoms of chloranil-ammon,

$$C^{24} Cl^4 H^{12} N^4 O^6,$$

yield to the acids one equivalent of ammonia,

$$N^2 H^6.$$

This new body, called by the author *chloranilum*, and whose rational formula should be

$$2C^{12} Cl^2 O^3 + N^2 H^6,$$

acts with the alkalis nearly in the same manner as chloranil-ammon. It is very slowly decomposed by them, and is then converted into alkaline chloranilates.

With metallic solutions, it likewise gives precipitates which are identical with those occasioned by chloranil-ammon.

ON THE INSTINCT AND IRRITABILITY OF PLANTS.*

BY JOHN S. HILEY, A.B., M.B., OF ELLAND, NEAR HALIFAX.

THAT season being fast approaching when "the Lark to Heaven's Gate sings," and when the earth is about to be covered with the vivid verdure of spring, I take the opportunity of completing a Memoir on the Instinct and Irritability of Plants, which I have had in contemplation during most of the winter months. The field is one in which I have invariably felt considerable interest, surrounded as it is with so many surprising facts calculated to arrest the attention, and to awaken the dormant embers of speculation.

In following out this design, it is not my wish either to treat of the subject in detail, or to hazard speculations as to the cause of such phenomena, but rather to set before the reader a few of its more prominent features, and such as will excite his curiosity, and induce him to pursue it further. Here much remains to be examined, and a mine might be dug sufficiently valuable to enrich the labourer, and to store his mind with numerous interesting and instructive ideas.

In the furtherance of my purpose, I shall, in the course of this essay, adduce a catalogue of those indigenous productions in the order of their classes, which are gifted with any qualities approaching to instinct or irritability, at the same time subjoining any observations which I may have to offer respecting them, or which may suggest themselves in the course of my writing. Nor will those exotics, remarkable for their irritability, be passed over unnoticed, but will solicit attention in their proper places.

Prior, however, to adopting this plan, it will be advisable to say a few words as to those curious functions present in most plants, which do not, strictly speaking, come under the head of irritability, since by this term I wish to be understood as expressing certain motions which may he imparted to different parts of plants, as their leaves and flowers, by the operation of external agencies, such as the touch, a breath of wind, the influence of light and shade, &c.

The function alluded to is most aptly termed instinct. Respecting the irritability of plants, I shall be enabled to bring forward a variety of phenomena highly curious and interesting, with which most readers will be glad to become familiar, and which have never hitherto been collected together in a single memoir. These phenomena are principally confined to the leaves, stamens, pistils and seed-vessels; and accordingly, when treating of them, I shall first direct my attention to such as have any connection with the leaves, and afterwards proceed to consider those bearing upon the functions of the stamens, pistils and seed-vessels.

The instinct of vegetable products can have escaped the observation of few. It is often promineutly set forth in their roots and stems, and more particularly in the disposition which these evince to grow in different, and mostly opposite directions. The obstacles which the fibrils of the root will overcome to accomplish this end, have excited the wonder and astonishment of naturalists from the earliest times. One or two examples, illustrative of this point, will, however, best explain the nature and operation of this principle. Thus, if you place the germinating seed of a bean or pea in such a manner that the cotyledons shall be in the earth, and the radicle in the air, you will soon find the radicle curve downwards to the earth in order to sink into it. Again, in certain exotic vegetables, such as the *Clusia rosea*, &c., roots grow from the stem at a very considerable height, as well as from the branches, and descend perpendicularly, in order to sink into the earth.

M. Du Trochet and Mr. Knight attempted to destroy this tendency of roots, but without effect. Thus some seeds of a French bean were fixed in the nave of a wheel, kept constantly moving in a vertical plane by a stream of water. The seeds being placed in moss constantly moistened, began to germinate. All the radicles were directed towards the circumference of the wheel, and all the gemmules towards the centre. This experiment was varied, but with similar results.

If humid earth be placed above the seeds, and they even receive the influence of air and light from below, still the radicle will descend into the atmosphere, and the plumule mount upwards to the soil lying above it.

Though these experiments prove that the roots are directed towards the centre of the earth, and that their general tendency is such, yet some vegetables seem to be governed by other laws. Amongst these last we may class all parasitical plants. Thus the mistletoe (*Viscum album*) shoots out its radicle in whatever situation chance may place it. When adhering by its envelope of

* Communicated by the author.

viscid glue to the upper part of a branch, its radicle is perpendicular to the horizon ;—if it be applied to the under surface of the branch, the radicle rises towards the heavens; whilst if situated on the lateral surfaces, the radicle will be directed laterally. In a word, the seed of this singular parasite, finding the first materials of its growth in the viscid matter with which it is covered, will germinate in any situation, not only on wood—whether living or dead—but also upon stones, glass, or iron.

Again, the radicle of the mistletoe is constant in shunning the light, and this is beautifully exemplified by making its seeds to germinate on the inner surface of a pane of glass. In this case, all the radicles are directed towards the interior of the apartment. On the other hand, when fixed to the outer side of a window, the tenacity with which the radicles adhere and press upon the glass, as if wishful to get out of the light into the interior of the apartment, abundantly proves the truth of our statement.

In addition to the disposition which most roots evince to descend, they will often grow in the direction of veins of good soil, and extend beyond their usual limits in order to reach places where the soil is looser and more substantial. Du Hamel, wishing to protect a field of excellent soil from the roots of a row of elms which were enlarging that way, and wasting a part of it, caused a deep trench to be sunk, so as to cut the roots which were stretching into the field. Soon, however, the new roots, having reached one side of the ditch, bent downwards along its side to the bottom. There they took an horizontal direction, and afterwards ascended at the opposite side along the acclivity, and again extended into the plain. Vine roots will penetrate very hard soils, whilst elm roots are arrested by them. Roots will occasionally perform the office of a foraging party. Thus it has been observed that a plant, growing on the declivity of a rock, where an insufficiency of soil exists, will occasionally send off a rootlet to imbibe support from a small hillock of earth on a neighbouring projection. The plant, when this is effected, begins to thrive much more vigorously. Many other instances of this kind might be adduced. Plants vegetating on rocks, as lichens ;—upon walls, as the snapdragon, common wallflower, the red valerian ;—on the trunks or roots of other trees, such as ivy, certain tropical species of orchis, most of the mosses, broomrape, the hypocyst; insert their roots in them, and being parasitical plants in the strict sense of the term, derive from hence the materials of their nutrition. The Clusia rosea, a sarmentaceous shrub of South America before alluded to,—the Sempervivum arboreum ; the Indian corn, the mangrove, and some exotic figs, besides the roots which terminate them below, produce others from different points of their stem, which often descend from a considerable height, and sink into the earth. The tendency of the stem upwards is as powerful as that of the root downwards, and this fact is so evident that it will not require any illustration. It cannot, however, be denied that there are a few exceptions to this law. In the weeping ash and weeping horse-chestnut the branches have a downward tendency from their origin. There are moreover others in which the branches likewise descend, but this disposition is more apparent than real. I allude particularly to the weeping birch, weeping willow, &c., where the reversion is wholly occasioned by the effect of gravity upon the long slender rods which compose them.

In connection with this part of my subject, it will not be out of place to allude to the localities of plants.

Sandy districts are best calculated for such low trailing plants as the wind cannot uproot, or for those whose roots are very large and branching, whilst clayey heavy tracts afford the best support to such species as have small roots, and which do not require much earth. Hence arises in some degree the attachment which certain families evince for different localities. In addition to the mechanical characters of the soil, its chemical properties will also exercise a marked influence on the orders of vegetables which grow upon it. Hence wormwood will alone thrive where there is an abundance of potassa ;—grasses and corn plants require a soil rich in ammonia, potassa, lime and magnesian phosphates ; the Fuci must be near the sea, where they can obtain iodine and soda ; and those plants which are peculiar to dunghills, as the Chenopodium, require a supply of ammonia and the nitrates. All this holds true in nature.

Numerous other facts might be mentioned of the like character, but already more than enough has been said to show the correctness of the general statement as to the instinctive power of plants.

Having thus placed before the reader a few singular incidents in connection with the root and stem, indicative of a sort of foreknowledge, I now propose, in accordance with the plan already laid down, to turn my attention to similar curious phenomena often visible in the functions of the leaves. Here we have much scope for observation, and numerous instances will be adduced exemplary of the instinct and irritability of plants.

The leaves have always been considered as the lungs of plants, for it is by their surfaces that the processes of perspiration and absorption are effected, and, what is more to the point, it is here the changes wrought upon atmospheric air are carried on. That leaves give out moisture is proved by the withering of plants after being plucked, or cut from their parent stem. This, however, only happens when the leaf is exposed to the air; for if preserved in a tin box, where the perspiration can be checked or controlled, the freshness of the specimens continues for some time. Again, a faded plant may be revived by moistening with water, or placing it in a damp cellar.

The perspiration of plants is influenced by the state of the atmosphere, being increased when the latter is hot, dry, and blowy, and diminished when it is moist. Hence the difference in the suitableness of a day for hay-making. Evergreens are found to perspire much more than other shrubs; and succulent plants, growing on sunny rocks or in sandy deserts, imbibe with the greatest facility, and perspire very sparingly. The perspiration of some plants is exceedingly great. The large annual sunflower is stated by Dr. Hales to perspire seventeen times as fast as the ordinary exudation of the skin; and the cornelian cherry emits an amount of cuticular excretion in twenty-four hours, equal to twice the weight of the whole shrub. The leaf of this plant is thin and dry. The reason that evergreens resemble succulent plants in their perspirable powers, arises from their cuticles being nearly alike.

The kind of matter emitted varies. Thus in some, it is water alone; in others, it has a saccharine taste. In the lime-tree it is glutinous,—in poplars, resinous,—and in the Fraxinella, inflammable. In many cases, what would seem to be an exudation, is in reality a secretion. Hence we have manna, honey, and wax, each characteristic of some disease going on in the plant which yields it.

The absorption of plants is likewise worthy of note. This would seem to be effected principally by the leaves; and in some the upper surface absorbs most, whilst in others the lower surface. Aquatic plants have something peculiar in this respect; and here we have a remarkable instance of that wonderful care and skill which Providence has displayed in the construction of the members of this tribe. To use the language of an old phrase, the means are beautifully adapted to the end. In the Potamogeton, or pondweed, the absorbing power is as great as its disposition to perspire. So vascular are the leaves, that they dry almost immediately in the air, and wither. Hence the necessity for its growing in water, which it does, and in numerous instances almost immersed. It likewise absorbs as rapidly, and, as a consequence, its situation is rendered more natural. The Nymphææ float with the upper surfaces of their leaves exposed to the air, which surfaces will not retain water; and since they dry very quickly, it was desirable that this liquid should be supplied in sufficient abundance. They accordingly imbibe by their under surfaces, and perspire by the upper.

The Sarracenia, an American genus, and the East Indian Nepenthis distillatoria, demand especial notice. The former, which grows in bogs, though not wholly in water, has tubular leaves, which catch and retain the rain like a funnel or rain gauge. In the Sarracenia purpurea, the margins of this funnel seem dilated expressly for the purpose, whilst below, the tube is contracted to restrain the evaporation. This provision, according to Linnæus, was, as in the case of the Nymphæa, rendered for supplying a larger amount of water to the plant, and thus enabling it to thrive without being entirely immersed in the river or pond. The Linnæan hypothesis, however, can hardly be correct, inasmuch as some members of this class, particularly the Sarracenia adunca and Sarracenia flava, have their leafy funnels so constructed and closed at the summit, that little or no water can enter them from without. The hollow, however, does contain this fluid; and, according to the statements of the most accomplished botanical observers, it would appear to be secreted by the base of each leaf. A question at once arises as to the utility of such an appendage. The answer is this. An insect of the sphix or ichneumon kind has been seen by gardeners to drag flies of various sizes, and force them into these hollows, where they are shortly afterwards drowned. In the Sarracenia adunca, which was particularly the subject of observation, all the leaves were found crammed with dead or drowning flies. In the Sarracenia purpurea, the scent of their putrefaction is perceptible on passing the plant. In this instance it is worthy of remark, that the margin of its leaves are beset with inverted rigid hairs, which, like the wires of a mouse-trap, prevent the escape of any fly which may have fallen or have been pushed into the watery tube. Flies, however, are often tempted by the liquid to enter these receptacles of themselves, and hence it is extremely probable that they assist in the maturing of some part of the plant. Thus the air arising from their putrefying remains may be beneficial to vegetation. Add to this, the sphix or ich-

neumon stores up flies here as food for itself and its progeny, probably depositing its eggs in their carcases.

The Nepenthis distillatoria can boast of a somewhat similar contrivance. This plant also flourishes in swamps and bogs. Each leaf terminates in a sort of tuhe, with a cover over it like a tankard, capahle of holding from one to two ounces of fluid. Such fluid, with which the hollows are usually filled, is secreted by the footstalk, for which purpose its large spiral-coated vessels are admirahly adapted. The lid in question is also raised hy numerous insects and worms, whose instinct directs them hither in search of the water which these cups are known to contain. According to Rumphius, amongst all the worms, &c., which crawl into the orifices of these prison-houses, one little Squilla, or shrimp, with a protuberant back, alone survives. All the rest perish, while the shrimp in question feeds upon their carcases.

The leaves of the Dionæa muscipula and of the Drosenæ, in all likelihood, are constructed to catch insects for a similar reason. These, however, will he more particularly treated of when discussing the effects of touch, &c.

It is unnecessary to make any remarks as to the power which leaves exercise upon atmospheric air in purifying or contaminating it. The fact that the green parts of plants evolve oxygen during the day and ahsorb carhon, whilst during the night they emit carbonic acid and ahsorh oxygen, though highly important in a chemical and physiological point of view, is yet so well known as to require no comment here. I will, therefore, turn my attention to the influence which light exerts over most vegetables.

Few circumstances connected with their growth are vested with greater power; and we are not so much indehted for this part of our knowledge to the researches of Hales and Ingenhouz,—names so dear in the annals of science,—as to our own every-day observation.

Every one must have noticed the tendency which growing plants have to seek the light, and how poorly they thrive when partly or wholly excluded from its influence. Trees buried in the deep recesses of a forest, never look so vigorous as those flourishing on its outskirts; and it is notorious how plants and shrubs, when situated in dark places, as cellars, push forth their young shoots in the direction of any window, crack, or crevice, which may happen to admit the light. The green color of vegetables is dependent on the action of light: hence, celery and rhubarb become blanched when debarred of its aid; nor do they regain their green tinge until it is re-admitted. Many vegetables owe their poisonous qualities to the effects of light, and accordingly become innocuous when deprived of it. It acts beneficially upon the upper surfaces of leaves, and hurtfully to the under sides: hence the former are turned towards the light; and though hy nailing, &c., these may happen to be thrown in an opposite direction, they soon regain their true position. All the plants in a hot-house present the fronts of their leaves to that quarter from whence most light proceeds. Succulent leaves are peculiarly sensible to it. The mistletoe, the sides of whose leaves are alike in appearance, are both equally directed towards this agent, as are also upright sword-shaped leaves. Vine-leaves, separated from the parent stem, and suspended by a thread, turn with their upper surfaces to the light. This turning of leaves will occasionally operate on the hranches. Singular, however, as this effect of light may seem, it is not confined to the leaves alone.

Flowers are powerfully influenced; and perhaps nothing in the whole range of vegetahle economy is more striking. We see the phenomenon on a large scale in the syngenesious plants, particularly amongst those whose flowers have much color. Of these, numerous examples will hereafter be given.

To return to the leaves. Besides looking towards the light, they will often, like compound flowers, &c., follow the course of the sun through the day. Of this we have familiar instances in fields of clover, and others of the Leguminosæ. In a word, the leaves of the pinnated leguminous plants are, perhaps, more strikingly affected by light than any other parts. To this agency many of them owe their expansion; for when night comes they fold over each other, or droop as if dying. The singular property alluded to has been beautifully termed by Linnæus "The Sleep of Plants," since at this time they cease to he acted upon by the stimulus of light. In the acacia, the leaflets at sunrise are opened out horizontally; hut in proportion as the sun mounts above the horizon, these leaflets become more erect, and are at length nearly vertical. On the other hand, when the day is on the decline, they hegin to droop. Berthollet alludes to an acacia in Teneriffe, the leaflets of which closed in the evening and expanded in the morning, whilst its flowers shut at sunrise and opened at sunset. This disposition of leaves, especially compound ones, was taken advantage of by M. De Candolle. He succeeded in reversing the hours of sleeping and waking. Thus hy putting some of the Leguminosæ in a darkened cellar

during the day, and exposing them to a strong lamplight by night, he caused the specimens to close their leaves during the former period, and to expand them through the latter. In some cases, the nocturnal folding of the leaves is for the purpose of sheltering the flowers from the dew, which, as I shall hereafter show, is occasionally injurious.

Extraordinary, however, as all this may appear, it is surpassed by the influence which, in certain instances, the touch, &c. can exert. I have numerous examples to offer in support of this position, many of which are taken likewise from the great family of the Leguminosæ.

The pinnated members of this class are particularly liable to be affected by agencies which cannot be referred to the influence of light. Thus the slightest shock of the air gently agitated by the wind,—the shadow of a cloud or any other body,—the action of the electric fluid,—heat, cold, irritating vapors, such as those of chlorine, or nitrous gas,—are sufficient to make their leaflets perform the most singular motions. Compound and jointed leaves, *i. e.* those whose leaflets are attached by a joint to the common petiole, are such as exhibit the most remarkable movements, whether caused by light or touch. In the number of those plants whose leaves are thus affected, I may class the Mimosa sensitiva and pudica; Oxalis sensitiva; Smithia sensitiva; Dionæa muscipula; Hedysarum gyrans, &c. If you touch a leaf of the first, or sensitive plant, it becomes erect, and applies itself to that which is next above it on the petiole. All the others will soon perform the same motion, and thus they will cover each other like the tiles of a house. Soon after the entire leaf will bend towards the earth. If the cause cease to act, all the parts which appeared withered shortly resume their natural position.

That the touch has great power over many plants, we are nevertheless not confined to the sensitive plant alone for an example. The Dionæa muscipula, or Venus's flytrap, a native of North America, is equally remarkable, and affords an instance of the effect of touch. The extremities of the leaves of this singnlar plant are composed of two lobes, united by a kind of hinge; these contain three or four glandular points on their upper surface, to the presence of which they owe their irritability, for there is no other part of the plant possessed of such. When, therefore, an insect, or any other body, touches and irritates one of them, the lobes become suddenly erect, approach, and seize it. Moreover, their margins are furnished with bristles; so that when suddenly closing on any unfortunate insect, it is imprisoned like a rat in a gin. When the insect is dead, the lobes again unfold, and await the coming of another victim.

The Droseræ, or sundews, three species of which are natives of Britain, furnish other examples of the irritability of leaves under the influence of touch. The upper surfaces of the leaves of these plants are supplied with hairs of a considerable length, each of which would seem to be tipped with a glandular and viscous globule. Accordingly, no sooner has an insect settled upon the leaf, than it becomes entangled and retained by the glutinous substance of the gland. By and by the hairs betray signs of much irritability; during which state they curve inwards, and thus hold the poor insect secure.

The Hedysarum gyrans, a native of Bengal, has also very singular motions. Its unifoliate leaves are accompanied laterally by two smaller stipules, which last are gifted with a double motion of flexion and twisting on themselves. One may move rapidly, while the other is in a state of repose; and this motion is independent of any external stimulus, and continues through the night. The motion of the middle leaflet, on the contrary, appears to be caused by the action of light, and ceases when the plant is no longer exposed to it. Shadows, as I before observed, likewise occasion this peculiar motion, or "sleep of plants." Thus the leaflets of the Porliera approach and embrace one another whenever the sky becomes covered with clouds.

In the preceding observations I have endeavoured to display to the reader, in as intelligible a manner as possible, the influence which the light of the sun, touch, &c. have on the leaves of many plants. I will now, therefore, make some remarks, by way of explanation of the mechanism in connection with the subject, prior to entering upon the discussion of those signs of irritability peculiar to the flower. I have already stated the nature of those motions caused by the action of light, and consequently I need not subjoin any thing further.

In the case of the sensitive plant, the mechanism of its motions resides in the swollen joints placed at the foot of each leaflet or petiole. This is proved by cutting away the upper part of those swellings when the leaf continues erect; whereas if the lower part is removed, it remains depressed. The tissue composing the swollen joint has considerable elasticity; and that which occupies the upper portions of the joints in the case of the leaflets and partial petioles, is weakened by any stimulus which may be applied,

while the lower parts of those connected to the main petioles continues elastic as before. The stimulus is transmitted from one leaf to another along the stem, through the intervention of the ducts contained in the woody parts. Such is shown to be the case by abstracting the wood through which the ducts circulate, when the effects are checked; whereas if the pith and cortical portions alone are removed, they do not cease.

In the instance of the Dionæa muscipula, the two fleshy lobes which contract when irritated, are united together by a species of hinge, and are furnished with ciliæ or bristles round their margins. The action of these ciliæ has been already explained. A question here arises as to the object of such provision. Nor is this inquiry confined to the Muscipula Dionæa, but extends equally to the Droseræ,—to the Sarracenia,—to the Nepenthis distillatoria, and to all the plants which either catch insects by the irritable and delicate mechanism with which they are provided, or which, like the Sarracenia, &c., drown them in their leafy cups containing water. The only plausible conjecture which has been offered has been furnished by Mr. Knight, in his remarks on this contrivance, as seen in the Dionæa muscipula: and whilst it affords us a satisfactory explanation in this instance, I consider it is one which will apply with equal force and correctness to all the other cases that have been mentioned. Mr. Knight supposes the Dionæa muscipula to require animal manure for the healthy performance of certain functions, and accordingly that nature has thus provided it with the means for procuring it. In corroboration of this opinion, Mr. Knight, after preventing certain plants from supplying themselves with flies, furnished some with scraped beef, and left the rest without any such provision. The result was, that the first flourished much better than the second, thus affording the most incontrovertible proof of their demand for animal matter.

The curious properties of the leaves, &c. which have now been brought forward, cannot but create astonishment in the minds of those who were previously unacquainted with them; and, however trifling they may appear at first sight, we may rely upon it that they have been given to the plant for some useful end. And as it is true that the hand of the Almighty has fashioned the sun, the moon, and the stars, so hath He directed his fatherly care to the formation of all the trees, shrubs, and vegetables with which this lower world is adorned.

In my next communication I will resume the subject, by treating of the instinct and irritability of flowers, &c.

[Although, properly speaking, the subject of this paper is one rather of vegetable physiology than of chemistry, we have given insertion to it on account of its interest, and we think that our readers will not regret that we have done so.—Eds. Chem.]

ON THE REPULSIVE ENERGY OF GASES.

BY REUBEN PHILLIPS.

According to the law discovered by Boyle, the volume of gaseous matter is as the pressure to which it is subject, and it has been considered as due to a self-repulsive energy possessed by gaseous particles, which, at a constant temperature, increases as the proximity of the particles. I shall endeavor to show in this paper, that the repulsion which maintains gases, is equal at all pressures at the same temperature.

Thus, if a volume of gas, at a fixed temperature, be subjected to a double pressure, its particles are approximated by half their previous distance, whereby twice the number of atoms bear on the surrounding matter, with the same individual power, which half that number did, when the gas occupied its former space; consequently the pressure on a given amount of surface becomes doubled.

Again, if a volume of gas, at a constant temperature, be expanded to twice its bulk, the number of atoms, exerting their force on a given amount of surface, become diminished by one half, and the apparent expansive power of the gas is decreased in the same ratio.

To prove that gases exert their power on surrounding bodies only at particular points, and that other bodies may exert themselves in those voids, the following will suffice:—A mixture, consisting of equal volumes of two gases, at a fixed temperature and pressure, not susceptible of combination at the temperature of the experiment, occupies precisely the same volume as before mixture; and as one species of gaseous matter has no repulsive force with regard to another species, therefore either gas occupies twice its previous volume, and they conjointly sustain the former pressure; for as the pressure on the mixture is equal to twice the pressure on either gas, consequently, half the pressure on the mixture must be supported by either gas mutually acting each in the interstices of the other.

If gases are considered as acting equally on all points, by what power is this equality maintained, their molecules not being in contact?

If gaseous repulsion centred in the atom,

Boyle's law would be very different from what it is: the real repulsion would then be inversely as the square of the distance; the apparent, deducible from the expansive energy of the gas, would be as the cube.

The admission, that equal temperature and pressure are capable of maintaining all gaseous particles at an equal distance affords an explanation of the hitherto unaccountable fact, that however distant the boiling points of gases may be, their combining volumes are always in the simple ratio pointed out by Gay-Lussac. But in explaining gaseous combination by this principle, we are led to adopt the views of Ampère, namely, that a molecule of any substance contains a multiplicity of indivisible atoms; for it will be seen by the following example, that the contrary does not apply:—Supposing equal volumes of chlorine and hydrogen gases to consist of single atoms dispersed through a vacuum, by mutual combination, the number of atoms are reduced to one half, then, at the same temperature and pressure under which they existed previous to combination, the volume which the compound should occupy, according to this theory, should be equal to the volume of one of its constituents taken before it combined; but experiment gives only half this density. It may be argued, that such an effect may be anticipated; for as combination does not decrease the size of the combined atoms, they press on the same extent of surface as before they united: such a view is, however, inadmissible, as, according to it, all gases should possess the space of their components.

Now, suppose that each molecule of chlorine and hydrogen consists of a plurality of atoms,—for the sake of simplicity let it be two,—then a molecule of chlorine and a molecule of hydrogen, by uniting, form two atoms of hydrochloric acid, which require the same space as the two molecules out of which they were formed; this agrees with experiment. If the number of atoms in a molecule of one gas be twice that existing in a molecule of another, the combining volume of the latter gas will be to the former in the ratio of 2 to 1. If a molecule of a compound gas be composed of two molecules of its constituents, the compound will occupy half the volume its components did before they united; if composed of three molecules (2 of one + 1 of the other), only one third of the volume, and so on.

ON THE ATOMIC WEIGHT OF CARBON.*

BY MM. ERDMANN AND MARCHAND.

One of the most important bases of the calculations in organic chemistry, and which numerous experiments seemed to have established beyond the possibility of dispute, has recently heen shaken by MM. Dumas and Stas. These chemists have shown, by a series of experiments based on what appeared to be a faultless principle, that the atomic weight of carbon should be altered from 76·43 to 75·0.

This number, at first sight, seemed to be opposed to the results of the most skilful chemists, who have calculated their analyses according to the old weight, without perceiving any error: it is especially contradictory of the former determinations of Berzelius, who, on the occasion of some doubts as to the entire accuracy of the weight established by the Swedish chemist, expressed by M. Dumas, repeated his experiments, which were confirmatory of his former ones.

Matters being in this position, we decided on repeating the experiments of Dumas and Stas. We voluntarily admit that we set to work with much diffidence; but we must say, our results in every respect agree with those of the French chemists.

The problem which it was requisite to resolve, was to burn in oxygen a given weight of pure carbon, and to weigh the carbonic acid produced, in order to ascertain the exact proportion in which the carbon combines with the oxygen.

Our apparatus was constructed entirely like that of Dumas and Stas. Having ascertained that it was susceptible of certain simplifications, we stopped at the arrangement which we are about to describe, and which we adopted in our combustions of the diamond and graphite.

The matter to be burned was put into a platinum capsule, and this capsule in a larger one. A platinum wire was fixed to the handle of the latter, in order to enable us to push it further into the combustion tube, and to draw it out again, after the operation, without removing any oxide of copper with the residue. Afterwards the capsule was pushed towards the middle of the porcelain tube, the anterior half of which was previously filled with oxide of copper and calcined copper turnings—the latter occupying the front of the tube. We found that the use of a second tube filled with oxide of copper, to burn the oxide of car-

* *Annales de Chimie et de Physique*, Decembre, 1841.

bon formed by the combustion of graphite or the diamond, was superfluous; for we ascertained that none is formed. The posterior part of the combustion tube was placed in a Liebig's furnace, and communicated with a gasometer of oxygen closed by lime water. The oxygen was obtained from chlorate of potassa and was purified before going into the tube, by means of sulphuric acid contained in a bulbous tube and pieces of potassa lodged in a tube two feet long. The gas of the combustion traversed, before being collected, a layer of chloride of calcium; it was absorbed by means of two bulbous tubes filled with a perfectly saturated solution of potassa, and to which were also adapted two U tubes filled with pieces of potassa. The last tube was used only to prevent the access of carbonic acid and atmospheric moisture; consequently, we did not weigh it. The pieces of potassa contained in one of the branches of the side of tbe combustion tube were, for the first two tubes, moistened with concentrated caustic potassa.

Before proceeding with our experiments, we ascertained experimentally, that the oxygen did not occasion excess in the weights, if we took care to displace this gas by a current of atmospheric air.

The balance which we used turned with $\frac{1}{10}$ of a milligramme, when charged with 100 grammes.

After having arranged all the apparatus, we commenced by gently passing oxygen into the combustion tube; then, after having surrounded it with burning coals, we regulated the current of gas in such a manner as to allow very little free oxygen to escape. The operation occupied about three hours. The combustion being finished, we continued the current of oxygen for some time longer: finally, after baving removed the coals, we closed the stop-cock of tbe gasometer, and adapted another gasometer for the purpose of passing atmospheric air into the apparatus. It may easily be seen that all the oxygen was displaced by the air, when the last tube of potassa presented no more difference of weight.

The samples of diamond which we used were for the most part cut. They were yellowish, or of the color of smoke. Before weighing them, we slightly calcined them.

The diamond burns, as Dumas and Stas have indicated, with much facility, from the time that the porcelain tube becomes red hot; the bubbles of gas were almost entirely absorbed in the bulbous tube. Our samples left only a very small quantity of ashes.

Our graphite was likewise very pure. We submitted it to the purifications indicated by Dumas and Stas. It always left a small quantity of incombustible residue, chiefly composed of silica and unburnt graphite.

Experiments I., II., III., IV. and V. were performed with a diamond; VI., VII. and VIII. with natural graphite; and IX. with artificial graphite.

The following is a detail of these experiments:—

No. of Exp.	Matter employed.	Ash.	Carbon burned.	Increase of Weight of the first bulbous Tube.	Increase of Weight of the second bulbous Tube.	Increase of Weight of the first Tube with Pieces of Potassa.	Increase of Weight of the second Tube with Pieces of Potassa.	Total Carbonic Acid collected.	Atomic Weight of the Carbon.
I.	0·8062	0·001	0·8052	2·9410	0·0028	0·0029	0·000	2·9467	75·19
II.	1·0867	0·0009	1·0858	3·9746	0·0097	0·0032	0·000	3·9875	74·84
III.	1·375	0·0018	1·3057	4·9573	0·0045	0·0023	0·0018	4·9659	75·10
IV.	1·6330	0·0025	1·6305	5·96895	0·00615	0·0030	0·00155	5·97945	74·98
V.	0·7510	0·0010	0·7500	2·7425	0·0025	0·0020	0·0020	2·749	76·03
VI.	1·5746	0·037	1·5376	5·6095	0·0234	0·0029	0·0009	5·6367	75·02
VII.	1·6578	0·0084	1·6494	6·0185	0·0183	0·0015	0·0001	6·0384	75·18
VIII.	1·4580	0·0075	1·4505	5·30325	0·0090	0·0025	0·0010	5·31575	75·05
IX.	1·9040	0·0105	1·8935	6·9235	0·0075	0·0035	0·0010	6·9355	75·10
					Mean				75·087

To these experiments we may add some which were made on certain organic matters.

Benzoïc Acid.—It was very carefully prepared from the urine of the horse, and was of great purity.

0·924, dried *in vacuo*, gave 0·414 water, and 2·332 carbonic acid.

	Old Atomic Weight.		New Atomic Weight.	
	Calculated.	Analysed.	Calculated.	Analysed.
Carbon . . .	69·26	69·78	68·86	68·83
Hydrogen . . .	4·86	4·97	4·91	4·97

Another analysis gave 69·79 of carbon, according to the old atomic weight.

Cumamic Acid.—Prepared by means of storax, purified by distillation, pressed between blotting-paper soaked in alcohol, and crystallised several times.

0·832, dried *in vacuo* at 100, gave 0·406 water, and 2·225 carbonic acid.

	Old Atomic Weight.		New Atomic Weight.	
	Calculated.	Analysed.	Calculated.	Analysed.
Carbon	78·38	78·94	72·96	72·93
Hydrogen . . .	5·33	5·43	5·40	5·43

Oil of Cloves, not purified.—15·775,—whose combustion occupied five hours,—gave 1·68 water, and 5·100 carbonic acid.

	Old Atomic Weight.		New Atomic Weight.	
	Calculated.	Analysed.	Calculated.	Analysed.
Carbon	88·46	89·37	88·25	88·24
Hydrogen . . .	11·54	11·82	11·75	11·82

Two other analyses gave 88·15 and 88·09 carbonic acid, according to the new atomic weight.

Naphthaline.—0·654 of sublimed and purified naphthaline gave 0·399 water and 2·244 of carbonic acid.

	Old Atomic Weight.		New Atomic Weight.	
	Calculated.	Analysed.	Calculated.	Analysed.
Carbon	94·87	93·87	93·58	92·76
Hydrogen . . .	6.27	6·13	6·27	6·24

All these experiments confirm the new atomic weight, 75, established by Dumas and Stas.

COMPOSITION OF WOLFRAM.*

BY F. SCHAFFGOTSCH.

A SERIES of experiments performed on specimens of wolfram procured from different localities, with the view of determining the variations of the proportions of oxides of iron and manganese presented in these minerals, led me to a much more interesting and entirely unexpected result.

I ascertained:—

1. That the sum of the ponderal quantities of the bases is always greater than it can be in a neutral tungstate of iron and manganese:

2. That the quantity of tungstic acid given by analysis is uniformly greater than that calculated according to the generally admitted composition; so that, finally, an excess of weight amounting to some hundredths is obtained.

These remarks, supported by numbers obtained in direct analyses, led me to ascertain that the wolfram does not contain tungstic acid, but oxide of tungsten.

The following, in few words, is the manner in which the analyses were made:—

The mineral, reduced to a very fine powder, was fused with three times its weight of carbonate of soda. On treating it by water, there remained the mixture of the two oxides of iron and manganese, which were redissolved in hydrochloric acid, and separated by the ordinary process, by means of ammonia and succinate of ammonia. The tungsten existed in the liquor in the state of tungstic acid; but as it is almost impossible to completely separate this acid from its combination with soda, or from its sulphosalt, by the acids, I was obliged to seek some other means. For this purpose, finely powdered wolfram was treated by boiling hydrochloric acid: this requires to be renewed several times, and the matter should be considered completely attacked only when the deposit is of a very pure canary yellow. This deposit is tungstic acid: it was collected on a filter; the liquor, precipitated by an excess of ammonia and hydrosulphate of ammonia, was evaporated after filtration. The residue, burned

* *Poggendorff's Annalen.*

in the air, gave the small quantity of tungstic acid which had been dissolved by the hydrochloric acid. I likewise tried fusing wolfram with chloride of calcium, thus separating the tungstate of lime formed. This process succeeded without difficulty.

The following tables contain the results of the analyses :—

I. WOLFRAM OF MONTE VIDEO AND EHRENFRIEDERSDORFF.

	Calculated according to the Formula $MnOTuO^2 + 4FeOTuO$.	Monte Video.	Ehrenfriedersdorff.	Calculated according to the Formula $MnOTuO^3 + 4FeOTuO^3$.
Protoxide of Iron .	19·26	19·24	19·16	18·26
Protoxide of Manganese .	4·89	4·97	4·74	4·63
Tungsten and Oxygen .	75·85	75·89	76·10	77·11
	100·00	100·00	100·00	100·00

II. WOLFRAM OF CHANTELOUPE.

	Calculated according to the Formula $MnOTuO^2 + 3FeOTuO$.	Found.			Calculated according to the Formula $MnOTuO^3 + 3FeOTuO^3$.
		I.	II.	III.	
Protoxide of Iron .	18·06	17·81	18·33	17·71	17·12
Protoxide of Manganese .	6·11	6·20	5·67	6·29	5·79
Tungsten and Oxygen -	75·83	75·99	76·00	76·00	77·09
	100·00	100·00	100·00	100·00	100·00

III. WOLFRAM OF ZIRMWALD.

	Calculated according to the Formula $2FeOTuO^2 + 3MnOTuO^2$.	Found.					Calculated according to th Formula $2FeOTuO^3 + 3MnOTu$
		I.	II.	III.	IV.	V.	
Protoxide of Iron .	9·62	9·55	9·49	9·50	,,	24·13	9·12
Protoxide of Manganese .	14·64	15·12	14·85	,,	14·57		13·89 inf
Tungsten and Oxygen .	75·74	75·33	75·66	,,	,,	75·87	76·99
	100·00	100·00	100·00	,,	,,	100·00	100·00

IV. EXCESS OF WEIGHT IN A HUNDRED PARTS OF WOLFRAM.

Wolfram of Chanteloupe.	Wolfram of Zirmwald.		Calculated by the Formula R O Tu O^2.
Analysis III.	Analyses I. and VI.	Analyses V. and VII.	
4·5	4·4	6·6	5·5

These analyses lead to the following conclusions:—

1. Wolfram is formed by two different combinations;—the first consisting of one atom of protoxide of iron, Fe O, and one atom of oxide of tungsten, Tu O^2; the second, of one atom of protoxide of manganese, Mn O, and one atom of oxide of tungsten, Tu O^2:

2. These two combinations occur in different proportions in specimens of wolfram coming from various localities; but these proportions always present simple atomic relations.

PREPARATION OF HYDROTELLURIC ETHER.*

BY F. WÖHLER.

The existence of this combination is very interesting; it gives a new proof of the remarkable analogy which exists between sulphur and tellurium, and shows us that, like sulphur, it is capable of entering into the constitution of organic compounds, and of replacing oxygen in them.

This ether is easily prepared by the double decomposition of sulphovinate of baryta and telluret of sodium: for this purpose, it is sufficient to distil a mixture of these two substances dissolved in water. The telluret is prepared by calcining tellurium, or native telluret of bismuth, with carbonate of soda mixed with charcoal: and, to avoid oxidation, the product is immediately put into the retort which contains the boiling solution of sulphovinate. The hydrotelluric ether distils over with the water, giving, at the commencement, much scum in the retort.

This substance forms a liquid of a reddish yellow almost of the color of bromine, but more clear; it is heavier than water, which dissolves only traces of it; its odor is disagreeable, extremely penetrating, and remains a long time; it resembles that of both hydrosulphuric ether and hydrotelluric acid; it appears to be very poisonous. Its boiling point is lower than 100° C.

This ether readily takes fire, and burns with a white brilliant flame, terminated by a blue of a very peculiar appearance, and spreading a very thick white smoke of tellurous acid. The liquid exposed to the air was speedily covered with a pellicle, yellowish at first, but which afterwards became white, and it was ultimately converted into tellurous acid. If it be exposed in a covered vessel to the direct action of the solar rays, fumes are immediately seen to arise, and its oxidation becomes quicker under the influence of diffused light. However, even in pure oxygen gas, the oxidation is not sufficiently energetic for the liquid to take fire. Nitric acid attacks it vividly, with a disengagement of oxide of nitrogen, and dissolves it; if hydrochloric acid be added to this solution, a heavy, colorless liquid, in oily drops, is separated, which has not yet been examined.

Hydrotelluric ether, according to calculation, should contain 68·53 of tellurium; analysis gave 68·75.

Indeed, 0·560 of ether were dissolved in nitric acid, hydrochloric acid was added to it, then it was boiled for a long time, after which sulphurous acid separated 0·385 of metallic tellurium, which was dried *in vacuo*. Thus this product is simple hydrotelluric ether, $C^4 H^{10} Te$, and is composed of

Carbon . . .	26·14
Hydrogen . .	5·33
Tellurium . .	68·53
	100·00

The product corresponding to mercaptan would contain 81·0 per. cent. of tellurium.

ON THE ACTION OF ANHYDROUS PHOSPHORIC ACID ON ANHYDROUS CAMPHORIC ACID.

BY P. WALTER.

The action which anhydrous phosphoric acid exerts on anhydrous camphoric acid does not resemble that of the sulphuric acid of Nordhausen. Anhydrous sulphuric acid acts, if I may so express myself, in a less destructive manner; it reacts according to the laws of substitutions. The elements of sulphurous acid take the place of the carbon removed from the camphoric acid,

* *Poggendorff's Annalen.*

and from this reaction a new acid is produced; while by acting with anhydrous phosphoric acid on anhydrous camphoric acid, it is attacked in all its particles, and gives rise to several compounds. Indeed, if we form several layers of anhydrous phosphoric and camphoric acids in a tubulated retort, furnished with a receiver with two openings, one of which is put in communication with a bent tube dipped in mercury, and if this mixture be cautiously heated, a considerable and continued disengagement of gas is remarked. When this disengagement is terminated, a condensation of liquid in the neck of the retort is perceived, and there flows into the receiver, a fluid, yellowish body, of a penetrating but not disagreeable odor, which, after several rectifications over anhydrous phosphoric acid, may be obtained quite colorless. At the bottom of the retort there remains a black and powerfully acid matter.

The gas which is disengaged during this action is of a complex nature: it is a mixture of carbonic acid and carbonic oxide; but in this mixture the gases exist in definite proportions. Many experiments have shown me that it consists of one volume of carbonic acid and four volumes of carbonic oxide.

The liquid is a hydrocarbon: two analyses, performed on two different preparations, gave the same quantity of carbon—88·4 and 88·2 per cent.; but the quantity of hydrogen varied about ½ per cent.; in one analysis I obtained 11·6, in another 11·07. If the first be correct, the hydrocarbon presents the composition, in hundredths, of essence of turpentine; but I do not think that it is essence of turpentine, although its formation may be at the expense of the formula of the anhydrous camphoric acid. I should rather be led to think that it is a species of naphtha, which contains 89·0 per cent. of carbon, and that the small quantity of carbon found in my analyses is owing to the presence of a little phosphuretted hydrogen, which it is difficult to separate from the hydrocarbon. In the latter case, to explain the formation of this hydrocarbon, by means of the formula of anhydrous camphoric acid, the formation of water must be admitted in the reaction. I should long since have decided this, if the quantity of hydrocarbon obtained in this reaction had been more considerable than it is.

ON TWO NEW MINERALS, ÆGIRINE AND TITANATE OF IRON, FROM SCANDINAVIA.*

BY P. PLANTAMOUR.

These two new minerals were recently analysed by M. Plantamour, in Berzelius's laboratory, at Stockholm.

The first was found in the rocks of sienite, in the vicinity of Brevig, in Norway, by M. Esmark, who gave it the name of *ægirine*, from Ægir, the Scandinavian god of the sea. Its external appearance is that of amphibole. According to M. Plantamour, it belongs to the Arfwedsonite species established by Von Kobell, in which the lime and magnesia of amphibole are substituted by a fixed alkali. Its color is variable from black to brown and green; its transversal fracture is full of asperities, arising from the unequal rupture of the lamellæ determined by the faces of the cleavage. Heated on charcoal, it fuses into a black, opaque and vitreous flux. Its analysis gave—

Silicic acid	46·571
Alumina	3·413
Titanic acid	2·017
Magnesia	5·878
Lime	5·913
Potassa	2·961
Soda	7·790
Manganoso-manganic oxide	2·068
Oxide of iron	24·384
Fluorine	traces
	100·995

By subtracting the protoxide of iron, which belongs to the titanate of iron, it is found that this mineral is an aluminiferous bisilicate of the ordinary bases of amphibole, to which are added potassa and soda, and in which a small quantity of silica is replaced by alumina.

The second mineral was a titanate of iron from the neighbourhood of Uddevalla, greyish-black, fracture compact, without any appearance of crystallisation. Heated alone before the blowpipe, it fused into a steel-grey flux, which, after cooling, showed traces of crystallisation. Its analysis yielded—

Titanic acid	15·5598
Protoxide of iron	11·3210
Peroxide of iron	71·2478
Fluorine, silica, and loss	1·8714
	100·0000

* *Bibliothèque Universelle*, No. 64.

METHOD OF OBTAINING THE IODIDE OF COPPER.

BY REUBEN PHILLIPS.

Dissolve iodide of potassium in a small quantity of a concentrated solution of hydrochloric acid, and add to it sulphate of copper, separately dissolved in a small portion of the same solvent; the cupreous iodide

separates under the form of a dark brown powder, possessing something of an olive hue. It is an exceedingly unstable compound, and all attempts to isolate it have been unavailing.

ON RESIN OF GUAIACUM.*

BY M. DEVILLE.

Resin of guaiacum gave, by distillation, three distinct substances:—

1. An oil boiling at 117° C.:
2. An oil boiling at 212° C., more dense than water, while the other is lighter:
3. A crystallised substance, volatile without decomposition.

This work, whose results would appear, at first sight, to have great analogy with those which I have published concerning balsam of tolu, made me hope that I might find in these two resins, analogous, if not the same principles. Knowing that several chemists are now studying guaiacum, and being desirous of preserving priority for investigations long since commenced with regard to all the resins, and guaiacum in particular, I have communicated these results, although incomplete, in the hope that I shall very soon terminate them and submit them to the Society.

M. Pelletier informed the Society that he also was engaged in the examination of guaiacum, and that he discovered in that substance the presence of two resins, one of which was capable of combining with the alkalis, while the other did not possess this property. Having submitted guaiacum to distillation, he obtained, like M. Deville, three different substances:—

1. A light oil:
2. A crystallisable matter, perfectly clear, which volatilises with the aid of steam:
3. A dense oil, which appeared to him identical with creosote.

CRYSTALS OF THE ESSENCES OF TURPENTINE AND CITRON.†

BY M. DEVILLE.

M. Deville presented to the Philomatic Society of Paris, at its meeting of the 27th of November, 1841, two new products which he had recently obtained under the form of crystals, remarkable for their beauty, transparency, and lustre.

The first was hydrated essence of turpentine, and the second, essence of citron.

He read the following observations on this subject:—

M. Wiggers announced that he had obtained fine crystals of the former substance by means of a mixture of alcohol, nitric acid, and essence of turpentine. I have repeated this experiment, which perfectly succeeded, and I obtained two or three hundred grammes of substance at the end of a month of contact between two kilogrammes of the mixture. In an analogous manner, I was able to prepare the hydrated essence of citron, not previously known.

These two substances crystallise with remarkable distinctness. They are isomorphous, and their forms are right rectangular prisms. They are likewise isomeric, and their formula is $C^{40}\,H^{32} + H^{12}\,O^6$, a formula which Dumas and Peligot had already assigned to hydrated essence of turpentine.

It appeared to me that there should also exist a liquid hydrate of the two essences.

Terebene, in the same circumstances, furnished a crystallised body. I have not yet obtained it in sufficient quantity for examination.

All these experiments, which have been commenced on a great number of essences, require a considerable time before they can be concluded. It is for this reason that I now introduce them to the notice of the Society, although they are yet incomplete.

ANALYSIS OF A SUBSTANCE SECRETED ON THE HAND OF A GOUTY PERSON.*

BY O. HENRY.

This substance showed itself after violent attacks of gout, under the form of a very thick, glutinous secretion, and was, as it were, sprinkled with a white matter. A very small quantity examined by Henry was formed of the following matters:—

1. A large quantity of albumen, amounting to $\frac{4}{5}$ of the whole:
2. Lactic and phosphoric acids:
3. Chloride of sodium and phosphate of lime:
4. Evident traces of urate of soda.

NEW SALT.

M. C. Boyer sent to the *Académie des Sciences*, Paris, at its meeting of Sept. 13, 1841, a sample of salt which he considered new, and the properties of which appeared to him to be worthy of the attention of chemists.

This salt was obtained by treating with boiling nitric acid and potassa the crude product arising from the action of sulphuric acid on essence of turpentine. It occurs in colorless and transparent crystals. Left in

* *Société Philomatique de Paris;* Nov. 27, 1841.

† *L'Institut*, No. 416.

* *Journal des Connoissances Médicales.*

a damp place, it loses its transparency in a few days, and effloresces of a yellow color, after six or seven months. It does not fuse on incandescent coals, but decrepitates in the same manner as sea-salt. It dissolves in water, precipitating a yellow body; the supernatant liquors, evaporated, give nitrate of potassa with all its known properties. The yellow precipitate carbonises in the fire, which proves its organic origin. It is very sparingly soluble in alcohol and essence of turpentine, which distinguishes it from the resins: it very readily dissolves in nitric and sulphuric acids, giving colorless and limpid solutions. If we add to these properties, those of being insoluble in water, of changing the characteristic properties of nitrate of potassa, and of being found in that salt crystallised, causing it to lose its transparency, and without communicating to it its purple color, we are led to conclude that it is a basic body.

The manner in which this yellow precipitate acts with potassa is somewhat remarkable. If it be treated by a concentrated solution of potassa, it does not dissolve, and its color increases in intensity, while by diluting it with water, its color gradually weakens, until it becomes white, and it then constitutes a limpid and colorless solution; its evaporation in the air gives colorless crystals, which readily turn yellow, and which, treated by sulphuric acid, are dissolved with effervescence, giving a colorless and limpid solution.

II. CHEMICAL MANUFACTURES.

ABUSES OF THE ALKALI TRADE. CAUTION TO SOAP-MAKERS.

Bonis nocet quisquis pepercerit malis.
LABERIUS.

As protector of the rights of the Chemical Manufacturer, our pen has not frequently been called into action, but we have ever evinced a desire to expose fraud, even when committed by those who would endeavour to conceal their dishonesty under the mask of respectability or undeserved reputation. To such exposure as is calculated to stop practices of a fraudulent nature, we are devoted, and we assure our readers that we always enter upon that part of our duty with heartfelt satisfaction.

In the present instance, it is with more than ordinary pleasure that we take in hand the cause of those who for years have been the victims of a system, which requires only to be exposed to be arrested in its infamous progress. For this *exposé*, no period could be better chosen than the present—a period when the soap trade is greatly depressed, from a variety of causes, into which it forms no part of our present purpose to enter.

Our readers may be aware that soap-makers consume, in the manufacture of soap, large quantities of the alkali called in commerce "soda ash," for which they pay according to the quantity of soda combined with carbonic acid,—which is all that is available for their purpose,—that it contains: this quantity is estimated by analysis. Soap-makers purchase their alkali of a broker, who is the medium between them and the manufacturer. In the contracts by which alkali is bought, the broker not unfrequently inserts a clause to the effect that it is to be paid for according to "the broker's analysis,"—a clause to which no soap-maker should ever agree; when he does, he volunteers to be defrauded.

These brokers, — on whose ANALYSIS (save the mark!) the soap-maker is to place implicit reliance, and against which it were worse than sacrilege to raise the slightest breath of suspicion,—are not unfrequently the holders, the actual owners, *under the rose*, of the article to be sold at *their own valuation*; and it is this circumstance that renders them so tenacious of the privilege. Even when not holders, they frequently connive with and aid the manufacturers in defrauding the soap-maker: indeed, the very fact of their being employed by the former should operate as a cause for suspicion in the mind of the

latter. We confess that our confidence in the chemical knowledge and analytical accuracy of these men is about as great as—nay, if possible, less than—our faith in their honesty.

The quantity of soda contained in the alkali of commerce is ascertained by testing it with sulphuric acid, of a strength determined by its saturating power with regard to the crystallised soda of commerce, which is variously stated to contain from 21·5 to 22·2 per cent. of real soda. From very extensive experience, we are inclined to believe that truth lies between the two. This standard, therefore, being a disputed one, should be altogether abolished. There are others, well known to chemists, and to which we always have recourse, which ought to be substituted for it, and concerning whose accuracy not a shadow of doubt can exist. Crystals of soda contain from *one* to *ten* equivalents of water, and a quantity, chiefly composed of crystals, containing ten equivalents, may be mixed with a small portion in which there are one, two, three, or four, &c. Another cause of error is the well known fact that crystals almost always contain uncombined water between their plates or layers. Moreover, the crystallised soda of commerce is never absolutely free from small quantities of sulphate of soda, chloride of sodium, (common salt,) &c.*

If the soda to be tested were perfectly pure, this standard might be considered more admissible; but the soda of commerce, in the manufacture of which bisulphuret of iron (iron pyrites) is used, always contains one or two per cent. of iron, about one per cent. of sulphite of soda, and a portion of lime and sulphuret of sodium, which absorb a certain quantity of the test acid, and for which the manufacturer makes no allowance to the purchaser, to whom they are worse than useless.

Considering, as we do, that this standard is inaccurate, and that it cannot be relied on, we reject it *in toto:* not so, however, the brokers; these worthies adopt it for the very reason that we repudiate it, and hail its inaccuracy as a recommendation. To explain to them more accurate methods were utterly useless: these are seldom suited to their comprehension, and never to their inclination.

From innumerable instances, we select the following as average examples: the first may be regarded as a specimen of their pitiable ignorance, the second as an illustration of their flagrant dishonesty:—

1. A sample of alkali, from the same cask, separated into two portions, was sent for analysis to one of these brokers, who is looked up to by his wondering brethren as an "authority:" the difference between the two amounted to only *six* or *seven* per cent.

2. A soap-maker purchased a quantity of alkali from another of the trade, stated, on the faith of the broker's analysis, to be of a certain per centage, which we forget; the second purchaser, finding that the alkali in question "worked short," as it is technically termed, sent it to the above-named "authority" to be analysed, who returned it about 6 per cent. short; this analysis was shown to the first purchaser, *who had bought it from the very man to whom the second took it to be analysed*, which worthy, of course, said that there must be some mistake in the *second* analysis, and requested to be furnished with another sample. This was acceded to, and he contrived to make it very near his original analysis.

When disputes arise between soap-manufacturers and the brokers, the matter is always referred to a third party, by whose decision both are to abide. This umpire is not unfrequently as venal as those who employ him, and consequently as little to be trusted.

In cases of dispute, when the soap-maker has the alkali tested by a person who is not leagued with these men, and who gives his opinion from analysis, without knowing from whom it is purchased, or what per centage of alkali it is stated by the brokers to contain, the latter sometimes refuse to "recognise" him as an "authority." *They* recognise him! God help them. Science

* This we have found to be the case, from very numerous experiments.

must be indeed reduced to a low ebb before their recognition can avail it any thing.

For the accuracy of these statements, we can vouch: they are derived from personal knowledge. Truly, there are exceptions to our observations; would we could say, many; but these, we are sorry to say, bear a striking analogy to angels' visits. Those to whom we allude will not fail to recognise themselves in this faithful portrayal of their delinquency. Let those whom the cap fits wear it.

Having said thus much, we have *at present* little more to add. We caution soap-makers against purchasing alkali on the broker's analysis, or on that of any one who has not attained such a rank in chemical science as to place him above all suspicion. They must not suppose that every dolt who can drop test acid into an alkali, and say that he has used more than he really has, is a chemist: a greater fallacy does not exist.

We think that we have said sufficient to deter soap-makers from any longer placing reliance on alkali brokers or their tools. We call on them for assistance in putting down so gross a system of fraud, and they may depend on our activity in watching over their interests. We shall return to this subject as frequently as occasion may require.

NOTICE OF A NEW MODE OF GILDING METALS BY THE HUMID WAY AND THE VOLTAÏC CURRENT.

BY M. LOUYET.

(*Concluded from No. XXVII.*)

To produce the electric current, I use, as I have said, voltaïc couples varying in dimensions according to the extent of the surface of the objects to be gilded: thus, to gild small objects, such as thimbles, pieces of money, rings, &c., I employ a couple formed of a plate of copper of from 12 to 16 centimetres square, bent in such a manner as to form a double plate, as in Wollaston's piles, in the space of which I place a plate of amalgamated zinc, of half the dimensions of the copper plate, and entirely covered with a piece of coarse cloth, to prevent contact between the metals. Two copper wires of ordinary size are soldered to the two superior angles of the zinc plate; two other wires are adapted to the middle of each of the copper faces. The wires should be very long.

The voltaïc couple being thus arranged, it is entirely immersed in a glass vessel or wooden trough, filled with water strongly acidulated by sulphuric and nitric acids. I have remarked that the addition of nitric acid is not without importance, for it has much influence on the beauty of the gilding; it is probable that nitric acid acts thus only by greatly augmenting the energy of the electric current.

The object to be gilded, which may be of copper, brass, bronze or silver, is previously cleaned; it is afterwards placed in a porcelain or glass vessel, which must be carefully chosen of such a form as that the object may be entirely covered with the smallest possible quantity of the solution of gold. I afterwards fix in it, by bending the extremities, the negative reophori of the voltaïc couple, that is to say, those which communicate with the zinc plate, disposing near the piece the extremities of the positive reophori, in such a manner as to make them alternate with the preceding. The apparatus being thus arranged, the solution of gold is poured into the vessel containing the object to be gilded, so as entirely to cover it, and the operation is left to itself for a space of time varying in duration according to the thickness which it is desired to give the layer of gold. At the end of five, ten, twenty or thirty minutes, the piece is drawn out with the reophori; it is disengaged from the latter, washed with pure water so as not to lose any of the solution, and it is then dried.* This operation, several times repeated, gives a thick coat of gold. When a very solid gilding is required, after having once withdrawn the object from the solution, and having washed and dried it, it may also be submitted to a temperature of from 250° to 300° C.; then, when it is cold, it may again be subjected to the action of the solution and the electric current. On this subject I made an important observation: if two pieces of equal dimensions and of the same form, one of copper, and the other of silver, be exposed in the same solutions, for the same time, to the influence of electric currents of equal intensity; and if they be washed and dried, and placed on a sheet of

* This washing is necessary, especially in operating on the large scale, to prevent loss. To recover the gold contained in the washing waters, as also that which remains in the partially exhausted solutions, they are evaporated to dryness, and the residue is strongly calcined in a crucible. The gold is reduced, and fuses into a mass which is found at the bottom of the crucible.

platinum, whose heat is gradually raised by means of a spirit lamp with a double current of air; the yellow color of the piece of silver is seen to become gradually weaker, until it becomes perfectly white: if the two pieces be now steeped in very dilute sulphuric acid, the silver will retain its white color; the piece of copper, on the contrary, which in proportion as its temperature was raised, appeared to turn black, then to acquire variegated tints, in contact with the dilute acid, took a beautiful gold color, and some portions of the metal which covered it were detached in consequence of the sudden change of temperature. This difference of action probably arises from the different degrees of affinity which exist between copper, gold, and silver: the latter had really absorbed the gold: by again subjecting the piece of silver to gilding by the humid way, it is possible to arrive at a point where it may be strongly heated without losing its golden color; its surface is then in some measure saturated with the latter metal. It must be remarked that gilders are aware that silver requires, in the process of gilding by fire, twice as much gold as objects of copper or bronze.

As I have already observed, care must be taken to arrange the apparatus, in gilding by the humid way and the voltaïc current, in such a manner as that the electricity shall be uniformly distributed over the surface to be gilded; this is effected, when the piece is large, by terminating the poles of the voltaïc couple, by several reophori, of which all the negative ones are put in contact with the different points of the perimeter of the object to be gilded, and plunging into the liquid all the positive ones, which should be equal in number to, and correspond with, the negative. It is necessary to avoid, especially in acting on silver objects, all contact between the positive poles and the object to be gilded; for this reason it is requisite previously to arrange the conductors with the object to be gilded in an empty vessel, and the solution must be poured in only when this arrangement is ascertained to be complete. For if, at the commencement of the operation, contact takes place with one or more positive conductors, the piece, if it be of silver, turns black and acquires variegated tints; this effect arises from the piece having become positive, the sulphur of the dissolved sulphuret of gold combines with the silver with the greatest rapidity; this accident, when it occurs, is easily remedied by replacing the reophori in their proper situations, and rendering the contact between the negative conductors and the piece as intimate as possible; thus at the end of a few minutes the variegated tints are seen to disappear entirely, giving place to a layer of gold. This accident is presented more especially in operating with a very concentrated solution of gold. It is advisable to multiply the conductors as much as possible, in order to have a perfectly uniform gilding; for I have remarked, and it stands to reason, that those parts of the object are best gilded which are situated nearest to the positive reophori, and that the parts where the layer of gold is thinnest are those in contact with the negative conductors of the pile. It may be conceived that it must be so, by remarking that the object to be gilded, being only the continuation of the negative pole of the couple, it is particularly on the parts nearest to the positive conductors that the decomposition of the dissolved sulphuret of gold, and the consequent precipitation of the metallic gold, takes place with greater intensity; for it is there that the voltaïc current is strongest. This remark is important in the practice of gilding; for when few conductors are employed, care must be taken to turn the object at the end of a certain time, in order to produce a gilding of uniform thickness.

To give to the gilding all the beauty and lustre of which it is susceptible, the pieces are put in color, by steeping them for some minutes in a boiling solution of saltpetre, alum, and common salt, in proper proportions, or by heating them after having covered them with a layer of gilder's wax.

I tried to repeat the operations of gilding by the humid way, above described, by substituting for the isolated voltaïc couple a pile with troughs of thirty pairs, each of about thirty square centimetres of surface. The electric current being then much more intense, the sulphuret of gold was very promptly decomposed, as well as the water charged with the cyanuret, which held it in solution, and the gold fell to the bottom of the vessel, without adhering to the piece. It might, however, be possible to turn so powerful a current to useful account, if it were required to gild objects of very large dimensions, as, for example, a pendulum of bronze: not having been able to repeat my experiments on a larger scale, I can say nothing positive in this respect. I may observe only that the intensity of the current should be in proportion to the size of the piece to be gilded, and that a very large voltaïc couple may be used for gilding objects of very different dimensions, provided that the force of the current be regulated according to the size of the object, which may be done by the more or less complete immersion of the voltaïc couple in the acidulated water.

By the employment of the process which I have just described, I am enabled to gild most of the metals used in the arts: among which I may principally mention silver, copper, brass, bronze, platinum, iron or steel, lead and tin.

It is necessary carefully to scour the surfaces of the pieces, for the slightest layer of oxide or grease completely prevents the deposition of the gold. This property may be turned to account for gilding certain parts of an object, leaving the others in their original condition: for this purpose the latter are covered with a fatty or resinous layer, on which, however, the free alkali of the cyanuret of potassium cannot act powerfully; a varnish composed of resin, wax, and alcohol, answers this purpose very well.

Watch springs may be very well gilded by the process just described; but it is only after having removed, by some means or other, the slight pellicle of oxide which forms the blue color they possess, that the gilding can be produced; without this precaution, they would remain in the liquid without being in any degree covered with gold.

None of the metals experimented with, with the exception, however, of zinc, exert a reductive action on the solution, when they are not submitted to the influence of the electric current.

I have tried to silver metals, by substituting the sulphuret of silver for the sulphuret of gold, in the above operations, but I have not obtained good results. The sulphuret of silver is very sparingly soluble in cyanuret of potassium: in substituting for it a solution of chloride of silver in liquid ammonia, there was no effect, as might be foreseen *à priori*, from the great affinity which exists between chlorine and silver—an affinity which powerfully resisted the means of decomposition employed. Since then, I have become acquainted with the process of silvering invented by M. de Ruolz, through the above passage from *L'Institut*. (See The Chemist, No. XXVII. page 84.) I have repeated the experiments, employing a solution of cyanuret of silver, in cyanuret of potassium; the results have been much more satisfactory, although they are far from being so good as those of gilding. The inconvenience which is here presented, is that the solution is easily decomposed, without the influence of electric currents, by less electro-negative metals than silver. Thus, a piece of copper which is steeped in it, immediately turns white; silvering by the humid way, also generally possesses little solidity; it requires the employment of dilute solutions and feeble electric currents. Notwithstanding these precautions, there are some metals or alloys on which this operation is impracticable, such as those which have a great affinity for oxygen; thus, pale colored brass, that is, in which there is a large proportion of zinc, acquires a coat of silver which does not resist the slightest friction. Pure copper best supports this operation; for I have been able to expose to a dull red heat copper coins silvered by the humid way and the voltaïc current, without the layer of the metal with which they were covered disappearing or detaching itself.

In order to complete the series of experiments on this new process of gilding by the humid way, with the hope that it may one day very advantageously become a substitute for gilding by fire and mercury, several experiments remain to be made, and several data to be established before a certain method can be laid down.

1st. To establish the relation which should exist between the size of the voltaïc couples, that is to say, the intensity of the current, and the surface of the objects to be gilded.

2nd. To indicate the quantities of cyanuret of potassium, sulphuret of gold, and water, best adapted for arriving at perfect results.

3rd. To ascertain the extent to which a given quantity of a solution of gold, containing a known quantity of sulphuret of gold, is exhausted of that metal under the influence of an object and of an electric current of which the dimensions and the intensity respectively are fixed, in order to know at what degree of strength a solution of gold may be employed.

4th. To determine the increase of weight suffered by a piece to be gilded, of known dimensions, when it is submitted to a voltaïc current in a solution of sulphuret of gold, the strength of the solution and the intensity of the current being also known; in order to be able accurately to calculate the thickness of the gilding, by the time that the piece has remained in the solution. This series of experiments is also connected with those indicated in No. 3.

I hope shortly to have the honor of submitting the continuation of this work to the Academy.

NEW METHOD OF PREPARING MOULDS FOR ELECTRO-CASTING.

To the Editors of The Chemist.

March 10, 1842.

Gentlemen,—

I beg leave to inform the readers of your esteemed Journal, that I at present employ a novel method of forming the matrices or

moulds used in electro-metallurgy. I first calcine sulphate of copper until the greater part of its water of crystallisation has been driven off; when, with the resulting white powder, I form an exact copy of the object of which I am desirous to have a facsimile in metal. The white powder of the sulphate is worked like plaster, and like it "sets" into a firm mass after a short time. When the model is thus completed, imbed it in the centre of a mass of Paris plaster, which should be mixed up with just the requisite quantity of water and no more. A small hole is bored in the plaster, and a copper wire inserted therein, and communicating with the sulphate of copper model. The other end of the wire is attached to an iron or zinc plate forming a voltaïc arrangement, which should be immersed in a trough separated into two compartments by a porous partition, one half being filled with the usual saturated solution of copper, and the other with dilute acid or solution of muriate of ammonia, into which latter the zinc or iron is, of course, placed, and in the other the prepared mould and its copper wire. The sulphate of copper gradually dissolves, leaving the copper wire to receive the depositing metal, which soon accumulates and fills up the hollow cavity of the mould.

Sometimes, if the object be flat, as a medal, I put into the torrefied sulphate of copper, a circular disc of copper, to which is fixed the wire attached to the zinc previous to covering the model with plaster of Paris. Furthermore, I often find it advantageous to wash out the model from its coat of plaster by immersing them together in a vessel of warm water; the sulphate of copper is dissolved, and the matrix left complete, with its wire attached, ready to be put into action.

I remain, Gentlemen,
Respectfully yours,
Z. J. Rockline.

AN IMPROVED CONSTRUCTION OF MARINERS' SIGNAL ROCKETS.

BY LAWRENCE BRUNTON.

The heavier the rocket-case is, the larger and more weighty is the stick or balance required, which must, of course, lessen the velocity of its ascent; and if the thickness of the case be diminished, the consequence will probably be that of the rocket bursting before any ascent takes place, from a want of sufficient strength to resist the pressure of the suddenly disengaged gases evolved by the inflammation of the combustible materials in the interior of the case. I have found, that by wrapping the outside of a thin case with moderately stout packthread, the effect gained was such that an "ounce" rocket ascended to as great an altitude as a "half-pounder" of the usual make, the increased velocity evidently resulting from the employment of a lighter rocket-case, and consequently a lighter counterpoise. The packthread was wound close, and every where round the case, and for what is called an "ounce" rocket should be of about $\frac{1}{24}$th of an inch in thickness. The common kind used by shopkeepers answers extremely well, and can be had at a low price. By comparison, I have found the spiral coil of twine to be infinitely cheaper than the cost of the immense mass of paper which would be required on the cases as ordinarily manufactured.

IMPROVED METHOD OF PRODUCING STEREOTYPE PLATES THROUGH THE AGENCY OF VOLTAIC ELECTRICITY.

BY T. W. NAYLOR.

First procure an electrotype cast or mould off the type in the usual way, and then smooth the back down by rubbing it on a sandstone till it appears sufficiently even. With this cast you may, by placing a sheet of type metal on its face, and subjecting it to strong pressure, procure an excellent stereotype plate vastly superior to any that are produced by the founder. It is requisite, for the production of a good cast, to use the "spaces," "quadrats," &c. usually employed in stereotype-printing, as the less indent there is in the page the better, as might naturally be supposed. The same principle might be applied to the production of copper-plate fac-similes, especially where the work is not required to be printed very fine and clear, as there is always a sort of smutty tinge communicated to paper printed in the copper-plate manner from alloys or mixtures of lead, tin, and such-like metals, as any one may satisfy himself by inspecting a page of printed music, which is printed from type-metal or pewter plates.

EMPLOYMENT OF AMMONIACAL NITRATE OF MERCURY IN PHOTOGRAPHIC EXPERIMENTS.*

BY M. CHARBONNIER.

At the sitting of the Academy of Sciences of Paris, held on the 15th of November, 1841, M. Charbonnier announced that he had found that the ammoniacal nitrate of mercury may be used instead of mercury in pho-

* *L'Institut*, No. 412, Nov. 18, 1841.

tographic experiments, and that it possesses the advantages of acting more promptly and of being more easy of preservation, &c. He thought also that this agent might modify the ordinary tints.

IMPROVED METHOD OF MULTIPLYING ENGRAVED PLATES BY VOLTAIC ELECTRICITY.

To the Editors of The Chemist.

March 5, 1842.

Gentlemen,—

I send you a description of an excellent method of copying plates by the electrotype apparatus. Lay the copper-plate, with its face uppermost, upon a flat table or board, previously slightly oiled, and spread entirely over it a layer of Paris plaster, about a quarter of an inch in thickness, taking the usual precautions for avoiding air-bubbles, &c. Let the plaster project over the edge of the plate (about an inch) all round, forming a kind of margin: when this is done, and the plaster firmly indurated, with a thin knife raise it from the board, and afterwards with the point lift the copper-plate from it in the same manner. All that has now to be done is to cement it by the flat margin to the thin copper, pewter, or metallised plate of plaster or wax, acting as the cathode of a voltaïe arrangement in a strong solution of sulphate of copper.

The mould must be allowed to remain so long a time in the solution as may be judged to be sufficient for the requisite deposition of copper to have accumulated within the *interior* of the mould, when the plaster may be broken off, and the east taken out. If you can find room for this piece of information you will oblige,

Gentlemen,
Your obedient Servant,
M. J. Brazendale.

SUBSTITUTION OF IRON FOR ZINC AS THE ELECTRO-POSITIVE METAL FOR EXPERIMENTS WITH VOLTAIC ELECTRICITY.

by z. j. rockline.

In all my electrotype experiments, I have employed, and with the greatest success, ordinary sheet iron instead of zinc for the positive metal,—than which it is much cheaper. The difference must, I think, be palpable in large electro-castings, where extensive surfaces of zinc would be necessarily requisite. Iron, in the form of cylindrical rods, which are extensively manufactured, would, if cut into suitable lengths, form most excellent and cheap substitutes for the zinc bars hitherto used in circular constant batteries. To electrotypists this metal, in whatever form it may be used, must prove a cheap substitute for zinc.

THE CALOTYPE PROCESS.

We have been favored by Mr. Collen, of Somerset Street, Portman Square, with a sight of his extensive collection of photographic portraits, taken by the calotype process. Nothing can be more admirable than the extreme accuracy of the likenesses: they are free from the defect which constitutes the common objection to this kind of portraiture, namely, the ghastly corpse-like hue given to the complexion.

The operation is simple, and of very short duration: a sitting of a few seconds is all that is required for taking a portrait of which any number of copies may be produced.

Among the likenesses exhibited to us were those of the French Ambassador, Mr. Young the performer, Landseer the painter, Professor Henslow, land many distinguished persons.

Mr. Collen is the only person licensed by the patentee to take portraits by this process.

III. PHARMACY, &c.

ABUSES OF THE DRUG TRADE.

letter ii.

March, 1842.

To the Editors of The Chemist.

Gentlemen,—

As it appears to me that the evils arising from the plan pursued at the public dispensaries, as well as from the system on which they are established, have not been sufficiently dwelt upon by any of your numerous correspondents, I think that a few observations may be beneficially advanced; and as I duly appreciate the value of such institutions, though fully cognizant of the injustice they inflict upon those chemists and druggists who perehance reside in their locality, I beg to be allowed to occupy a small space of your columns with a few brief remarks, which, it is pre-

sumed, will be acceptable, as being an *exposé* of one of those abuses which form the subject of these letters.

To be justly entitled to the denomination of "charitable institutions," they should be founded upon equitable principles—upon principles which would enable them to afford aid to the distressed without visiting injury upon other classes of society, by acting on plans detrimental to the interests of neighbouring tradesmen, a class of persons for whom there is less protection than for any other members of the community.

In No. 16 of your journal, Vol. II. page 120, in a note, you observed, alluding to dispensaries, "In these places much reform is required. What will our readers think of medicines being given in a piece of newspaper, and the patient told verbally, not in writing, to divide the powder into six or eight parts?" It would be well, not only that a reform in these institutions should take place, but that as *dispensaries* they should exist no longer otherwise than for the sole purpose of distributing PRESCRIPTIONS to the poor applicants who stand in need of gratuitous advice, which prescriptions should be intrusted to the more careful and prompt dispensing of the neighbouring chemists at a FIXED PRICE, commensurate with the objects of the institution, to the directors or governors of which the dispenser should be responsible, and from whom he should likewise receive compensation. By this plan no injustice would be entailed upon the chemist and druggist, and the numerous evils of the present system would be abolished, and the designs of their founders might be both effectually and beneficially carried out; and then alone can they hope to obtain the support and approbation of society at large.

It must be admitted, that the humiliation, neglect, and frequent insult, which the poor experience at the hands of both officials and pupils at the dispensaries, together with the unnecessary trouble heaped upon them, causes them to evince the greatest reluctance to avail themselves of the assistance there offered. It should be an invariable rule in the formation of all public charities that they should press as lightly as possible upon the interests of other classes of individuals. Now it is obvious the public dispensaries must make great inroads upon the legitimate business of the druggist, to his evident disadvantage, and also that of the poor, for whose especial benefit they were established. My attention has been more immediately directed to this subject by the perusal of an article descriptive of pharmacy in Norway, in which we are informed that "medicines for hospitals are purchased at the retail price, because it is considered that, as these establishments diminish the number of private patients, the interest of the druggist should not suffer. Where there are two druggists in a town, they supply the hospitals with medicines in rotation. With such securities the chemist and druggist in Norway is sure of an honorable living." One or other of these two plans, with certain modifications, or additional regulations, might, I think, be advantageously adopted in support of the chemists and druggists of this country, which would greatly tend towards ameliorating their present condition, and conduce to the general respectability of the body.

I am, Gentlemen,

Respectfully yours,

PHARMACOPOLA.

THE PHARMACEUTICAL SOCIETY, AND QUALIFICATION OF CHEMISTS AND DRUGGISTS.

GENTLEMEN,—

Observing the following assertion in allusion to the bill to regulate the Medical Profession, in the last No. of the "Pharmaceutical Journal and Transactions,"—"The chemists and druggists, having undertaken to reform themselves, will be allowed to proceed unmolested in their laudable undertaking"—may I crave a short space in your valuable periodical to address a few observations to my brethren, the chemists and druggists of Great Britain, and their assistants.

In London, a great portion of the business of many chemists and druggists consists in prescribing and selling medicines over the counter, in the manner of apothecaries of the olden time,—with what capacity for such office it is not my province to inquire. On the appearance of Mr. Hawes' Medical Reform Bill last year, a sudden alarm was felt by the greater part of the trade in London (I mention London in particular, because I do not think any such alarm was felt in the country, nor that there exists the same cause for it), lest this lucrative department of their business should be removed from them. The trade* met in self-defence, and subscribed money to oppose any medical reform bill which should cut

* The trade, *i. e.* the wholesale trade, with a select few of the leading members of the retail body.—See list of present Council. Of course my remarks cannot be supposed to allude to some of the honorable names therein mentioned.

off this valuable branch of their business; and becoming aware, probably for the first time, of their general ignorance as a body, they creditably adopted the resolution of forming themselves into a Society, — I cannot say for mutual instruction, their whole amount of scientific, or indeed of any other kind of knowledge not being found worth the dividing amongst so many,—but "for the purpose of uniting the chemists and druggists into one ostensible, recognised, and independent body, for protecting their general interests, and for the advancement of Pharmacy, by furnishing such a uniform system of education, as shall secure to the profession and the public the safest and most efficient administration of medicine."

Such an object would be most praiseworthy if properly and impartially carried out; how far such is the case, and how far the love of science actuates this body, (or in other words, its representatives, "the Council,") I leave you to judge, by the exclusive character and bearing of its laws and regulations, giving the right of membership exclusively to those who have been or are at present in business on their own account,— a notable standard of qualification truly, and which has, doubtless, been embraced by some notable members of the fraternity.

The Editor of the "Transactions" seems to have a great deal of trouble to convince his stupid correspondents what a chemist and druggist really is, for in the last No. he informs one of them that a "Chemist and Druggist who has retired from business is a retired Chemist and Druggist;" perhaps he might be able, with due mental labour and reflection, to solve the following problem:— A tailor, cooper, medical herbalist, or any other tradesman retires from business, and subsequently sets up a chemist's shop, and becomes M.P.S.G.B., &c.; should he again wish to retire from business, under what title might he be allowed so to do? As two or three of their members might probably find themselves in such a predicament, in common justice the Council should investigate and finally settle this important question as speedily as possible.

The difference of title between member and associate, and the possession of an *unearned diploma*, can be of little or no consequence to any of us; but the classification of assistants, of however long standing, and of whatever attainments, with apprentices of a week's growth, to their joint exclusion from the right of voting for officers or taking any part in the management of the affairs of the Society, accompanied as this may be with the demand of a good round sum as an examination fee, after a certain time,* are matters of graver importance. Here, however, I must observe, that they appear to have been rather premature in the announcement of their intentions, for though they have given little time for those who are to be examined to study Chemistry, Pharmacy, &c., they have reserved as little for their own qualification as examiners,—it must of course take some length of time to manufacture examiners out of the raw material of their present Council and members; —*query*, do they mean to hire?

It is not stated that any intention now exists to solicit from the legislature the privilege of granting or refusing licenses to commence business, to the at present *unattached* members of the trade; should such demand at any time be made, I trust the Government and the public will be duly informed of what advantage this Society, under its present management, is likely to be to any one. That some such institution should be established is desirable; and that such would be the case if the proper and necessary reform was forced upon the trade by the legislature, is equally certain. The disallowing the power of chemists and druggists to prescribe medicines, would not be very materially felt in country towns, any more than are the boasted advantages of the P.S.G.B.; while in London any bill which prevented the counter practice of the chemist, and which at the same time restrained the present system of surgeons and apothecaries retailing drugs, &c., as is now so universally the case, would more than repay the chemist for his partial loss of business, and if enforcing a proper education, would render him more respectable and respected than any diploma, or other imaginary honour conferred upon him by the present self-instituted, pretended self-reforming Society.

I am, Gentlemen,
Respectfully yours,
A Chemist and Druggist.

REVIEWS:—

What to Teach, and How to Teach It, so that the Child may become a Wise and Good Man. By Henry Mayhew. Part I. The Cultivation of the Intellect. pp. 44. Price 1*s.*†

This work has but little connection with the

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* July, 1842.

† At the request of numerous correspondents, some of whom are resident abroad, we shall in future give the price and number of pages of every book that we review. We request, therefore, that the publishers of works sent to us will inform us of the price, when not marked on the work itself.

subjects which form the matter of THE CHEMIST; still, as every thing connected with education is interesting as well important to our readers, we feel that we should not be warranted in neglecting to notice it.

The cultivation of the intellect is a subject of high interest: nothing can have so great demands on our attention as the most rational system of bringing to maturity the wonderful powers of the human mind.

The system pursued by most parents is remarkable for nothing but absurdity: nay, we might say, criminality. The foolishly fond mother forgets, that in her unwarrantable indulgence of her children she is fostering all that is bad, thus perverting the susceptibility of the youthful mind, which was intended for the reception of wholesome impressions.

We may take another opportunity of recurring to the subject of this work; in the mean time, we recommend every parent whose eye our remarks may meet to peruse it, and carefully to consider the important subject of which the author, who is an observant and thinking man, treats. Its moderate price brings this book within the reach of the humblest persons.

ON THE USE OF THE ERGOT OF RYE.*

BY GEORGE FIFE, M.D., NEWCASTLE-UPON-TYNE.

DR. FIFE states, as the result of his experience, that the ergot of rye is a useful medicine in *polypus uteri*, attended with profuse hemorrhage,—*menorrhagia*, where there is no inordinate action of the heart or arteries, or morbid sensibility of the uterine system,—in *leucorrhœa*, when independent of inflammatory action,—in *chlorosis* with amenorrhœa,—and in *dysmenorrhœa*. The first time Dr. Fife saw the ergot exhibited, was by the late Dr. Parr, of Newcastle-upon-Tyne, in 1828. "It was in a case of *polypus of the uterus*,† accompanied with frequent and frightful attacks of hemorrhage. He gave the ergot, after all ordinary means had proved unavailing. The effect produced was not only the moderation of the hemorrhage, but also the expulsion of large and numerous masses of the tumour, in many of which a distinct fibrous structure was perceptible. After its continued employment, the woman, who had arrived at the climacteric period, enjoyed comparative health and comfort, being freed from the repeated and alarming hemorrhages, and experiencing no inconvenience from the small portion of the tumour which, when last examined, still remained. In certain cases of *menorrhagia*, Dr. F. has found this medicine of the greatest value. "In this disease," he remarks, "it is necessary to ponder well on the individual circumstances of each case, and to use the utmost caution in ascertaining the cause on which the disease depends; as where such precaution has been neglected, it has been my lot to see both the sufferings and danger aggravated by the ergot. In this respect it does not differ from other active medicines. The slightest reflection will suffice to call to mind the very different states of the system in which this disease occurs. It may, for example, happen in the most phlogistic and plethoric, or it may arise in a person of a diametrically opposite constitution. If given in the first state, it is decidedly prejudicial, unless preceded by such means as are calculated to remove alike the plethora and morbid sensibility; and even when this has been done, its operation requires to be carefully observed. In the last, it acts most beneficially, as it at once raises the nervous energy of the uterus, and, through this medium, probably imparts increased tone to the relaxed and debilitated vessels from which the exhalation takes place." In "several distressing cases of *leucorrhœa*, where the strongest astringent injections had been employed without any effect, except exciting inflammatory action which did not previously exist, I have found the ergot, aided by injections of simple warm water, or the decoction of poppy capsules, perfectly successful." In this disease, it is particularly necessary to attend to the state of the system and the nature of the uterine disorder, before deciding upon exhibiting the ergot. "In *chlorosis* and *amenorrhœa*, I have," says Dr. F., "frequently experienced the good effects of the ergot, after aloes, iron, valerian, cantharides, &c. had all been employed without the slightest advantage. In those cases where extreme nervous sensibility exists, it may be most advantageously combined with the valerian; and where the alvine system is torpid, with aloes." In *dysmenorrhœa*, Dr. Fife found it very useful; and in one very severe case, wherein he combined it with valerian, "it appeared almost magical in its operation." The doses recommended are, gr. x to ℈i of the powder, and ℥ss to ℥i of the concentrated tincture; but how frequently administered is not stated. In one case of menorrhagia, ℥ss was ordered to be taken, in ℈i doses,

* *London and Edinburgh Journal of Medical Science.*

† *Vide* "Monthly Journal," p. 416, 1841, (Mr. Moyle's paper,) and p. 570, (Dr. Somerville's paper).

every three or four hours. As to the *modus operandi*, practical men will care little if the facts are found to be as Dr. F. and others report them to be. However, it is not difficult to see how "a congested state of the uterine vessels will in one person lead to menorrhagia—the same cause operating on a different constitution will be attended with leucorrhœa." Again, when the nervous system of the uterus is at fault, we in one person have chorea, in another simple hysteria, in others cardiac and pulmonary symptoms.—Abridged from *Med. Gazette*, June 18, 1841.

ON THE PREPARATION OF MEDICINAL WATERS WITH MAGNESIA.

To the Editors of The Chemist.

Gentlemen,—

I regret having given publicity, in the January number of your Journal, to an erroneous hypothesis relative to the solubility of iodide of iron; my deductions having been drawn from operations performed on an impaired article, which I, having none in stock, unfortunately obtained from a neighbouring chemist; though, I freely confess, had I been more perfectly acquainted with its properties, I had not been so deceived. I regret this the more, as I was led to comment in somewhat severe terms upon the, as it appeared to me, erroneous statements of your correspondent, J. S. Not that he is right because I have erred, but that I feel that something in the shape of the *amende honorable* is due to him whom I have charged with propagating fallacies, at the same time that I was no less culpable than him whom I accused.

Your correspondent somewhat more correctly infers that the precipitate I obtained with distilled water was sesquioxide of iron, —a fact which the evidence he has adduced from Phillips's Translation of the Pharmacopœia goes to prove; but how does J. S. reconcile this with his statement that the results in both cases are identical? If the magnesia had the effect he ascribed to it, the precipitate must have been essentially different from that caused by the absorption of oxygen from the atmosphere. On reading your correspondent's observations I immediately pronounced his conclusions fallacious, for, according to his own theory, the result of the decomposition would be a *carbonate of iron*,—the carbonic acid of the magnesia combining with the iron of the iodide,—which, nevertheless, he persists in calling a sesquioxide, because, forsooth, the College, in the last edition of the Pharmacopœia, having proved the article hitherto called carbonate of iron to be a sesquioxide, have denominated it accordingly; as if, therefore, he conceived such a compound as carbonate of iron could not exist under any circumstances, or that a carbonate must necessarily be a sesquioxide.

If J. S., who has made several extracts from Phillips's Translation, before-mentioned, will turn to the formula for the preparation of sesquioxide of iron, he will find that carbonate of iron is the first product, and that it is the latter part of the process which converts it into sesquioxide, namely, the washing and subsequent exposure to the air whilst drying, whereby it acquires oxygen and loses carbonic acid. This is not the case, however, with the precipitate he obtained; on the contrary, it was occasioned by the presence of carbonic acid, agreeably with his own statement.

Commenting on my remarks upon distilled waters, J. S. says, that during his experience he "never knew or heard of an instance in which any of them became deteriorated," though he does not inform us whether re-distillation was the plan adopted in the establishment to which he alludes.—In reply to this, authority being preferred, I also quote Mr. Phillips, who, in his remarks upon distilled waters, states, "when they have been long kept, they undergo a kind of decomposition, and become mucilaginous and sour." Dr. A. T. Thomson also observes—"When long kept, many of the distilled waters undergo a species of decomposition: they become slightly sour, and a ropy, viscid matter forms in them, owing to the essential oil they contain undergoing decomposition, and changing into mucilage." Moreover, he suggests, "it is essential for the preservation of the waters that they be kept in closely-corked vessels," and adds, "the addition of spirit is intended to prevent this change from taking place, but it is not adequate to the effect." Dr. Paris in his "Pharmacologia," says—"A question has lately arisen, whether the addition of spirit, in so small quantity, may not actually provoke the fermentation it is intended to prevent." But both writers agree that "a much more preferable mode is to re-distil them, after which they may be kept good for several years." It seems scarcely necessary to refer to the change that takes place in the Aq. Fl. Sambuci, and Aq. Fl. Aurantii, both of which, it will at least be allowed, are invariably distilled, also in the Aq. Destillata itself,—circumstances with which every druggist is familiar. The following, copied from page 128 of Vol. I. of The Chemist, formed a subject for the elucidation of which the Society of Medicine of Bordeaux offered a prize of 300 francs,

to be decreed in 1841. "To make known the alterations which distilled waters in general may undergo, particularly those of orange-flower, mint, melissa, and the cherry-laurel; to point out the chemical causes of these alterations. Is there a general method of preserving distilled waters? Is there one for some among them?"

J. S. asserts that distilled water "is rarely if ever used" in the preparation of medicated waters by trituration with magnesia; all I can say is, that at least it is ordered by the College in the edition of the Pharmacopœia to which he refers me; and that there are reasons for continuing the use of waters carefully prepared in conformity with those directions, which I need not encroach upon your valuable space in detailing, as I doubt not they will be readily recognised and appreciated by the majority of *really* practical men.

With respect to my statement that the strength might be increased *ad libitum*, I presumed the term would be construed to mean so far only, as regarded their capability of being made of uniform strength with those distilled.

In conclusion I beg respectfully to solicit the insertion of this in your next number, as an act of justice to both parties, and

I remain, Gentlemen,
Your obedient Servant,

St. Marylebone, March, 1842. G. C.

SULPHATE OF QUININE AS A CURE FOR ANGINA PECTORIS.*

A MAN, 25 years of age, who had been complaining for some days of a sensation of weight in his chest, was suddenly seized with angina pectoris, after having experienced much annoyance. His body was bent forward, his countenance was livid, his respiration was hurried, with a dreadful feeling of constriction at his heart. Nothing relieved him, until about a month after, having obtained the object of his desire, he found his complaints disappear.

New sources of annoyance having occurred, his pain again returned. His fits of suffering came on irregularly, but they were always most severe at bed-time. Several physicians were consulted, but nothing gave him any relief, until Dr. Van Brabander, under whose care he was, struck with the greater urgency of the symptoms at bed-time, determined to give him quinine. This was accordingly done, and after it had been administered for several days, the patient gradually recovered his usual health.

* *Journal de Méd. et de Chirurgie Pratiques*, March, 1841; and *London and Edinburgh Monthly Journal of Medical Science.*

EMPLOYMENT OF CHLORIDE OF TIN IN CANCEROUS AFFECTIONS.*

BY DR. NAUCHE.

THE distinguished practitioner to whom we owe the employment of chloride of tin in cancerous disorders, administers it in the following manner:—

SOLUTION.

	Grammes.
R Chloride of tin	0·025
Distilled water	500·000

Dissolve.

The patient takes every day in a cup of mucilaginous drink a spoonful of this solution, which may also be used for rubbing ulcers.

POMMADE.

R Chloride of tin . . .	0·05 to 0·10
Lard	30·00

M. S. A. and divide into eight doses, one to be used every day in friction on the legs and thighs.

Dr. Nauche has found very good effects from this chemical product in glandular swellings, in schirrous affections, and even, as it would also appear, in ulcerated cancer.

POISONING BY ARSENIC. SUSPENSION OF THE SYMPTOMS FOR EIGHTEEN DAYS—RELAPSE, AND CURE.†

BY DR. SALLAMEA.

MADAME X., 27 years of age, took about a gramme of arsenic; from half an hour to an hour afterwards the symptoms of poisoning commenced. Under the treatment employed she recovered, and Dr. S. gave up attending. Eighteen days after his last visit he again called, when he found his patient vomiting a green fluid mixed with blood, with a sensation of heat at the epigastrium, headache, and small pulse, constipation, and a diminution of the secretion of urine. The hydrated oxide of iron was administered, and the patient gradually got better. Arsenic was detected in the urine passed, by M. Magonty, Professor of Chemistry. Sixteen days after the relapse, the urine was found no longer to contain any arsenic. The above case, if it is authentic, and no fraud existed on the part of the patient, (a circumstance less rare in women than is supposed,) is worthy of interest. That a relapse should occur eighteen days after all symptoms of poisoning had disappeared, without any more arsenic having been taken, is a fact so rare that it is worthy of notice.

* *Journal des Connaissances Médicales.*

† *Gazette Méd. de Paris*, 25th Sept. 1841; and *London and Edinburgh Monthly Journal of Medical Science.*

SPASMS OF THE STOMACH CURED BY THE APPLICATION OF HORSE-RADISH TO THE LAST DORSAL VERTEBRÆ.*

BY DR. ARNOLDI.

A WOMAN, aged 40, of a full habit, was frequently troubled with spasms of the stomach; on one occasion these became intolerable. The pain was seated in the region of the stomach, extending from both sides to the back; it was increased by slight pressure on the lowest dorsal vertebræ: on this part was placed a cataplasm of scraped horse-radish and vinegar. The rubefacient action was almost instantaneous. The pain diminished, and completely disappeared in ten minutes.

COLWELL'S TRUSSES.

THERE are few instruments for the alleviation of human suffering more important than trusses. The frequency as well as the distressing and dangerous character of those diseases in which they are employed, render it necessary that the greatest perfection in their principle and construction should be adopted, in order the more perfectly to substitute the natural conformation of the affected parts, or to supply casual or congenital deformities. In no instruments, therefore, is the aid of art more necessary, nor have greater varieties of improvements, nor a more numerous display of inventive ingenuity, been exhibited, than in the different kinds of trusses, from the mere suspensory bandage to the most complicated instrument which human skill can devise. Every instrument-maker has his peculiar method of constructing trusses. It is impossible to pass up the public streets without observing in the shops some of different formation, whether intended for hernia or for prolapsus ani or uteri. All that is to be expected of, or effected by, these instruments is, support and pressure, conformably with the nearest approximation to nature, and at the same time unproductive of the slightest inconvenience or incumbrance.

We have in the course of a long experience examined many and very ingenious contrivances of this kind, which have had for their objects the perfect remedy of the particular malady, and at the same time possessing the least possible weight or inconvenience.

We have lately been afforded an opportunity of inspecting a vast variety of these instruments, invented and made by Mr. Henry Colwell, of Leadenhall Street, and are enabled to state that they are constructed in a manner so perfectly adapted to their numerous purposes, yet with such simplicity and lightness, and at so moderate a price, as to subserve their intended objects and to come within the reach of all classes. Those trusses designed for prolapsus ani are admirable in their construction, and for the efficiency with which they perform their office; but those which are intended for prolapsus uteri are the most perfect instruments we have ever seen; they afford a perfect support to the descending uterus, allow the various motions and positions of the body to take place with all the ease and freedom of nature, and are formed of such materials as protect the sufferer from inconvenience and the loathsomeness of offensive odor, (the too common defects of other trusses,) whilst they afford every facility to the performance of the necessary functions of the parts: in fact, they are the best auxiliaries to nature in these cases which have ever come under our notice, and therefore we feel it but justice to recommend them to the patronage of the medical public, and to all who are interested in the subject.

TO CORRESPONDENTS.

Mr. N. B. MILLARD, Apothecary, Elburg, Guelderland, Holland.—Our correspondent will perceive that we have acceded to his request, in which he was joined by others.

Will "A WORCESTERSHIRE FARMER," enable us to address him privately.

J. R. M. can obtain the No. of THE CHEMIST which he requires from our publisher.

A READER.—Press out the principal portion of the elaïne; then add cold alcohol to the remaining steärine, taking care to thoroughly incorporate them. Subject this to strong pressure in folds of linen. If very pure steärine be required, repeat the operation a second time. Drive off any remaining alcohol by means of heat.

"SNIPE" is referred to THE CHEMIST, Vol. I. page 126.

ANSWERED BY POST.—R. H., and J. S.

The Review of Mr. Brown's work on Vaccination is unavoidably postponed until next month. We may observe, however, that it merits an attentive perusal.

NOTA BENE.—*All Communications and Books for Review must be addressed, "To the Editors of The Chemist, Care of Mr. Hastings, Publisher, 13, Carey Street, Lincoln's-inn Fields, London." Communications must be prepaid, and sent before the 15th of each Month; Books for Review before the 10th.*

* *Caspar's Wochenschief für die Gesammte Heilkunde.*

THE CHEMIST.

I. CHEMISTRY.

ON THE MOLECULAR CHANGES WHICH SUGAR UNDERGOES UNDER THE INFLUENCE OF WATER AND HEAT.*

BY E. SOUBEIRAN.

The modifications which sugar undergoes by the simultaneous action of water and heat, have not yet been the subject of any connected work on the part of chemists, notwithstanding the high interest which attaches to it. I may, therefore, easily recapitulate in a few lines all that has been published on this subject.

According to M. Thenard, by exposing for a long time a solution of cane sugar to a temperature of from 90° to 100° C., it becomes colored, and the greater portion of the sugar which it contains loses its property of crystallising. Sugar thus rendered uncrystallisable forms, to all appearance, a new kind. Berzelius says, in his turn, that by heating beyond 100° C. a concentrated solution of sugar, the latter is altered and is converted into uncrystallisable sugar. Liebig expresses himself in nearly the same terms. Pelouze has observed that uncrystallisable sugar and grape sugar are formed at the same time. With Bouchardat, the conversion of cane sugar gave an uncrystallisable syrup, which was afterwards converted into another sugar. Finally, I should add that Biot has seen that cane sugar boiled in such a manner as to assume the solid form on cooling, lost a portion of its power of rotation, and became analogous to sugar of starch.

From these contradictory assertions, none of which, it must be said, have been supported by experiments followed up with sufficient perseverance,—what conclusion is it possible to draw, if we are still unable to affirm nothing concerning the alterations which cane sugar undergoes under the influence of heat and water? Analyses having proved that the sugar cane and beet-root contain the greater portion, if not the whole, of their sugar in the state of crystallisable sugar, it is matter of astonishment that chemists have not paid more attention to phenomena which relate to one of the most extensive and interesting branches of manufacture. I will endeavour, in this memoir, to fill up a portion of this gap, and to throw some light on this very difficult subject. I shall be led, by the very nature of the matter, to say a little concerning other sugars besides cane sugar, because the alterations which heat causes them to undergo, are intimately connected, as will hereafter be seen, with those which cane sugar experiences under the same circumstances. For the sake of greater clearness, I will first explain what are the kinds of sugar known. They are connected with three types, or principal kinds :—

1. Cane Sugar.—It is perfectly defined; nothing is known which can admit of several varieties of it.

2. Grape Sugar (Glucose of Dumas).—It is distinguished by its crystallisation in grains, by a less sweet taste than cane sugar, greater solubility in alcohol, the facility with which it separates in the solid form from its solutions, by its rapid alteration under the influence of alkalis, and by its power of rotation to the right, differing, in intensity, from that of cane sugar.

This sugar exists perfectly formed in honey; it is that which is deposited when the juices of acid fruits have been saturated, and sufficiently concentrated. It is also produced by the action of diastase on starch, or when the syrup resulting from the action of dilute acids on starch or sugar is left to itself. It is also found in diabetic urine.

It is generally admitted that there is an identity between these sugars of different origin, when each of them has been properly purified. This identity is far, however,

* *Journal de Pharmacie*, January, 1842.

from being supported by sufficient experiments. It is known, indeed, that Bouchardat has observed that the sugar in grains furnished by the grape does not resist the decomposing action of dilute acids so long as the sugar of fecula. According to Guérin-Varry, the sugar obtained by means of starch and diastase contains two atoms of water more than grape sugar. Perhaps it may be the same with the sugar of honey and with that furnished by the acids and fecula. Biot informs us that if the powers of rotation of ordinary sugar of starch and of diabetic sugar are equal, a sugar of starch obtained by diastase had a superior power, and that this power was nearly double in a sugar which M. Jaquelin prepared, by acting with $\frac{3}{1000}$ of oxalic acid on starch under strong pressure. Finally, in the experiments which Biot made with Persoz, those gentlemen had reason to suppose that different sugars are formed when dilute sulphuric acid acts on fecula for a more or less prolonged time. Ought not all these facts to be again submitted to experiment? Does not this subject, which has been regarded as well studied, on the contrary, call for new and laborious experiments?

3. Uncrystallisable Sugar (chulariose, from *χυλαριον*, syrup, retaining the termination *ose*, adopted by Dumas for grape sugar).

This kind of sugar has much analogy, in its properties, to grape sugar; it is clearly distinguished from it because it is not crystallisable, and because it turns to the left the plane of polarisation of polarised light. Our knowledge concerning the different varieties of this sugar is extremely vague; perhaps the modifications which may have been observed are due to mixtures. The following is the state of science with regard to them:—

1. Uncrystallisable sugar of honey. It has never been obtained except in combination with sugar in grains.

2. Uncrystallisable sugar, resulting from the reaction of dilute acids on cane sugar. Bouchardat admits two varieties of it, which differ only in their more or less sweet taste. My experiments do not confirm these data.

3. Uncrystallisable sugar of the juices of fruits. It is probably the same as the preceding; Liebig regards it as a combination of sugar and acid: my experiments contradict this view.

4. Uncrystallisable sugar arising from the alteration of cane sugar. It constitutes molasses for the most part. It has never been obtained except in the state of mixture.

The state of our knowledge concerning the different varieties of sugar being thus established, I will yet pause before passing to the special object of my Memoir. I wish to draw fresh attention to uncrystallisable sugar.

It is known, that when cane sugar is treated by dilute acids, it gives a syrup whose power of rotation is exerted towards the left, and which does not crystallise: the sugar which is contained in the juice of acid fruits seems to be of the same nature. This uncrystallisable syrup forms a species as distinguished by the direction of its rotation as by its other characters. Liebig regards it as a compound of acid and sugar; with sulphuric acid it forms sulphosaccharic acid, with the vegetable acids analogous compounds from which the alkalis cannot separate the acid. I think, on the contrary, that this uncrystallisable sugar should be considered as a very distinct sugar. I found this opinion on the fact, that as its power of rotation is entirely opposite to that of other sugars, its molecular composition is certainly different; a few thousandths of acid being sufficient for its transformation, it cannot be admitted that all the sugar formed is in combination with the acid; finally, it may be obtained entirely free from acid, as the following experiment proves:—I boiled cane sugar with water and a little sulphuric acid; I saturated the acid by powdered marble, and I dissolved the syrup in alcohol; I filtered and evaporated in the sand-bath. The syrup thus obtained contained neither lime nor sulphuric acid: the direction of the rotation was to the left: it did not crystallise.

A very curious character of this sugar, and doubtless the most remarkable, is the property which it has of being converted after a long time into crystallisable grape sugar. The sugar in very concentrated syrup, being kept in a cool place, remained for some months without giving the slightest indication of crystallisation; then a few grains were formed which daily increased in quantity. These grains, as Biot observed, are grape sugar with its deviation to the right: the uncrystallisable sugar was transformed. I ascertained, by well followed up experiments, that so long as the sugar in grains did not appear, the syrup had not undergone the least alteration in its power of rotation to the left: I have not been able to observe notable differences in this syrup even when part of the sugar was solidified, the part which crystallises doubtless removing nearly as much water as the liquid syrup itself contains. Thus the syrup retains the molecular state which constitutes it uncrystallisable sugar so long as it remains liquid. It is at the moment when it solidifies that its molecular state is changed; at the same time its power of rotation is inverted.

I may also add, to distinguish this sugar from the other two kinds, that it is easily altered under the influence of water and of dilute acids, which distinguishes it from glucose, and that it is also very alterable by the alkalis, which constitutes the difference between it and cane sugar.

I find in the Memoir of M. Bouchardat that the syrup obtained by the action of an acid on cane sugar does not give any sign of crystallisation, even after a considerable lapse of time. This is contrary to my observations; and the use which that chemist made of lime to effect saturation might be the sole cause of our disagreement. Bouchardat announces that by submitting uncrystallisable sugar to the prolonged action of acids, it is converted into grape sugar. In one of his experiments, the uncrystallisable sugar which was made by the first action of the acid, having been submitted to a heat of 60°, maintained for four hours, under the influence of $\frac{1}{100}$ of sulphuric acid, and the syrupy liquid having been saturated by lime, it had lost its sweet taste; it assumed the solid form after some days repose. It is very true that the passage of uncrystallisable sugar into the state of sugar in grains is effected after an indefinite time, and I have never seen it crystallise until at least six or seven weeks after preparation; but I cannot admit with Bouchardat that the more prolonged action of an acid determines its change into grape sugar. By operating in an apparatus in which no evaporation could take place, and by continuing the operation until the syrup was strongly colored, I ascertained that the sugar, which at once took its rotation to the left, preserved both the direction and the intensity of this rotation at all periods when it was examined, which could not be if it were grape sugar. I cannot therefore admit this second conversion under the influence of the acids; it is demonstrated to me that it is the result of the molecular motion which effects the solidification of the saccharine matter.

ALTERATION OF CANE SUGAR UNDER THE INFLUENCE OF HEAT, WATER, AND AIR.

All the experiments which I am about to relate were made with syrup resulting from the solution of cane sugar in distilled water. The syrup was put into a matrass, which communicated by means of a tube with a refrigeratory apparatus, in such a manner that the aqueous vapors which escaped from the syrup were condensed, and fell again into the matrass, in which they maintained the liquor at the same degree of concentration during the whole duration of the experiment. The power of rotation of the syrup was at first proved on the primitive syrup, then it was taken, from time to time, by moving a portion of syrup, which was bleached, if necessary, by filtering it, when cold, through purified animal charcoal.

It will be understood that in these experiments, as in the rest of this Memoir, the power of rotation is reported for the mean yellow ray, and for a length of 100 millimetres. The almost exclusive use which I have made of optical characters will not be surprising. They were the only ones by means of which it was possible to follow out the changes effected in the saccharine matter.

FIRST EXPERIMENT.*

Primitive syrup . . . Rotation + 68 *r.*
——— after 18 hours id. + 54 id.
——————— 48 hours id. + 20 id.
——————— 90 hours id. — 7 *l.*
———————114 hours id. —16 id.
———————138 hours id. — 6 id.

At this time the syrup was black, acid, and held in suspension an abundant black precipitate.

SECOND EXPERIMENT.

Primitive syrup . . . Rotation + 68 *r.*
——— after 108 hours id. — 2 *l.*
——————— 192 hours id. — 8 id.

THIRD EXPERIMENT.

In this experiment, the matrass containing the syrup was steeped in a bath saturated with salt.

Primitive syrup . . . Rotation + 68 *r.*
——— after 10 hours id. + 15 id.
——————— 40 hours id. — 8 *l.*
——————— 60 hours id. — 0 id.

The syrup at + 15 *r.* was neutral; the syrup at —8 *l.* was slightly acid; that at 0 was strongly acid.

FOURTH EXPERIMENT.

It was made in a bath of chloride of calcium, which was kept at 100° C.; the vapors arising from this bath were carried by a tube into a refrigerator which condensed them and allowed them again to fall into the bath.

Primitive syrup . . . Rotation + 71 *r.*
——— after 18 hours id. — 8 *l.*
——————— 30 hours id. —15 id.
——————— 48 hours id. — 0

FIFTH EXPERIMENT.

Primitive syrup . . . Rotation + 71 *r.*
——— after 30 hours id. —10 *l.*

SIXTH EXPERIMENT.

Primitive syrup . . . Rotation + 71 *r.*
——— after 24 hours id. —10 *l.*
——————— 49 hours id. — 0

* In these experiments the letter *r.* signifies the rotation to the *right*, and the letter *l.* to the *left*.

SEVENTH EXPERIMENT.

Primitive syrup . . . Rotation + 71 *r.*
——— after 44 hours id. — 0

All these substances had evidently become acid, and at the same time they became colored.

The experiments which I have just detailed prove that the changes undergone by sugar are operated more quickly when the temperature is higher. They show that the power of rotation to the right, proper to cane sugar, is gradually weakened; that it does not exist at a certain moment, past which the rotation is made in an inverse direction, and after having increased during some time in this new direction, it diminishes in its turn, and finishes by entirely disappearing.

But the properties of transformed syrups left no doubt that the action was more complicated. The syrups were always highly colored; a brown precipitate was formed; they had a very distinct acid reaction. I investigated the nature of this acid.

For this purpose, I diluted with water one part of syrup, and I distilled it. The liquor obtained was acid. I concentrated it by new distillation, then I put it in contact with oxide of mercury; one part of the oxide was reduced, whilst another furnished a salt in beautiful white micaceous plates, easily recognised as acetate of mercury. During the reaction, formic and acetic acids were formed. There was nothing extraordinary, then, in M. Pelouze having obtained grape sugar by a similar reaction. Cane sugar, under the influence of the acids, should undergo its habitual transformation, and furnish a sugar which has a rotation to the left, which is deposited, after a long time, in crystalline grains of grape sugar; afterwards there are seen to appear, as it takes place indeed, those more carbonaceous brown matters, which are the last product of the reaction of dilute acids on cane sugar.

However, I may remark that the power of rotation of this syrup changes before any indication of acidity can be perceived; the formation of acids may, therefore, be regarded as a secondary phenomenon, due perhaps to the influence of the air, which complicates the reactions, and of which it is possible to get rid. In the following experiments, I freed the sugar from this agent of destruction.

ALTERATION OF CANE SUGAR UNDER THE SOLE INFLUENCE OF HEAT AND WATER.

To avoid the contact of air with the syrup, I at first fitted up with great care apparatus for operating in an atmosphere of non-oxygenous gas; I very soon substituted for them a thick layer of oil on the surface of the syrup; I could then with facility study the influence of water and heat on the syrup of cane sugar, without any fear of the phenomena being complicated by the oxygenising action of the air.

FIRST EXPERIMENT.

It was made in a bath of sea salt; the syrup was contained in a matrass, in such a manner that the vapors might be condensed and return to the matrass. The apparatus was kept full of carbonic acid gas.

Primitive syrup . . . Rotation + 68 *r.*
——— after 72 hours id. —10 *l.*

SECOND EXPERIMENT.

It was made in the same conditions as the preceding.

Primitive syrup . . . Rotation + 68 *r.*
——— after 60 hours id. — 2 *l.*

THIRD EXPERIMENT.

Primitive syrup . . . Rotation + 68 *r.*
——— after 72 hours id. — 2 *l.*
——————— 96 hours id. —15 id.
———————112 hours id. —10·2 id.
———————128 hours id. — 5 id.
———————144 hours id. — 2 id.
———————160 hours id. — 0
———————184 hours id. — 8·6 id.

During these transformations the syrup was more and more colored; the last contained a portion of brown insoluble deposit.

In the following experiments, the syrup covered with oil was put into the sand-bath of an alembic; the retort contained a saturated solution of common salt, in which there was an excess of salt. At the commencement of the experiment, the syrup entered into ebullition, it became sufficiently concentrated to cease boiling: it was then maintained in the same conditions until the end of the experiment.

FOURTH EXPERIMENT.

Primitive syrup . . . Rotation + 71 *r.*
——— after 18 hours id. + 21 id.
——————— 24 hours id. —11·4 *l.*
——————— 37 hours id. —8·58 id.

FIFTH EXPERIMENT.

Primitive syrup . . . Rotation + 71 *r.*
——— after 18 hours id. + 21 id.
——————— 24 hours id. —11 *l.*
——————— 36 hours id. — 8·5 id.
——————— 49 hours id. — 2 id.
——————— 60 hours id. — 1·5 id.
——————— 66 hours id. — 0

SIXTH EXPERIMENT.

Primitive syrup . . . Rotation + 71 *r.*
——— after 24 hours id. —11 *l.*
——————— 25 hours id. —22 id.
——————— 28 hours id. —15 id.

Primitive syrup . . .	Rotation		
——— after 36 hours	id.	—11	*l.*
——— 48 hours	id.	— 6·6	id.
——— 60 hours	id.	— 1	id.
——— 72 hours	id.	+ 2	*r.*

SEVENTH EXPERIMENT.

Primitive syrup . . .	Rotation	+ 71	*r.*
——— after 2 hours	id.	+ 68	id.
——— 4 hours	id.	+ 58	id.
——— 6 hours	id.	+ 38	id.
——— 8 hours	id.	+ 32	id.
——— 12 hours	id.	+ 25	id.
——— 18 hours	id.	+ 20	id.
——— 20 hours	id.	+ 0	id.
——— 25 hours	id.	—11	*l.*
——— 26 hours	id.	—22	id.
——— 27 hours	id.	—24	id.
——— 28 hours	id.	—16	id.
——— 34 hours	id.	—12·5	id.
——— 42 hours	id.	— 8	id.
——— 50 hours	id.	— 5	id.
——— 58 hours	id.	— 3	id.
——— 64 hours	id.	— 0	id.
——— 72 hours	id.	+ 3	*r.*
——— 76 hours	id.	+ 5	id.

In this experiment, I carefully noted the state of the syrup; it was little colored when it arrived at the first zero; from that period it began to color more quickly, but still preserving transparency. At —12·5 *l.* it was of an extremely deep brown, almost black; but it still gave a transparent solution with water; at —5 *l.* it was rendered turbid by water; at zero, it held in suspension a brown insoluble matter, whose quantity rapidly and greatly increased; but at the moment when the operation was interrupted, the syrup freed from the black matter was still very sweet to the taste.

It very evidently results from the preceding experiments that a solution of cane sugar, heated out of contact of the air, and submitted to the sole influence of heat, undergoes changes which succeed with regularity.

1. The power of rotation towards the right, proper to cane sugar, diminishes more and more, and is finally destroyed.

2. From this point, the direction of the rotation of this syrup proceeds towards the left, and attains a maximum which, in the experiment wherein it is shown the strongest, was found to be —24 *l.* for the mean yellow ray, and for a length of 100 millimetres.

3. The deviation to the left decreases in its turn, and the power of rotation is once more destroyed.

4. The syrup again assumes a rotation in an inverted direction. It progresses more and more towards the right.

5. These changes are so much the more prompt as the syrup is more concentrated as the temperature is higher.

If we stand by probabilities, the sugar took a power of rotation to the left in being converted into uncrystallisable sugar; hence the accessive diminution observed in the rotation to the right, and finally the passage into, and increase of, the inverse direction. But this uncrystallisable sugar was modified in its turn in a manner still unknown, and hence arose the decrease in the power of rotation towards the left and the return towards the right, which were perceived in the latter part of the operation. Experiments on the influence which the combined action of heat and water exerts on sugars resembling cane sugar may one day throw some light on the question.

Sugar of honey in grains, very white and very pure, was dissolved at a gentle heat in half its weight of distilled water. The syrup which resulted deviated from the plane of the polarisation of the polarised rays about + 47 to the right. It was kept in a bath of salt in a glass matrass; it was covered with a layer of oil which preserved it from the contact of atmospheric air. Before taking each deviation, it was made to weigh exactly the same as it did previous to the action of heat, so as to have always the same degree of concentration.

The results were:—

Primitive syrup . . .	Rotation + 47	*r*	
——— after 40 hours	id.	—46·5	id.
——— 80 hours	id.	—47	id.

and the syrup was hardly colored.

The same experiment was repeated with solid sugar arising from the crystallisation of a syrup obtained by acting with dilute sulphuric acid on cane sugar. When the maximum of left deviation had been attained, the syrup was saturated with marble, then it was mixed with a large quantity of alcohol, in order to separate all the sulphate and the calcareous carbonate. The alcohol having been removed by distillation, this syrup, after a long time, deposited crystals which were purified by repeated washing in alcohol and pressure. This sugar, in grains, gave the following results:—

Primitive syrup . . .	Rotation + 22	*r.*	
——— after 50 hours	id.	—22	*l.*

The syrup was very little colored.

It must be acknowledged, from these experiments, that solid grape sugar, whether obtained from honey, or formed by the action of sulphuric acid on sugar, is very little alterable by heat and water.

Uncrystallisable syrup arising from the reaction of dilute sulphuric acid on cane sugar, and prepared with the precautions indicated above, was submitted, out of contact of the air, in a matrass, to the action

of the heat of a salt bath; it was strongly colored during the experiment.

The results were as follows:—

			Color of the Syrup.
Primitive Syrup	Deviation,	—20 *l.*	white.
——— after 24 hours .	id.	—16 id.	red.
——————— 64 hours .	id.	—14·7 id.	red brown.
——————— 80 hours .	id.	— 8	deep red brown.
——————— 120 hours .	id.	—2·9	black and turbid.
——————— 168 hours .	id.	—2·5 *r.*	black and abundant deposit.

These experiments show us, that while grape sugar in grains powerfully resists the combined action of water and heat, uncrystallisable sugar, which arises from the action of acids on sugar, is much more alterable than cane sugar itself; that this alteration is manifested by an incessantly increasing coloration of the liquor, and by the change of its rotatory power, which, at first directed towards the left, perpetually and slowly weakens, finally being nullified, and proceeds in an inverse direction, towards the right. Now these are precisely the alterations which we have seen produced in cane sugar converted by heat. Have we not a right to conclude from them that, under the influence of water and heat, cane sugar is converted into uncrystallisable sugar similar to that which is formed under the influence of the acids.

The comparative effect of acids and alkalis on sugars will also throw some light on this question.

(*To be concluded in our next.*)

INVESTIGATIONS CONCERNING THE METALLIC ACIDS.*

BY M. E. FREMY.

In submitting the metallic acids to a general examination, I found new compounds of the metals with oxygen, and obtained new salts remarkable for their beautiful crystalline forms. The metallic acids may be divided into two classes: in the first are placed those which result from the immediate combination of the metals with oxygen, and which dissolve, without heat, in the alkalis; in the second, the acids which are formed when a metallic oxide is exposed to the simultaneous influence of an alkali and an oxygenising body.

The metallic acids which are produced by these two processes present essential differences in their properties. The first are generally stable, and capable of forming with bases crystallisable and well defined salts; the second, on the contrary, are easily decomposed, and lose a portion of their oxygen under feeble influences.

As an example of acids of the first class, I have selected the last degree of the combination of tin with oxygen, which has received the name of stannic acid; and to characterise the acids of the second series, I have studied a new compound of iron with oxygen, which I call *ferric acid.* In taking as examples metallic acids of important metals, I have wished to indicate the existence of similar compounds of metals which are less known.

I commence with the examination of ferric acid. In my Memoir, I explain, in detail, the different processes employed for preparing the ferrates. I show that the compounds of ferric acid with bases may be obtained by both the dry and humid ways. The processes by the dry way consist in calcining peroxide of potassium with sesquioxide of iron in a vessel which exerts no action on the ferrate. The most easy process of obtaining ferrate of potassa by the dry way consists in throwing on iron filings previously heated to redness, nitrate of potassa dried and reduced to powder; 5 grammes of iron and 10 grammes of nitrate of potassa should be employed; there is thus obtained a reddish mass, which contains a large quantity of ferrate of potassa. I prepared ferrate of potassa by the humid way by availing myself of the beautiful experiments which M. Berthier made on the action which chlorine exerts on the metallic oxides. It was by passing chlorine into very concentrated potassa holding in suspension hydrated peroxide of iron, that I produced ferrate of potassa by the humid way. I enter into some details concerning the action of chlorine on very concentrated potassa; I demonstrate that in this particular case it does not form chlorate of potassa and chloride of potassium, as is generally believed, but a peculiar compound to which I give the name of chlorated potassa (*potasse chlorée*), which has the property of being decomposed, by a slight elevation of temperature, into chloride of potassium, oxygen and potassa: it is this body which, by acting on hydrate of peroxide of iron, converts it into ferrate of potassa. In my Memoir, I dwell on the use which may be made of chlorated potassa for producing new compounds of metallic acids with bases.

* *Comptes Rendus de l'Académie des Sciences*, No. 12, 21 Mars, 1842.

I mention some applications, and I prove, for example, that oxide of copper is converted, under the influence of chloruretted potassa, into a compound of potassa, with a new metallic acid which I call *cupric acid.*

However, my object was not to study in an especial manner the action which chlorine exerts on the alkalis; that is a question which of right belongs to the chemists who have lately published such interesting Memoirs on this part of the science.

I afterwards pass on to the examination of the properties of the ferrates; I prove that heat, the presence of organic substances and divided bodies, may decompose the ferrates, and I compare these reactions with those which oxygenated water presents under the same circumstances. I give the composition of ferric acid which is represented by the formula $Fe\ O^3$; this acid therefore may be placed beside chromic, manganic and sulphuric acids, &c. I demonstrate, by analyses, that the ferrates obtained by the humid and dry ways have exactly the same composition, but that the latter are often mixed with nitrates which, when the ferrates are decomposed, absorb a certain proportion of oxygen, in order to be converted into nitrates.

I then detail all the experiments which I made with the view of producing either a more oxygenous acid than ferric acid, or an oxide corresponding to peroxide of manganese, and to hisulphuret of iron; I then speak of the action which hinoxide of barium exerts on sesquioxide of iron, and I prove that in this case it appears to form a compound of iron and oxygen, intermediate between sesquioxide of iron and ferric acid.

Such are the different questions which I have treated in the first part of my Memoir. The second is devoted to the examination of stannic acid.

I commence by recapitulating the different works which have been published concerning this acid, and I especially dwell on the remarkable experiments of Berzelius, and on the very just observations which Gay-Lussac has made on this subject. I also speak of a Note by Graham, inserted in Liebig's *Annalen*, explaining the modifications of stannic acid noticed hy Berzelius. The first experiments which I made on stannic acid were with the view of ascertaining the true part which it acts in compounds. In this particular, chemists are divided in opinion. Should stannic acid be considered as an acid, or as a base, or does it alternately perform the part of an acid and that of a base? Such are the questions which I have examined.

All the examinations to which stannic acid has been subjected have demonstrated that this acid cannot in any case be regarded as a base. When it is prepared, for example, from chloride of tin, by decomposing that body by an insoluble carbonate, an acid is precipitated which presents well developed acid properties, and which, even in this state, can redden litmus paper. Chloride of tin treated hy carbonate of potassa does not precipitate stannic acid, but stannate of potassa, which becomes insoluble under these circumstances. In afterwards examining the combinations of stannic acid with the acids, I proved that these compounds ought not to be considered as salts of peroxide of tin, but, indeed, as combinations of stannic acid with the acids; it is known that chemistry presents numerous examples of the mutual combinations of acids, forming double acids. I also call to mind M. Chevreul's experiments, which are conclusive: M. C. has proved that stannic acid put in contact with the coloring matter of Campeachy wood, acts as an acid, whilst the metallic oxides properly so called, and even the protoxide of tin, act as bases. The last degree of the combination of tin with oxygen must, therefore, be regarded as an acid.

After the examination of this first point of the history of stannic acid, I proceed to the study of the properties of this acid. The first experiments which I describe serve for ascertaining the cause of the modifications which stannic acid presents. As this question applied to other metallic acids, it became important to resolve it, on account of its generality, and of the labors of Berzelius to which it gave rise.

My experiments having shown that the two modifications of stannic acid constitute peculiar acids, I have given to these acids different names. I have retained for the acid which is produced by nitric acid the name of stannic acid, and I have given to that prepared from chloride of tin the name of *metastannic acid.*

In determining comparatively the quantities of water contained in the two isolated acids, I ascertained that metastannic acid was more hydrated than stannic acid. As these two acids differ only by certain proportions of water, a slight desiccation may convert metastannic into stannic acid; by applying to these acids the ingenious ideas which Graham has put forth concerning phosphoric acid, I ought to consider that the stannates ought to differ from the metastannates only by their proportion of base; analysis demonstrated this; for hy representing, in a general manner, the neutral stannates by the formula $Sn^3\ O^6\ M\ O$, the metastannates have the composition $Sn^3\ O^6\ 3M\ O$: thus according to this hypo-

thesis, which I discuss at some length in my Memoir, stannic acid should be considered as a monobasic acid and metastannic acid as a tribasic acid. The relation that exists between the composition of the stannates and that of the metastannates explains a curious fact which I observed; that is, that the stannates heated with an excess of alkali are immediately converted into metastannates. The stannates are obtained, without heat, by dissolving in the alkalis stannic acid prepared by heating to redness tin and nitric acid.

The metastannates are produced by two different processes:—

1st. By dissolving in the alkalis the metastannic acid procured with chloride of tin and an insoluble carbonate;

2nd. By calcining in a silver crucible, stannic acid with an excess of base.

The metastannates of potassa and soda readily crystallise. These compounds are by no means inferior to the best defined salts, and represent, perhaps, the most beautiful crystalline compounds of tin.

The study of stannic acid has led me to the discovery of a compound of tin and oxygen intermediate between the protoxide and stannic acid, which must not be confounded with the sesquioxide of tin lately discovered by Fuchs. This compound is obtained by treating stannic acid, without heat, by protochloride of tin. The acid immediately acquires a fine orange yellow color; pure hydrochloric acid remains in solution. That body whose properties I give in my Memoir, should be considered as a stannate of protoxide of tin, and corresponds with the molybdate of oxide of molybdenum (blue oxide of molybdenum), with tungstate of oxide of tungsten, and with chromate of oxide of chromium. The facility with which stannic acid acquires a yellow color under the influence of chloride of tin, may, in many cases, serve for characterising this acid.

Finally, in examining the decomposition which the stannates undergo under the influence of heat, and by extending those experiments to other metallic salts, I have arrived at this general conclusion: that certain compounds of metals with oxygen become acids only when hydrated; but in this case, the water is not driven off by the bases as with other acids, but enters as a constituent principle into the salt. If a metallic acid be dishydrated by heat, when it is in combination, it loses the property of combining with bases, and is precipitated in the anhydrous state.

NEW CRYSTALLISED COMBINATIONS OF AMMONIUM AND SULPHUR.*

BY M. FRITZSCHE.

A CONCENTRATED solution of sulph-hydrammon† having been left for a long time to repose in a corked glass flask, the exterior of the cork was covered with a crust of sulphur arising from the decomposition of the compound contained in the flask, which had been able slowly to escape through the closing of the flask. On removing this crust, it was perceived that it had not on the internal surface the yellow color of sulphur, but that it possessed an orange color, and that it belonged to a substance with which the yellow sulphur was covered. Notwithstanding the smallness of the quantity of this substance, it was easy for M. Fritzsche to ascertain that it was a compound of sulphammon and sulphur, and as he has no where seen the description of this red compound, he endeavored to prepare it in large quantity.

He first put flowers of sulphur in the liquor which had given rise to the formation of the new substance in greater quantity than it was able to dissolve, and, after agitating it several times, he left the liquor to itself. At the end of a few days, the undissolved sulphur was converted into a yellowish red compound, consisting of small crystals which possessed all the properties of the above mentioned body, but which, by the manner in which it was obtained, might also contain sulphur mechanically mixed. M. Fritzsche therefore resolved, in order to obtain a product of greater purity, to employ another means, and the following is that which he found most successful.

Into an ordinary sulph-hydrammon, he introduced an excess of sulphur, which gave rise to a very considerable disengagement of sulphuretted hydrogen; then he added a large quantity of ammoniacal gas, always taking care that there should be present a quantity of sulphur which was dissolved in large quantity. When the liquor contained much free ammonia, a current of sulphuretted hydrogen was passed into it until the gas was absorbed, and as the liquor thus obtained allowed no deposition of the compound which it contained, the same operation was repeated, by means of which much more sulphur was dissolved. It was during this second absorption of sulphuretted hydrogen, which was disengaged from a large apparatus and a very strong current, that the liquor treated without heat was

* *L'Institut.*

† Hydrosulphuret of ammonium? E. C.

formed in great part into a kind of crystalline magnesia. By heating inthe sand-bath to 40° or 50° C., a clear liquid was again obtained, from which, by slow cooling in a corked flask, crystals separated in great abundance.

These crystals were penta-sulphammon. They formed prisms of an orange color, from one to two centimetres long, from two to four millimetres thick, and it was easy to perceive that their form was a prism with four faces, with terminal pyramids opposed to the angles of the faces.

These crystals possessed the following properties:—

In the air they were quickly covered with a yellowish crust of sulphur, and by longer exposure were entirely decomposed; crystallised sulphur was separated; the ammoniacal gas and sulphuretted hydrogen were partly decomposed; part of the sulphammon melted, and the yellow mass of sulphur which remained consisted of a mixture of sulphur with hyposulphite of ammonia, from which the latter might be separated by means of water. The atmospheric moisture performed a very evident part in this decomposition, as it proceeded less rapidly when confined under a receiver with sulphuric acid; this acid was then covered with a yellow scum of sulphur arising from the sulphuretted hydrogen and sulphuric acid set free, and the crystals, changing form and grouping, united into masses of small crystals of sulphur. After a repose of eight days, part of the compound still preserved its red color, while, by exposure to the air, the decomposition, which was much more rapid, was complete and penetrated even to the centre.

Even in close vessels this compound very soon exhaled ammonia and sulphuretted hydrogen, and during this disengagement, which was caused by a spontaneous decomposition and evaporation of a small quantity of water mechanically combined, it colored the whole vessel with a yellow liquor. There thus resulted from the orange yellow compound another compound likewise crystallised, which was distinguished at first sight from the other by its ruby red color, and by the form of its crystals. Thrown into water, penta-sulphammon formed a lemon yellow solution less rich in sulphur; sulphur is separated, which at first was presented in the soft state, but which very soon crystallised. Alcohol at first gave an orange yellow solution without separation of sulphur; nevertheless, at the end of a certain time, in the air, there separated a little sulphur in crystals, which were larger and better formed than those obtained from the aqueous solution.

The author undertook the analysis of this body, which, on account of its easy decomposition, presented very great difficulties. This analysis led him to results which do not precisely coincide with the formula $N H^4 S^5$, but sufficiently, however, to allow it to be presumed that the formula expresses the composition of this body. In order to facilitate comparisons, the following are the results which he found with regard to those calculated by the formula $N H^3 H S + 4 S$:—

I.	II.		*Calculation.*
17·45	16·79	ammonia . . .	17·40
15·80	16·43	sulphuretted hydrogen	17·33
64·57	64·83	sulphur . . .	65·27
2·18	1·95	loss	,,
100·00	100.00		100·00

The author regards this compound as an anhydrous penta-sulphammon, which he supposes to contain 18·41 of ammonium and 81·59 of sulphur.

The second compound, which, as it has been said, was formed by the decomposition of penta-sulphammon, was a hepta-sulphammon, which may be obtained directly crystallised from solutions, and which the author prepared by re-dissolving penta-sulphammon in its supernatant liquors, and by cooling the hot liquor contained in a capsule under a bell placed on a glass plate. For a long time isolated bubbles were disengaged, and finally crystals were deposited, which were immediately distinguished, by their ruby color, from those of penta-sulphammon. The author did not determine the form of the crystals; he says only that it was different from that of the salt from which they were obtained.

In general, hepta-sulphammon has the properties of penta-sulphammon, but it offers greater resistance to the influence of the air, especially when it is secluded from the direct rays of light and heat; it rather more difficultly dissolves in water; and with hydrochloric acid the decomposition also proceeds less rapidly.

By analysis, numbers were obtained, which, according to the formula,

$$N H^3 H S + 6 S,$$

represent experiment very well.

Experiment.		*Calculation.*
13·00	ammonia	13·12
12·92	sulphuretted hydrogen .	13·06
75·09	sulphur	73·82
100·00*		100·00

Consequently the formula $N H^4 S^7$, expresses the composition of this body, which

* 101·01 ? Ed. Chem.

should contain 13·88 ammonium and 86·12 sulphur. It presented a degree of sulphurisation of which there was no example, and the highest known, with the exception of arsenic.

The two new compounds, which are penta and hepta-sulphammon, and which may easily be prepared by means of an excess of sulphur or of proto-sulphammon, possess also some other properties when they are heated. Penta-sulphammon very soon passes, by a gentle heat, to the higher degree of sulphurisation, and the latter commences to undergo decomposition at the fusing point of sulphur. There is disengaged in both a combination of a lower degree of sulphurisation, which is deposited in small yellow drops on the cold sides of the vessel, but which, on the application of heat, abandon sulphur, and are converted into very volatile crystals (a sulph-hydrammon?); then there is formed round the crystals, while their color is still the intense red, a layer of very light yellow melted sulphur, into which the whole compound is finally converted.

The author concludes by observing, that he has not had time to try to obtain under the solid form other inferior sulphurous compounds of ammonium, the existence of which has thus become probable, and he regards as such the yellow crystallised substance which is obtained, according to chemical works, by passing vapor of sulphur with ammoniacal gas into a tube heated to redness, and which is, in his opinion, so much the more pale, as it contains less sulphur; however, M. Fritzsche proposes to resume this subject, which promises interesting results.

ON THE SLOW ALTERATION, IN THE TURF-PITS OF DENMARK, OF OIL OF TURPENTINE OR OF A SUBSTANCE ISOMERIC WITH IT.*

BY M. FORCHAMMER.

From observations made by the author and other men of science, it appears that Denmark was formerly covered with a forest of fir trees; this vegetation has disappeared, but the trunks and roots of those firs are still to be met with in almost all the turf-pits in Denmark. There had been found in these turf-pits a crystallised matter, which was confounded with the scheererite of Uznach, in Switzerland. The author examined these crystals *de novo*, and found them to be formed of two substances, which he called, one, *tekoretine*, on account of its easy fusibility, and the other, *phylloretine*, because it crystallises in fine scales. These two matters are soluble in boiling alcohol, but as tekoretine crystallises before phylloretine, it may be separated by repeated crystallisations.

Tekoretine is colorless: it crystallises in large prisms: it fuses at 45° C.; it boils at about the same temperature as mercury, and distils without alteration. Its density, at 11° 25° C., is 1·008. As it is more dilated than water, it swims on the surface of that liquid, when the temperature of the water is raised. It is insoluble in water, very soluble in ether, and sparingly so in cold alcohol.

It was distilled over potassium without being altered; it therefore contains no oxygen. It presented the following composition:—

	Formula.	*Theory.*
C. = 87·17	C^5.	C. = 87·19
H. = 12·84	H 9.	H. = 12·81

There might be seen in this substance a combination of hydrogen with essence of turpentine, and its composition might be represented by $C^5 H^8 + H$; but this formula cannot be admitted, for it is known that by passing chlorine into essence of turpentine, a certain quantity of hydrochloric acid is formed at the expense of the hydrogen of the essence, which thus produces, with an undecomposed portion of essence of turpentine, artificial camphor. The author has ascertained that tekoretine treated by chlorine does not act like hydruret of essence of turpentine.

Chlorine, in acting on tekoretine, produces different chlorous compounds, and at the same time forms hydrochloric acid. As the matter, in taking up chlorine and losing hydrogen, becomes less fusible, the author was able to obtain a complete saturation only by dissolving in ether tekoretine which had already been submitted to the action of chlorine.

By stopping as soon as no more hydrochloric acid is disengaged, after having raised the temperature of the mixture, the ethereal liquid is thrown into boiling alcohol, which deposits, by cooling, a crystallised body of a yellowish brown, whose formula is $C^{40} H^{44} Cl^{16} + H^2 O$. Nitric acid converts tekoretine into oxalic acid, and an apparently nitrogenous resin.

Phylloretine fuses at 87·5° C.; it boils at the same temperature as tekoretine. It is colorless; it crystallises in micaceous folia; it is insoluble in water, and very soluble in ether; it is more soluble in alcohol than tekoretine. Potassium does not alter it. It contains

	Formula.
C. = 90·18	C^5.
H. = 9·24	H^6.

* *Liebig's Annalen*, January, 1842.

Supposing that it is essence of turpentine which has been converted into tekoretine and phylloretine, the following formula may account for this transformation :—

$$\underbrace{C^{15}\,H^{24}}_{\text{3 atoms of essence of turpentine.}} = \underbrace{C^{5}\,H^{6}}_{\text{Phylloretine.}} + \underbrace{C^{10}\,H^{18}}_{\text{Tekoretine.}}$$

By treating fossil deal wood with alcohol, evaporating the liquor, and re-dissolving the residue by ether, there is obtained, by the evaporation of the ethereal liquid, a crystallised resin, which the author calls *xyloretine*.

This resin melts at 165° C. ; it is insoluble in water, and dissolves in alcohol and ether. Its composition is :—

	Atoms.	*Theory.*
C. = 76·97	C^{40},	C. = 79·02
H. = 10·87	H^{66}.	H. = 10·64
O. = 10·16	O^{4}.	O. = 10·34

This substance differs from sylvic acid by only two atoms of hydrogen. The author is persuaded that these two substances are not isomeric, and that the difference, although slight, which analysis has demonstrated, does not arise from an error in the experiment, for he always took care to avoid the presence of hygrometric water. Xyloretine, treated by potassium, disengaged hydrogen and combined with the potassa formed. The author, therefore, regards it as an hydrate, whose formula is $C^{40}\,H^{64}\,O^{3} + H^{2}\,O$. It may be considered as formed by an inferior degree of oxidation of the radical of sylvic acid.

When fossil deal and its bark were boiled with alcohol, the liquor deposited a pulverulent matter of a grey brown color, which the author calls *boloretine*. Boloretine melts at 76° C.

Boloretine contains—

	Atoms.
C. = 75·50	C^{40}.
H. = 11·70	H^{75}.
O. = 12·80	O^{5}.

By representing the composition of this substance by $C^{40}\,H^{64} + O^{5}\,H^{10}$, it is evident that boloretine might be considered as an hydrate of essence of turpentine; but the author has not succeeded in reproducing essence of turpentine by treating boloretine by anhydrous phosphoric acid.

Moreover, boloretine is met with in most of the varieties of turf, and in the pine leaves.

Finally, the author submitted amber to several experiments; he ascertained that the part of amber which is insoluble in ether and alcohol, and which he calls *succinine*, has the composition

	Atoms.	*Theory.*
C. = 79.69	C^{40}.	C. = 79·27
H. = 10·22	H^{64}.	H. = 10·35
O. = 10·19	O^{4}.	O. = 10·38

It is evident that this substance is isomeric with pinic and sylvic acids. The author thought to find in the part of amber which is soluble in ether, a substance having a great analogy to boloretine.

REPORT ON A WORK BY M. TROMMER, RESPECTING A MEANS OF DISTINGUISHING FROM ONE ANOTHER GUMS, DEXTRINE, GRAPE AND CANE SUGAR, AND ESPECIALLY GRAPE SUGAR IN VERY SMALL QUANTITY.*

BY M. MITSHERLICH.

M. Trommer mixes the solution of these substances with a solution of potassa, then adding a weak solution of sulphate of copper. The solution of the gums gives a blue precipitate, insoluble in the alkaline water, soluble in pure water, and which may be boiled without becoming black—a proof that this precipitate is not hydrated oxide of copper, which, at 100° C., gives up its water, acquiring a black color, but, indeed, a compound of gum and oxide of copper.

Amylon and gum tragacanth act in the same manner when they are dissolved in hydrate of potassa, and when a solution of sulphate of copper is added. A solution of dextrine, on the contrary, gave not a trace of precipitate, but a liquor of a deep blue, which, when left to repose for some time, underwent no change, and which, heated to 85° C., yields, in the form of a red crystalline precipitate, an oxide of copper, which may be completely dissolved in hydrochloric acid.

If, to a solution of gum, a little dextrine be added, a blue liquor is always obtained with the precipitate; and if a little gum be added to the dextrine, a blue liquor and a precipitate are formed. Dextrine, therefore, is a substance entirely different from gums, and the manner in which it acts enables us to detect its presence in gum arabic or any other kinds of gum. If we add to a solution of grape sugar and potassa, some solution of sulphate of copper, as long as the hydrated oxide of copper which is separated is dissolved, we find at the ordinary temperature, and after a very short time, that there is a separation of oxide of copper; and if the solution be heated,

* *Academy of Sciences, Berlin*, meeting of June 21, 1841.

there immediately separates from it, even when very little sulphate of copper has been added, oxide of copper, and the liquid very soon becomes colorless. A liquor which contains only $\frac{1}{100000}$ of grape sugar, gives, when boiled, a very evident precipitate; and when it contains only $\frac{1}{1000000}$, it may be perceived by making the light fall on it, that it has a very sensible reddish color.

A solution of cane sugar, to which potassa has been added, and then sulphate of copper, is colored of a deep blue: it may even, where an excess of potassa is used, be boiled without any oxide of copper being separated, and this separation can be effected only by long boiling; when the experiment is made without raising the temperature, the solution may remain several days without undergoing any change, and it is only by very long boiling that oxide of copper can be separated. There was likewise only a very slight separation of this oxide when left for a long time to settle, and after several weeks, the reduction of the peroxide of copper into the protoxide was not quite complete.

By this means, it may very easily be demonstrated that cane sugar to which yeast has been added, is at first very readily converted into grape sugar, which immediately enters into fermentation. Sugar of milk acts in the same manner as grape sugar: however, it still more quickly operates the reduction of the peroxide of copper into the protoxide.

This method is particularly valuable for detecting the slightest traces of grape sugar, and for seeking it in chyme, chyle, and blood. It has hitherto been impossible by this means to discover its presence in blood, although $\frac{1}{10000}$ of grape sugar added to blood may easily be appreciated in that liquid.

ON THE DECOMPOSITION OF WATER BY IRON.

BY JOHN VIPER.

When a stream of hydrogen is passed over ignited oxide of iron, the oxide is reduced to the metallic state, and water is generated. On the contrary, when watery vapor is brought in contact with red-hot metallic iron, the oxygen of the water quits the hydrogen and combines with the iron. The affinity, observes Turner, of oxygen for the two elements, hydrogen and iron, must be either equal, or unequal. If equal, the result of the experiments is determined by modifying circumstances. But, if the forces are unequal, the decomposition in one case must have been determined by extraneous causes, in direct opposition to affinity. Now as regards the equality or inequality of the attractive forces, it is impossible to decide, nor is it very material. The question to be solved is, what are the modifying circumstances, and I think it must be ascribed to the mutual repulsion of gaseous particles.

The part which repulsion plays in this instance, is similar to that which cohesion acts in the decomposition of one salt by another, when one of the produced salts is insoluble. Any explanation as to the *manner* in which cohesion or repulsion acts, involves this supposition, namely, that when atoms susceptible of combination come within a certain distance of each other, they begin to exercise the peculiar attractions and repulsions incident to them in their future state.

By passing steam over ignited iron, the particles of water and iron are pressed together, with a force proportional to the barometrical pressure to which the vapor is subject; and, as one gas is a vacuum with respect to another, the hydrogen has by its own self-repulsive force a tendency to disperse, and in conjunction with the mutual affinity of the oxygen for the iron, effects the decomposition of the water. In the case wherein a stream of hydrogen is passed over ignited oxide of iron, the atoms, on the same principle, tend to produce water and iron, as in this case the hydrogen is a vacuum with regard to the steam.

ON LITHOCHOLIC ACID.*

BY M. WÖHLER.

M. Wöhler occupied the attention of the Society of Sciences of Göttingen, at its sitting of the 11th of November, 1841, with investigations which he made concerning lithocholic acid. The peculiar substance which Professor Goebel, of Dorpat, discovered a short time ago in an animal concretion, and which he described under the name of lithofellic acid, (*see* The Chemist, No. XXVI., page 45,) M. Wöhler likewise found, and this discovery gave him an opportunity of examining it. As this body is very rare, and as, particularly on account of its existence in the bile, it possesses some interest in a physiological point of view, we may here give in detail the results of his observations.

The concretion which Wöhler examined was procured from a collection of pathological products, but unfortunately did not present any indication as to its origin. Nevertheless, from its color and from the substances which were found mixed in it, and which resembled the colored bodies of the

* *L'Institut*, No. 417, Dec. 23, 1841.

bile, M. Wöhler presumed that it was a biliary concretion, most probably belonging to a foreign animal. It was doubtless because it was regarded as a bezoar, that it was kept. At all events, it was too large for a human biliary concretion. It weighed 40 grammes, and was of an oval form ; its color was brownish, and its appearance waxy. It consisted of a great number of fine layers, easily detached from one another, alternately of a light and dark color, and without crystalline structure. It contained a kind of oblong nucleus, a dense and brown substance, which resembled the remainder of the stone in its principal characters, but only half of it melted, and left, after combustion, a yellowish ash, which had an alkaline reaction, and which contained phosphate and carbonate of lime, with traces of oxide of iron. The principal mass of the stone melted as easily as wax, passed thus to white, and disengaged with a faint aromatic odor a vapor which was not aromatic; it burned with a bright flame, and almost without residue. It was soluble in alcohol, with the exception of a little brown residue. The solution had a greenish color, and on cooling, it deposited, but very slowly, small shining crystals, continually combining to form lamellæ, or scales. Repeated solutions in alcohol, with treatment by animal charcoal, gave perfectly colorless lithocholic acid. This acid, with the exception of a small portion of coloring matter, forms almost the entire mass of the concretion.

The crystals of lithocholic acid are always very small. Under the microscope, they appear like translucid, flattened, hexagonal prisms, with flattened summits. They are hard, easily pulverised, insoluble in water, soluble in very considerable quantity in boiling alcohol, but dissolving slowly, and also crystallising again only very slowly. Their solution has an acid reaction. This acid is only slightly soluble in ether. It melts at 205° C., and concretes, when not heated beyond that degree, in crystals which are not translucid. But if the temperature be carried beyond that indicated, it assumes the form of a translucid, vitreous, non-crystallised mass, which becomes very electrical by friction. If alcohol be poured on it, there are immediately manifested, and with a certain degree of regularity, a great number of small *soubresauts;* and if it be covered with a very fine layer of alcohol, the mass is speedily converted into a heap of regular crystals. But the most remarkable circumstance which vitreous and amorphous lithocholic acid presents, is that its point of fusion is, in this state, 100° C. below that of the crystallised acid, that is to say, it melts at between 105° C. and 110° C. into a soft mass, which may be drawn out in threads. By solution in alcohol, this mass may be crystallised *de novo*. If it be for some time kept fluid at a temperature which has not yet been accurately determined above its melting point, it sometimes concretes at that temperature, and again crystallises. Heated in the air to its melting point, it evaporates in the form of white smoke, of a slightly aromatic odor: it burns with a bright and smoky flame.

Lithocholic acid is readily soluble in caustic ammonia or carbonate of ammonia. It may be freed from all ammonia by evaporation. The solution does precipitate the salts of baryta and lime. This acid is also easily soluble, and in large quantity, in caustic potassa. If the solution is slightly alkaline, it leaves, after evaporation, a translucid, gummy mass, soluble in water, and insoluble in a solution of potassa. The solution by sal ammoniac is milky. The acids form in it a white, thick and coagulated precipitate, which very soon deposits under the form of a powder, and which, after desiccation, is white and earthy. With the microscope, it may be seen that it is not crystalline, but that it consists of very small translucid spheres ; it is evidently the amorphous variety of lithocholic acid, which melts also at 105° C. In the concretion it is under this form.

The solution of the salt of potassa being saturated, gives with the neutral salts of silver and lead a white precipitate, which, on raising the temperature of the liquor, assumes a soft and plastic appearance. The salt of lead appears to be a sesquibasic salt. It gives 32 per cent. of oxide of lead, and by calculation it should give 32·8. A salt of lead, obtained by the saturation of the acid in ammonia, appeared to be a bibasic salt; it gave 41·40 per cent. of oxide of lead, and should have given only 39·5 by calculation.

The salt of silver is continually dissolved during the washing. The solution leaves by evaporation, a pellicle, resembling the cream of milk, and dries without crystallising. The crystallised salt mentioned by Ettling and Will, could not have been obtained by this means. It is presumable that the two states of the acid are continued to the salts, and that we thus obtain salts possessing different properties. The portion of the amorphous salt of silver which is not dissolved—only 0·080 gram.—gave 0·019 of silver, and consequently 25 per cent. of oxide of silver. Ettling and Will obtained 25·63 and 25·33, numbers which are more accurate, because they were obtained by a great number of analyses.

Lithocholic acid is soluble in concentrated sulphuric acid; water renders the

solution turbid and milky. It is also very soluble in acetic acid, and by evaporation it remains in the state of crystals.

The following are the results of two analyses of lithocholic acid :—

I. 0·3905 gram. of crystallised acid, dried at 150° C., gave 1·006 of carbonic acid, and 0·374 of water.

II. 0·3515 gram. of acid yielded 0·909 of carbonic acid.

These results give for its composition, supposing that the atomic weight of carbon be = 75·854 :—

	Calculated.	Experiment.
40 atoms of carbon . .	70·83	70·83
72 atoms of hydrogen .	10·48	10·60
8 atoms of oxygen . .	18·69	18·57
Atomic weight = 4283·4	100·00	100·00

Which signifies that the crystallised acid is

$$C^{40}\ H^{70}\ O^{7} + Aq.$$

And that the atomic weight of the crystallised acid is 4171. From an analysis of the salt of silver, made by Ettling and Will, this atomic weight = 4212, and from a second analysis = 4276. For the crystallised acid they establish, from their analyses, the formula

$$C^{42}\ H^{74}\ O^{7} + Aq.$$

which infers for the anhydrous acid the atomic weight of 4347·6. Will thinks his formula the most probable, because it agrees with the proportion of the number of atoms of carbon in the crystallised resins, and that lithocholic acid, from all its properties, is nothing more than a resin.

OBSERVATIONS ON THE FUSING POINTS OF CERTAIN BODIES IN THE CRYSTALLISED AND AMORPHOUS STATES.*

BY M. WÖHLER.

M. Wöhler made to the Society of Sciences of Göttingen, at its sitting of November 11, 1841, the following observations :—

We are at the present time acquainted with a great number of bodies of the most varied nature, which crystallise under certain circumstances, and which, in other cases, remain amorphous. In this passage from one state to another they change, as it appears, all their physical properties, their color, their density, their luminous refraction, their specific heat, and their solubility; nevertheless, without being much changed in chemical nature. It may be conjectured that this double mode of existence is even reproduced in chemical combination; and indeed Berzelius furnishes an example which seems to confirm this idea, in crystallised and amorphous pyroracemic acid. The manner in which lithocholic acid acts, which seems to have a different point of fusion in the crystallised and amorphous states, determined Will to make some experiments to ascertain whether this phenomenon did not occur in other bodies. From these trials he thought that he might establish, as a general principle, that every dimorphous body has two points of fusion; at least he proved it in the following bodies :—Sugar, amygdaline, and sylvic and lithocholic acids, all well crystallised bodies, which are converted, by cooling, into a vitreous non-translucent mass, without having lost their property of crystallising. In this state all these bodies have a lower fusing point than when in the crystallised state.

	Crystallised State.	Amorphous State.
Sugar	160° C.	90°—100° C.
Amygdaline . .	200°	125°—130°
Sylvic acid . .	140°	90—100°
Lithocholic acid	205°	105—110°

It is difficult very precisely to determine the fusing point of amorphous bodies, because the state of liquidity is always preceded by a state of softening, which is peculiar to them; but at the indicated temperatures these bodies were so softened, that they could be drawn out in threads. The same difference in the temperature of fusion is observed in ordinary glass and crystallised glass (devitrified glass, (*verre devitrifié*) Reaumur's porcelain), and especially in sulphur; it is doubtless owing to analogous circumstances. At a temperature of 90° to 100° C., the softening of the last-mentioned body is so considerable, that small spheres placed on one another immediately form a single mass. Crystallised sulphur does not melt, as is known, before it attains 111° C. It must therefore be concluded that the two dimorphous kinds of sulphur have also different points of fusion. That of transparent and vitreous arsenious acid is very probably less elevated than the point of evaporation of the crystallised acid, and the fusibility of the first probably rests on the circumstance that at a certain temperature it first becomes amorphous.

* *L'Institut*, No. 417, December 23, 1841.

ON THE COMPOSITION AND PROPERTIES OF THE RACEMATES.*

BY REMIGIUS FRESENIUS.

In the important investigations which we are about to relate, the author wished to establish differences between the properties of racemates and those of tartrates. He ascertained that the racemate of potassa does not form a double salt with racemate

* *Liebig's Annalen*, January, 1842.

of soda. Racemic acid should therefore be regarded as a monobasic acid. The neutral racemates are always formed of one equivalent of acid and of one equivalent of base, which replaces the equivalent of water retained by the acid when dried at 100° C. The author obtained a neutral racemate of ammonia and an acid racemate which presented the general composition of ammoniacal salts. The author ascertained that the acid racemate of ammonia had not the property, like the acid tartrate of ammonia, of combining with arsenious acid.

The formula of the neutral racemate of potassa is $\overline{R}$ P O + 2 Aq.—and it loses by desiccation 13·52 of water.

The biracemate has the formula $\overline{R}$ K O + $\overline{R}$ H^2 O.—Racemic acid forms, like tartaric acid, a very soluble compound with boracic acid and potassa; this compound is not deliquescent.

The author analysed the neutral racemate of soda, R S O—as well as the acid racemate which is represented by the formula $\overline{R}$ S O + $\overline{R}$ H^2 O + Aq.—It is evident that this salt has the same composition as 2 equivalents of crystallised racemic acid, in which one of the equivalents of water is replaced by an equivalent of soda.

By saturating acid racemate of soda by ammonia, the author obtained a salt crystallised in quadrilateral plates, whose composition was $\overline{R}$ S O, Raz^2 H^8 O + 2 Aq. It is evident that this salt corresponds with the acid racemate of soda, in which one equivalent of water, oxide of hydrogen, is replaced by one equivalent of oxide of ammonium. Finally, the author, in analysing the racemates of baryta, strontia, lime, magnesia, manganese, nickel, and copper, demonstrated in a positive manner that racemic acid should be regarded as a monobasic acid, and that in this respect it essentially differs from tartaric acid, which is bibasic. These investigations fill up a vacuum in the history of racemic acid.

ON THE COMPOSITION OF NICOTINE, AND OF SOME OF ITS COMBINATIONS.*

BY V. ORTIGOSA.

The author prepared nicotine by the following process:—He put the powder of tobacco in maceration for twenty-four hours in water acidulated with sulphuric acid; he expressed the liquor, evaporated it to a syrupy consistence, and distilled it with suitable quantities of potassa: he added from time to time water to the retort to avoid too great a concentration of the potassa, which would decompose the nicotine. He obtained by distillation a mixture of nicotine and ammonia, which he neutralised by means of oxalic acid. He evaporated to dryness; he treated the two oxalates by absolute alcohol at the boiling temperature, which dissolved the oxalate of nicotine, and separated the oxalate of ammonia. He then heated the oxalate of nicotine with a solution of potassa, and treated it by ether, which dissolved the nicotine.

The mixture of ether and nicotine, submitted to distillation, gave nicotine which the author did not regard as pure; he thought that it still retained water and alcohol. This nicotine, submitted to analysis, gave the following numbers:—

	I.	II.
C.	= 66·60	68·05
H.	= 9·37	9·45

Potassa decomposes nicotine.

NICOTINO-CHLORIDE OF PLATINUM.

If chloride of platinum be treated by hydrochlorate of nicotine, we obtain a yellow crystalline precipitate, sparingly soluble in water, insoluble in alcohol and ether, and soluble, by the aid of heat, in dilute hydrochloric acid. This salt crystallises in oblique tetrahedons. It is formed of—

		Atoms.
C.	= 21·02	C^{10}.
H.	= 3·12	H^{18}.
N.	= 4·90	N^2.
Cl.	= 36·79	Cl^6.
Pt.	= 34·17	Pt.

The rational formula of this salt should be C^{10} H^{16} N^2 + Cl^2 H^2 + Pt. Cl^4.

Pure nicotine should be composed of—

	Hundredths.
C^{10}.	C. = 73·26
H^{16}.	H. = 9·65
N^2.	N. = 17·09
	100·00

NICOTINO-CHLORIDE OF MERCURY.

This salt is obtained by precipitating a solution of nicotine; this precipitate is white, crystalline, insoluble in water and ether, and sparingly soluble in alcohol; it is partially decomposed before 100° C. It is formed of:—

		Atoms.
C.	= 27·64	C^{10}.
H.	= 3·64	H^{16}.
N.	= 6·45	N^2.
Cl.	= 16·13	Cl^2.
Hg.	= 46·14	Hg.
	100·00	

* *Liebig's Annalen*, January, 1842.

PREPARATION OF OXIDE OF PLATINUM.*

BY M. WITTSTEIN.

THE author commenced by preparing sulphate of platina by dissolving platinum in nitro-muriatic acid, and by afterwards heating the mass with sulphuric acid; for 24 parts of platinum, 23 parts of sulphuric acid were taken.

The liquid was evaporated in a sand-bath, and continually stirred; a pulverulent, blackish residue was then obtained, which was entirely soluble in water. The sulphuric acid was removed by means of nitrate of baryta, and precipitation was effected by carbonate of lime, and boiling the liquor: a brown precipitate was obtained, which was washed with acetic acid, to remove the excess of carbonate of lime.

Oxide of platinum, thus prepared, is of a dull brown; it detonates when heated.

By analysing this compound, the author ascertained that it should be regarded as a bihydrate, which he represents by the following formula:—

$$Pt\,O^2 + H^4\,O^2.$$

COMPOUND OF CHROMIC AND SULPHURIC ACIDS.†

BY M. WITTING.

ACCORDING to the author, a regular octohedral crystallisation may be obtained by acting with sulphuric acid on bichromate of potassa, or, better still, chromate of lead. As in the crystallisation of ammoniacal copper, to obtain more regular crystals, we may put on the liquid a layer of alcohol.

INVESTIGATIONS CONCERNING A RESIN EXTRACTED FROM BALSAM OF COPAIBA.‡

BY M. FEHLING.

THE author examined a slightly crystalline deposit formed in balsam of copaiba of good quality. He determined the composition of this resin in the hydrated and anhydrous states. Its atomic weight was deduced from the combustion of the salts of lead and silver. By analysis, it gave the following results.

HYDRATED RESIN.

			Atoms.
C.	=	72·345	C^{40}.
H.	=	9·022	H^{60}.
O.	=	18·633	O^{3}.
		100·000	

ANHYDROUS RESIN.

C.	=	76·274	C^{40}.
H.	=	8·805	H^{56}.
O.	=	14·921	O^{6}.
		100·000	

This resin is isomeric with the oxysylvic acid of Hess, which contains—

C.	=	72·14
H.	=	8·74
O.	=	19·12
		100·00

This resin, treated by nitric acid, gave rise to two products: one soluble in water, and containing no nitrogen; the other insoluble and nitrogenous.

Finally, the resin evaporated to dryness with nitric acid produced a black mass, which, treated by alcohol, left a residue insoluble in water and soluble in alcohol, which the author compared with ulmic acid. This matter contained 65·47 of carbon and 5·43 of hydrogen.

ON THE SULPHATES OF CHROME.*

BY M. SCHIOTTER.

OXIDE of chrome forms with sulphuric acid compounds which have the greatest analogy to those of the same acid with alumina.

The sulphates of chrome present the following composition:—

$$Cr^3\,O^3 + 2\,S\,O^3 -$$
$$Cr^2\,O^3 + 2\,S\,O^3 + 15\,H^2\,O.$$
$$Cr^2\,O^3 + 3\,S\,O^3 -$$
$$Cr^2\,O^3 + 3\,S\,O^3 + 15\,H^2\,O.$$

The author obtained an alum of chrome and soda, which corresponded with soda alum with an aluminous base, as well as an alum of chrome and ammonia, which presented the same composition as ammonia-alum. These two alums contained only two equivalents of water.

He also analysed the hydrated oxide of chrome which is represented by the formula $Cr^2\,O^3 + 6\,H^2\,O$.

M. Schiotter likewise investigated the alterations which the salts of chrome undergo under the influence of a temperature of 70°. He ascertained that the passage of the green modification into the blue was owing to the combination of the salts of chrome with different quantities of water. He compares this hydratation with that which phosphoric acid undergoes under certain circumstances. He relies on these experiments for showing how it is that the alum of chrome can be immediately formed only with the blue sul-

* *Buchner's Repertorium.*
† *Ibid.*
‡ *Liebig's Annalen*, Oct. 1841.

* Poggendorff's *Annalen.*

phate of chrome, while it is only very slowly obtained by operating with the green sulphate.

LONDON ELECTRICAL SOCIETY.

TUESDAY, MARCH 15TH.

WE have much pleasure in communicating the results attendant on a repetition, by Mr. Weekes, of the experiments at Broomfield, in which certain Acari became visible. The philosopher of Sandwich has taken every precaution to exclude the agency of extraneous causes. He prepared solutions of silicate of potassa, by the agency of furnace heat, with flint taken from the centre of a block. Moreover, he placed the solution, with many precautions, in a bell-glass over mercury. After a voltaic action of eleven months, insect life was developed. The experiment, which commenced December 3rd, 1840, is still in progress; and insects are continually seen.

In order to submit the question to a still more rigorous test, he at the same time submitted a portion of the solution to a like action, within a bell-glass of oxygen. The battery in this instance was a water-battery. In fifteen months very fine specimens of Acari were seen. To subject the experiment to a still severer test, he is about to put a third apparatus in action, so constructed as to banish all idea of foreign agency entering into the elements. We have not space to follow the writer through the minutely descriptive account both of what he *did*, and what he *saw*.

Among the papers read was one of considerable importance in electro-chemistry, inasmuch as it contains a mass of practical information deduced from actual experiment, by James P. Joule, Esq., Mem. Elec. Soc. The theory of local action is cleverly and practically commented on. The relative value of several voltaic arrangements is given in four deductions. We must refer to the paper itself for detail, to which an abridged notice could not do justice. Papers on the Gymnotus, on Electrotint, on the Etching of Daguerrotype Plates, and Mr. Weekes's Register for February, were also read.

REVIEW:—

PROCEEDINGS OF THE LONDON ELECTRICAL SOCIETY. Session 1841-2. PART IV. April, 1842. London: Simpkin, Marshall, and Co. Price 2*s.* 6*d.*

THE fourth part contains, like its predecessors, much that is interesting to the now very numerous class of persons who devote their attention to Electrical Science. Among its more valuable contents we may mention Mr. Pine's paper "On the Electrical Relation between Plants and Vapors;" Mr. Weekes' "Monthly Registers of the Electrical, &c. State of the Atmosphere," for December, January, and February; very interesting and important paper, to which we direct especial attention, by Mr. Weekes, entitled, Development of Acarus Crossi [a kind of insect], in a close Atmosphere, over Mercury; with some useful articles by the talented Secretary, Mr. C. V. Walker.

To our previous recommendations of this work little need be added, and nothing requires to be deducted: its high character is not only maintained, but improved.

II. CHEMICAL MANUFACTURES.

ABUSES OF THE ALKALI TRADE.

"Bonis nocet quisquis pepercerit malis."
LABERIUS.

BEING determined to expose to the utmost the inaccuracy of the present system of testing soda ash adopted by the alkali brokers, we again call the attention of the soap trade to the subject.

In our last article, we stated that the amount of alkali contained in the soda ash of commerce is ascertained by means of sulphuric acid of a certain strength, which strength is determined by the quantity of the crystals of soda that the acid will neutralise. Now, such being the case, it necessarily must be of the greatest importance to know the exact amount of alkali which these crystals contain, and to see if that quantity is so uniform and stationary that full and implicit reliance may be placed on this method of testing: our own observations lead us to the contrary conclusion.

It is well known to chemists that the crystallised carbonate of soda is composed of 10 equivalents,—90 parts,—of water, 1 equivalent,—31·3,—of soda, and 1 equivalent,—22,—of carbonic acid; this, by calculation, would make the crystals to contain 21·83 per cent. of real soda.

We have made several analyses of these crystals, and, as we stated last month, we have always found them to contain uncombined water, between their plates, in addition to which they contain, on the average, 0·40 per cent. of chloride of sodium, and 0·60 of sulphate of soda; these of course diminish the per centage of alkali, and we have found that however frequently crystallisation may be repeated, they cannot be entirely purified, and consequently are totally unfit for use as a standard by which to prepare the acid for testing soda ash.

We have thus fully shown that those who are in the habit of calculating the crystals of soda as containing 22·20 per cent. of real soda, are injuring the purchaser to a very serious extent, and it is here one of our chief objects to prove this in such a manner as will for the future effectually prevent them from pleading ignorance as their excuse; not that we attribute their adherence to this system entirely to ignorance; we are too well aware that it is as much, if not more, owing to their dishonesty. We repeat, that this standard should be immediately rejected.

In our former article, we stated that the soda ash of commerce contained certain impurities, and their average amounts, for which no allowance is made to the purchaser; and fearless of the odium which may be attempted to be cast on us, we now unequivocally and distinctly state, that it is a complete system of the most impudent fraud, and that if it be not discontinued, so long as it lasts, the foulest of stigmas will attach to those who are interested in, and connive at, its continuance. We warn these parties that we will spare no pains in searching this matter to the bottom, and that we shall fearlessly and in the most determined manner expose to the scorn and derision of society those who thus lay themselves open to censure.

Want of space, and very pressing engagements, unfortunately prevent us from going further into the subject this month; we will therefore conclude by recommending soap-makers to have every alkali they purchase tested by a chemist, and, if found deficient, to carry the matter before some legal tribunal: the first who does so will merit the thanks of the whole of his brethren. Our strictures on the conduct of the parties in fault will increase with the continuance of the evil, and will cease only with its existence.

REPORT MADE TO THE ACADEMY OF SCIENCES, PARIS, ON THE NEW PROCESSES INTRODUCED INTO THE ART OF GILDING BY MR. ELKINGTON AND M. DE RUOLZ.*

BY M. DUMAS.

The importance of this report and of the numerous applications mentioned in it, induces us to give our readers such an extract as may enable them to obtain the principal results mentioned.

I. GILDING OF BRASS AND SILVER BY THE MERCURIAL PROCESS AT PRESENT IN USE.

After having carefully scoured the piece an amalgam of gold is applied to it, then it is burned. The mercury in evaporating leaves the gold on the surface of the object to be gilded; but, in this process, the workmen being incessantly exposed to the contact of liquid mercury or of the vapor of mercury, experience in the highest degree the fatal effects of poisoning by mercurial emanations.

II. GILDING BY THE HUMID WAY.

This process of gilding, which is very simple in practice, consists in dissolving gold in nitro-muriatic acid, which converts it into perchloride; in mixing the latter with a great excess of bicarbonate of potassa, and boiling the whole for a very long time. Pieces of brass, bronze or copper, previously well scoured, are steeped in the boiling liquor, and the gilding is immediately applied, a portion of the copper of the piece being dissolved to replace the gold which is precipitated.

Mr. Wright, in a note addressed to the Academy, gives the following explanation of this process: according to him, the per-

* Abridged from the *Comptes Rendus de l'Académie des Sciences.*

chloride would not answer the purpose of gilding, and the protochloride produces the best effects; the long boiling of the perchloride with bicarbonate of potassa causes it to pass to the state of protochloride, on account of the organic matters which the bicarbonate contains. When the organic matters are not present, the operation only very difficultly succeeds; but these matters, which are only accidentally present, may easily be substituted by adding to the liquor, sulphurous acid, oxalic acid, or oxalate of potassa, which quickly reduce the perchloride of gold to the state of protochloride.

The committee of the Academy considers this opinion of Mr. Wright to be correct, and regards the liquid to be employed in gilding by the humid way, as essentially formed of a compound of protochloride of gold and chloride of potassium, dissolved in a liquid impregnated with carbonate, and even bicarbonate of potassa.

The proportion of gold deposited in gilding by the humid way is much less considerable than by the dry way. The committee has ascertained that the best gilding by the humid way fixed at the most 0·0422 gr. of gold per square decimetre, while the smallest deposition by means of mercury fixed at least 0·0428 gr.

III. MR. ELKINGTON'S GALVANIC PROCESS.

Mr. Elkington takes 31 grammes 25 centigrammes of gold converted into oxide, 5 hectogrammes of cyanuret of potassium, and 4 litres of water, and boils the whole for half an hour, when it is ready for use. When boiling it gilds very quickly, and when cold, very slowly. In both cases, the two poles of a constant battery are plunged into the solution, the object to be gilded being attached to the negative pole at which the metal of the solution is deposited.

In the experiments made by the committee, on Mr. Elkington's process, brass, copper, and silver were gilded.

By this process, the thickness of the layer of gold may be augmented at will, and the thickness may be estimated by the duration of the immersion.

IV. GALVANIC PROCESS OF M. DE RUOLZ, FOR THE APPLICATION OF A GREAT NUMBER OF METALS TO OTHER METALS.

Gilding.—For applying gold, M. De Ruolz employs a constant battery; but he used such a variety of solutions of gold, that he found cheaper and more convenient ones than those of Mr. Elkington. Thus, he made use of cyanuret of gold dissolved in the simple cyanuret of potassium, in the yellow ferrocyanuret, and in the red ferrocyanuret of that metal. He also employed the chloride of gold dissolved in the same cyanurets, the double chloride of gold and potassium dissolved in the cyanuret of potassium, double chloride of gold and sodium dissolved in soda, and, finally, sulphuret of gold dissolved in the neutral sulphuret of potassium.

Even chemists must be astonished that the last of all these processes,—that which is based on the employment of the sulphurets,—is the most convenient, and that, applied to the gilding of metals, such as bronze and brass, whose sensibility as regards sulphuration is well known, it succeeds wonderfully, giving the finest gilding and of the purest tone.

Jewellers will derive great benefit from this process, but science will also gain much advantage. Thus, in future, there will be no obstacle to gilding at a cheap rate all the copper instruments which so rapidly corrode in our laboratories; we may procure tubes, capsules, and crucibles of gilded copper, which will be good substitutes for vessels of gold; which are sometimes necessary, and which no chemist of the present day possesses.

Among the pieces laid before the Academy, there is a gilded brass capsule, which has very effectually resisted the action of boiling nitric acid.

Steel and iron are very well and solidly gilded by this method, which bears no comparison, in this respect, with the very imperfect processes of gilding on iron and steel. Only, as iron has little affinity for gold, the process is commenced by putting on the iron or steel a cupreous pellicle, which favors the adherence of the gold, and fulfils the office of what is called in dyeing, a mordant. Dessert knives, surgical instruments, arms, and laboratory utensils may receive this layer of gold with economy and facility.

Silvering.—All that we have said with regard to the applications of gold, applies with equal force to those of silver. M. De Ruolz has likewise been able, by means of silver dissolved in cyanuret of potassium, to apply it with the greatest ease.

Silver may be applied to gold and platinum, and also to brass, bronze and copper, in such a manner as to substitute plating. Tin, iron and steel are also easily silvered; for the use of chemists, we have proved that a capsule of brass silvered over may answer the purpose of a silver one, to the extent of resisting the fusion of hydrate of potassa. It will not be uninteresting to look forward to the application of these new processes to the preservation of balances, physical instruments, household utensils, those employed by confectioners and druggists, in the

preparation of food or medicine containing acid, if iron or cast iron be required. These metals, fashioned into covers, (dish-covers?) and covered with a layer of silver, will favor the popular use in France, on account of their cheapness, of objects already common in England. Many covers of silvered iron are already made, by other more expensive and much less perfect processes, at Birmingham, and are in common use in many families in England.

Platinising.—By making use of the double chloride of platinum and potassium, dissolved in caustic potassa, a liquor is obtained which allows of platinising with the same facility and promptitude as in gilding or silvering.

Chemists will find, in this process, a means of procuring large capsules of platinised brass, which combine cheapness with the necessary resistance to saline or acid solutions.

Armorers will turn to account, under different forms, this means of preserving oxidisable and sulphurisable metals, used in the manufacture of arms.

Platinum, thus applied, may be obtained from the crude solution of platinum ore, as the metals which accompany it do not injure the effect. This process reduces the price of platinum to that of silver, and seems to open for platinum, now but little employed, an unlimited sale, particularly in the manufacture of chemical products, and in the concentration of acids (especially sulphuric acid).

The extensibility of platinum is such, that, by the processes of M. De Ruolz, 1 milligramme of platinum is capable of uniformly covering a surface of 50 square millimetres, which corresponds to a thickness of $\frac{1}{100000}$ of a millimetre, analogous, as is evident, to the most attenuated pellicle of which we can form a just idea by direct observation.

Coppering.—The same process is used as for silvering, namely, by means of cyanuret of copper dissolved in the alkaline carbonates; but the precipitation of copper is more difficult than that of the precious metals.

Leading.—By acting on the solution of oxide of lead in potassa, by means of the pile, iron plate, iron, and all the metals in general are leaded.

The manufacture of chemical products will also be benefited by this discovery, in thus obtaining iron boilers lined with lead, and in which the solidity of the iron is united with the resistance of lead to the chemical actions of saline solutions and weak acids.

Tinning.—The new processes may be extended in applications, by giving an easy and prompt means of tinning copper, bronze, brass, iron, and cast iron itself, by operating, without heat, on all kinds of utensils.

Cobalting, Nickeling, &c.—M. Dumas exhibited to the Academy several pieces of metal covered with nickel or cobalt. Nickel is very readily applied to iron, which may become of great importance.

Zincing.—Among the experiments tried by M. De Ruolz, those which refer to the zincing of metals, and of iron in particular, were very interesting to the committee.

Zinc, when applied to iron, doubly preserves it: it protects it like a varnish, and likewise by a galvanic action. This peculiarity accounts for the success which zinced iron has obtained in all the applications in which iron, or iron plate employed cold, did not require all their tenacity, and could support additional expense.

The zincing of iron, made by steeping iron in a bath of melted zinc, has some inconveniences; besides, the iron combining with the zinc, constitutes a very brittle superficial alloy; the iron loses its tenacity, a circumstance which is not perceived, however, except in trying to zinc fine iron wire or very thin plates. Besides, the surface, thus covered with a layer of a not very fusible metal, is always ill-formed.

Thus, by this process fine iron wire cannot be zinced; it would become fragile and deformed: bullets cannot be zinced, as they become misshapen and no longer of the same calibre.

Manufacturers, and those concerned in military affairs and the fine arts, will learn with interest that the processes of M. De Ruolz enable us to zinc, in an economical manner, iron, steel, and cast iron, by means of the pile, with the solution of zinc; by operating without heat, and consequently not interfering with the tenacity of the metal; by applying it in thin layers, and by thus preserving the general forms of the pieces, and even the appearance of their minutest details.

The thinnest plate may receive this preparation without becoming brittle, and may thus be turned to account in roofing buildings.

The committee wished to ascertain that cast iron, and bullets in particular, might be zinced. This application must excite the attention of the ministers of war and marine, especially the latter, for bullets are so rapidly altered at sea, that their dimensions are very soon modified to an extent injurious both to the justness of the tier, and the duration of the pieces. The committee laid a zinced bullet on the table of the Academy.

NEW MODE OF TESTING MANGANESE.*

BY M. A. LEVOL.

As the value of manganese in the arts always depends on the quantity of oxygen which it loses in passing to the minimum degree of oxidation, or, which amounts to the same thing, on the quantity of chlorine which it is capable of disengaging from muriatic acid, it is evident that the consumers often require both of these quantities to be established. Many processes, most of which are very accurate and ingenious, have been pointed out with this view; but as they sometimes are attended with some difficulties to inexpert or inexperienced manipulators, and especially as their execution requires considerable time, I thought, and it is also the opinion of many persons acquainted with the wants of commerce, that it might be useful to publish a new, simple, and expeditious method which I propose for the cases, which are doubtless very numerous, where it is requisite to test manganese rapidly, even though something should be sacrificed as to the very accurate estimation of its quality.

I should say, however, that the method, of which the following is a description, very easily enables us, in half an hour or so, to estimate the quality of a sample of manganese to very nearly half a chlorometric degree.

It is based on the facts, that protochloride of iron containing an excess of hydrochloric acid does not allow to escape a trace of chlorine from any body giving off chlorine put in connection with it, so long as its affinity for chlorine is not satisfied, or, in other words, so long as it is not entirely converted into the perchloride of iron; and in the second place, that chlorate of potassa, put in contact, with the aid of heat, with an excess of hydrochloric acid, disengages from it as many equivalents of chlorine as it contains equivalents of oxygen.

To put this process in execution, the following are required:—

1st. A matrass, capable of containing about three decilitres, with a short and rather broad neck, closed with a cork, fitted with a small, straight, funnel-mouthed tube, drawn out at its inferior extremity.

2nd. An aqueous solution of chlorate of potassa, 100 grammes of which contain 1gr·829 of this salt in a state of great purity; theory and experience indicate that, from these 100 grammes of solution should result exactly 3gr·170 of chlorine under the circumstances of the operation, or 100 chlorometric degrees.

Nothing can be more simple than the manipulations required; they consist, in one case, of weighing 3gr·980 of the manganese to be tested, (it is known that this quantity, in the pure or normal state, should disengage from hydrochloric acid, exactly 1 litre of dry chlorine at 0°, and 0m 760, which weighs 3gr·170: this quantity of chlorine represents 100 chlorometric degrees). In the other case, 4gr·858 of iron in fine and very clear wires: this quantity of iron is capable, being reduced to the state of protochloride, of absorbing, in order to pass to the maximum, all the chlorine produced by means of 3gr·980 of normal manganese.*

The iron is introduced into the matrass, from 80 to 100 grammes of pure and concentrated hydrochloric acid are poured on, the apparatus is closed by means of a cork cut sloping so as to allow the hydrogen to be disengaged, without the air being able to flow in too easily, and it is gently heated to accelerate solution, after having inclined the neck of the matrass in such a manner as to prevent loss by projection: the solution of the iron being thus effected, the 3gr·980 of manganese are added to it, which should be wrapped in paper, as will hereafter be explained: immediately after this addition the apparatus is agitated by a circular motion, and it is closed with the cork fitted with the funnel-mouthed tube; it is then boiled for a few minutes, being agitated all the time; it is then removed from the fire, and a moistened strip of litmus paper, or, better still, paper colored with indigo, is suspended in the upper part of the matrass, which is easily done, by fixing one end of the paper between the neck of the vessel and the cork.†

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* *Journal de Pharmacie et de Chimie*, March, 1842.

* The traces of impurities which this iron contains have no appreciable influence on the results; nevertheless, if it be desired to take them into the consideration, they may be estimated by Berzelius's process, to 0gr·024, and consequently 4gr·882 of iron must be taken instead of 4gr·858. However, this process very naturally presents a means of controlling by one another the iron and chlorate used; 4gr·858 of pure iron, reduced to the state of protochloride, should indeed require 1gr·829 of chlorate of potassa, equally pure, to be converted into the perchloride; this useful test may be performed in a few minutes in the little apparatus used for testing manganese.

† Indigo paper is the most sensible, and it may easily be prepared by steeping very white sized paper in sulphate of indigo, and

At this moment only a portion of the iron is at the maximum, as the manganese of commerce is never pure; and as it would require to be so in order to disengage from the hydrochloric acid the quantity of chlorine necessary for converting the whole of the protochloride into perchloride: now, it is precisely to determine what remains of this salt to be converted into the perchloride that the solution of chlorate of potassa, already spoken of, is employed; it is poured from a bottle filled with it, which is previously weighed, into the matrass by the funnel-mouthed tube, until the decoloring of the blue paper announces that the chlorine begins to predominate, when it is certain that all the iron has passed to the state of perchloride.*

Arrived at this term, there is nothing to do but to note the weight of the solution of chlorate employed to produce this result; and as 100 grammes of this liquor give, by heat, with hydrochloric acid in excess, $3^{gr}\cdot170$ of chlorine = to 100 chlorometric degrees, it is sufficient to subtract from 100 the number of grammes minus $0^{gr}\cdot5$ of solution of chlorate employed, instantly and without further calculation to ascertain the value of the manganese tested; the thing is so simple that it would be idle to quote examples; but thinking that it cannot be useless to enter into the minutest details in describing a process, I may add that, as much for the sake of expedition as to avoid loss of manganese, it is advisable to pass that oxide from the scale into a cylindrical and rather broad glass funnel with a long beak, on which a small piece of strong paper has previously been rolled, the lower edges of which are united by twisting; the same is also done to the other end, after the introduction of the manganese, so that there is thus easily and quickly obtained a kind of small cartouche containing the exact quantity of manganese, which is thus introduced without fear of accident into the midst of the chloride of iron. In order to accelerate, but principally to avoid errors in the weights, it is well to have two collective weights, one of $4^{gr}\cdot858$ for iron, and the other of $3^{gr}\cdot980$ for manganese.

I may remark, in conclusion, that the method which I have just described is applicable in many other circumstances in which it is required to ascertain the quantity of chlorine liberated.

REVIEW:—

ELEMENTS OF ELECTRO-METALLURGY; *or the Art of Working in Metals by the Galvanic Fluid.* By ALFRED SMEE, F.R.S. Second Edition. London: Longman and Co., and E. Palmer. PART I. price 1*s*.

WE are glad to see a second edition of this work. Having reviewed the first edition at considerable length, there is no occasion for us again to go over the ground.

This edition is to contain, according to the advertisement, the new processes of Electro-gilding, Electro-plating, and the improvements introduced into the Electrotype process. "More than one third original matter has been added."

We may in a future No. recur to the subjects of the present part.

III. PHARMACY, &c.

THE PHARMACEUTICAL SOCIETY.

IN reply to some letters which we have received, and, in particular, one of an especially expostulatory character, from a correspondent who arrogates to himself the title of "FAIR PLAY," we may observe, that our strictures on the Pharmaceutical Society have been directed more to the injudicious management of its affairs; to the inefficiency to which that mismanagement has given rise; and we have throughout hoped that the abuses would cease as soon as made evident. To point out deficiencies in the administration of the affairs of this Society, is a duty, which, as the first periodical in which the rights of chemists and druggists were advocated, or in whose pages their calling was mentioned otherwise than with the most contemptuous expressions, such as, "impostors," "impudent quacks," "ignorant chemists," &c., we owe to that

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afterwards washing it with slightly alkalised water, then with pure water, and leaving it to dry in the air.

* Experiment proves that it is sufficient to pour into a matrass of the capacity of 3 decilitres, *containing only pure hydrochloric acid*, from 4 to 5 decigrammes of the solution of chlorate of potassa to produce the decoloration of the paper.

body whose members were rejoiced at the establishment of such a work as THE CHEMIST.

At the period when, as we have said, no one deemed it of any consequence to advocate the cause of the oppressed and suffering druggists, impressed with the injustice of the attacks made on them by the apothecaries, through the medium of THE LANCET, and other medical journals interested in upholding the latter in their aggressions and aspersions on the former, we stood forth to vindicate them, and to assist them in procuring a just and equitable establishment of their rights. We defended them boldly and alone; unaided, for a time, even by themselves; we aroused them to a sense of their wrongs and to action. We suggested to them, in No. IX. page 281, the formation of a Society for the protection of their rights, —a suggestion which was adopted after an interval of six months, in the establishment of the Pharmaceutical Society. This Society had our support for some months after its establishment; nor would it ever have lost that support, had it not been that it seemed to pander to the sinister designs of a man who, for his own ends, to bring into notice his retail shop, procured its meetings to be held at his own house. Let it not be for a moment supposed that we here allude to Mr. Pigeon, who so kindly and disinterestedly afforded accommodation to those who were entrusted with the formation of the Society; his respectability and long standing in the wholesale trade must ever shield him from any such imputation. The individual to whom we refer cannot be mistaken.

The mismanagement of which we have spoken consists chiefly in the exorbitant amount of subscription demanded from provincial druggists, for which no equivalent is offered: these persons, in joining the Society, are paying their money for the advancement of their London brethren. Treat this abuse as they may, it is, and ever will be an abuse: while it exists, country druggists will not join. Why should they pay as much as those who reside in London, and who have the opportunity of attending the monthly meetings, and of profiting by all the advantages which can be derived from the Society? Until this is remedied, the Pharmaceutical Society of *London* would be its more fitting appellation. We do not consider the difficulty by any means insurmountable.

There are many other points of mismanagement into which we have not on the present occasion either time or space to enter: we will do so, however, at another opportunity, and we hope that our observations will be taken in the spirit in which they are meant. Our endeavors have always been directed solely to the advancement of the political position and scientific attainments of chemists and druggists, and in no instance have we been found aught but their firm, unyielding, uncompromising advocate.

The Pharmaceutical Society has done little in the way of improving the political position of druggists, and certainly nothing towards advancing them in scientific knowledge.

In conclusion, we may notice, though scarcely worth while, the remark of "FAIR PLAY," in reference to our observations on Mr. Bell's "conducting the PHARMACEUTICAL JOURNAL;" he, with more candor than liberality,—we had almost said, with more impertinence,—attributes our strictures to "rivalry and jealousy." This remark is too absurd to merit notice: THE CHEMIST is a work with which, from its high and purely scientific character, the PHARMACEUTICAL JOURNAL cannot for an instant be compared. THE CHEMIST never gained any thing by its advocacy of the rights of druggists; neither was anything expected or desired; our motives, therefore, in devoting a certain portion of a scientific periodical to that purpose, were purely those of good feeling towards them; we had no interest to serve; we were unconnected with them as a body. "FAIR PLAY" is therefore perfectly mistaken in supposing that we would prostitute our columns in any such way.

With respect to the letters from correspondents reflecting on the Society, which we inserted, we have only to remark that we should have done the same with those that were favorable, had we received any. The communications speaking in the strongest terms of disapprobation of the management of this Society's affairs, have been sent to us in such great number that we have found room for only very few. Many of them are from MEMBERS!!!

ABUSES OF THE DRUG TRADE.

LETTER III.

THE PER CENTAGE SYSTEM.

To the Editors of The Chemist.

GENTLEMEN,—

At page 247, Vol. I. of THE CHEMIST, the following observations occur:—"There is little room for the highest branches of the profession to complain of the druggists; for it is well known that some of the FIRST physicians and surgeons—even those with the highest titles that can be bestowed on medical men, some of the attendants on ROYALTY,—take a regular 'per centage' from the druggist on every prescription

they send him."—"These facts,—and that they are facts not a doubt can exist,—demand immediate correction: all complaints against quackery and imposture proceed with a very bad grace from a profession teeming, in every grade, with the most disgusting practices, flagrant abuses, and sordid cupidity."—The per centage system is, of itself, but a too powerful corroboration of the truth of the above statement; and much is it to be regretted that a practice so pernicious should be allowed to exist; though "*Herculean*," indeed, must be the efforts which shall uproot a system cherished alike by all classes of the profession; the druggist having equally to thank his brethren for the evils arising therefrom, by reason of the underhanded parts they take in these nefarious contracts.

It is no uncommon thing to see in the daily papers advertisements to the following effect:—"To the Medical Profession.—A Chemist and Druggist of *established respectability* is desirous of entering into an engagement with a Physician or Surgeon in good practice, to dispense their prescriptions.—Confidential communications to be addressed A. L. &c. &c."

Again, it is an invariable practice with dispensing surgeons or general practitioners, on relinquishing their dispensaries, to negotiate with the neighbouring druggists, for the performance of a duty, which they no longer care to incur, always accepting the lowest tender, congratulating themselves on striking the bargain, with the assurance that their remedies will be prepared at all events with as great an amount of attention, both with regard to accuracy and quality, as when concocted at their own pharmacopolium. The contemplated separation of the trade from the profession of medicine is calculated to present a wide field for the propagation of this disgraceful trafficking in the health of the people; and it behoves Medical Journalists, as the guardians of the public health, to be on the alert to prevent the perpetuation of so glaring an injustice.

The practice of physicians, who give advice *gratis*, recommending certain shops, at which only their prescriptions are to be dispensed, is a like evil, and equally reprehensible. Others derive a considerable annuity by tendering *advice gratuitously* at stated periods, at certain chemists' shops, where of course the prescriptions are dispensed; the proportions in which the several parties engaged in these proceedings receive remuneration is irrelevant to our present inquiry; sufficient is it to know that such a state of things exists, and to warn the public from countenancing a system at once detrimental to its interests, and derogatory to the profession.

That this is an evil which would be increased a hundred-fold, by prohibiting druggists from prescribing, cannot be doubted, —a circumstance wholly overlooked by medical reformers, though in itself a greater evil than any other of the many abuses to which the profession is subjected. It is needless to occupy more of your valuable space by commenting upon these delinquencies of the trade and profession of medicine, further than to state that it is recorded of the Salernum School of Medicine, the most celebrated of the middle ages, that amongst other regulations, the physician was forbidden to enter into any lucrative compact with the apothecary or druggist. I will conclude by adding my conviction of the truth of your assertions, "that it will indeed be a Herculean task to restore to respectability a profession so sunk in degradation, and so replete with abuses." And "well, indeed, will that man deserve of society, who succeeds in placing the profession of medicine on a respectable, or even decent, footing."

I am, Gentlemen,
Respectfully yours,
PHARMACOPOLA.

MEDICO-LEGAL REPORT CONCERNING A CASE OF SUPPOSED POISONING BY HYDROCYANIC ACID.*

BY M. ORFILA.

THE following fact, which has lately occurred at Chambery, proves to what an extent human justice may be led into error by unfounded assertions.

A man, aged 64, named Pralet, died almost suddenly, about a year ago, in the capital of Savoy; he was buried; but the suddenness of the symptoms which preceded his death awoke the attention of the magistrates, and the body was ordered to be exhumed: five medical men examined the organs, and unanimously declared that the defunct had been poisoned by hydrocyanic acid. M. Héritier, his nephew, was accused of having committed the crime, and was thrown into prison. M. Orfila, being consulted for the defence, returned a Memoir, of which we here give an abridgment, which completely weakened the conclusions of the five *expertises*. The process was stopped, and the accused transferred to the High Court of Justice of Turin,

* *Archives Générales de Médecine*, Nov. 1841.

where he will probably be acquitted, instead of suffering capital punishment, a fate which was reserved for him.

The physicians had concluded from the symptoms observed during life, and from lesions found in the body after death, that Pralet had died through swallowing a narcotic principle. The chemists, after comparative experiments on the organs of the deceased, and on liquids to which hydrocyanic acid was added, concluded that the matters which had been delivered to them contained a certain quantity of that poison. M. Orfila, discussing their assertions one after another, gives it as his opinion :—

1st. That the symptoms observed in the deceased were not those caused by hydrocyanic acid; that, indeed, that agent uniformly produces general or partial convulsions, followed by sinking.

Now Pralet presented rigidity in one arm, and a deviation of the mouth ; he had not suffered convulsions.

2nd. That the lesions observed after death essentially differed from those which result from the action of hydrocyanic acid. In Pralet, the venous system was empty, the respiratory apparatus little congested, and the brain was the seat of a ventricular hæmorrhage.

In individuals who die from this kind of poisoning, the veins and the lungs are found to be gorged with blood, and the brain healthy.

3rd. That the symptoms and the lesions observed are the effects of a fit of apoplexy.

M. Orfila has had no trouble in establishing that the sudden hemiplegia, the tetanic rigidity of the arm, a symptom which I have proved to exist in all cases of ventricular hæmorrhage of the brain, left no doubt concerning the nature of the disease.

4th. That none of the analyses made at Chambéry, prove that hydrocyanic acid was procured from the organs of Pralet.

Indeed, the reactions obtained by the different medical men, although acting on the same matters, varied sensibly ; they were not sufficiently numerous. Cyanogen gas was not isolated.

5th. Finally, that even if it should be proved that there existed hydrocyanic acid in the nerves of M. Pralet, it would not follow from that that he was poisoned by that acid, as its presence might depend on its production at a certain stage of putrefaction, or from its having been injected into the rectum or stomach after death.

INDIGO IN EPILEPSY.*

BY M. SIMONIN.

A MASON, aged 28 years, of a good constitution, experienced a great fright, and was afterwards submitted to a prolonged cause of cold.

At the end of eight days he was attacked with epilepsy, which returned after an interval of eight days, and left a slight headache. He was bled in the arm. Fifteen days afterwards, he had another fit. Bleeding in the arm not being sufficient, leeches were applied to the arms twice in a month. Another attack after six weeks.

Indigo was then administered, in the dose of 16 grammes, mixed with two grammes of an aromatic powder, and a little syrup.

During two days the patient took half a dose. During the eight days following, the entire dose. Although the treatment was then discontinued, he has remained six years without another fit.

At first sight, this case appears conclusive in favor of the efficacy of the treatment adopted. Indeed, is not the absence of relapses for six years an authentic proof of the solidity of the cure ? This is incontestable ; but was it nature, the bleeding, or the indigo, that effected the cure ?

All practitioners know that epilepsy, suddenly developed in young subjects by an accidental cause, may disappear of itself without the intervention of medicine.

However, I do not pretend that the bleedings did not act favorably. They were indicated, and every thing shows that they diminished the frequency of the fits.

With regard to the indigo, it is impossible to affirm that it had any influence whatever. Indeed, at the time when its administration was commenced, the epileptic attacks had progressively diminished in frequency, to such an extent that an interval of six weeks separated the two last. Who can say that the disease had not terminated previous to its administration ?

Besides, this medicine was given in complete doses for only eight days, and the duration of this treatment will appear very short to those who know with what perseverance we are ordinarily obliged to pursue nervous disorders to obtain a simple amelioration.

TREATMENT OF INTERMITTENT FEVERS BY ARSENIOUS ACID.*

DR. BOUDIN, physician to the Military Hospital of Marseilles, has recently extolled arsenious acid, as a medicine worthy of being substituted for the preparations of quinquina, in the treatment of intermittent fevers. It is not the first time that this

* *Bulletin de Thérapeutique*, Dec. 1841.

* *Journal de Pharmacie et de Chimie*, Janvier, 1842.

dangerous substance has been boasted of as a specific against this malady, which is so common and so obstinate. Employed in Germany since the 18th century, by Slevoght, Friek, Pleuciz (of Vienna); in England, by Arnold, Withering, Freer, Willan, and Pearson; at the commencement of this century, in 1811, by Harles, of Nuremberg; it was introduced into France by M. Fodéré, and tried on a large scale at the Military Hospital, Martigues, in 1810.

Dr. Boudin employs arsenious acid in the following manner:—

℞ Arsenious acid . . . . . $\frac{1}{5}$gr. French.
Add, by small portions at a time,
powdered sugar of milk . 20gr. French.

Triturate in a glass mortar, for at least ten minutes, and divide into twenty packets, each of which will contain half a milligramme, or a hundredth of a grain, of acid.

Administer one packet in a spoonful of water, five or six hours before the fit.

Dr. Boudin adduces, in support of his views, a table which presents a total of more than two hundred cases. The experience of this physician, added to that of his predecessors, incontestably proves the efficacy of arsenical preparations in certain intermittent fevers. But if it is easy to believe that at the time when sulphate of quinine was unknown, it might have been desirable to substitute for quinquina a less expensive and more efficacious febrifuge, it can, at the present, he admitted only with great hesitation, that this valuable medicine should yield the palm to arsenious acid, as to its therapeutical properties.

Besides, we must say that more powerful reasons are required for introducing into medical practice so violent a poison, and also that the pulverulent form adopted by Dr. Boudin presents serious inconvenience which every one will comprehend, and which may easily be avoided by substituting for it that of solution in any vehicle whatever.

REPEATED VOMITING AS A THERAPEUTICAL AGENT IN CROUP.*

BY M. MAROTTE.

M. Marotte treated three children for croup in two months: all three were cured.

He attributes this success to the method suggested to him hy Dr. Larroque, which consisted of making the children attacked with croup vomit, a great number of times within twenty-four hours, so as to detach the pseudo-membrane from the larynx as fast as it was formed. For this purpose, M. Marotte employed one or other of the following formulæ:—

	Gram.
℞ Tartar emetic	00·10
Syrup of ipecacuanha . .	30·00
Water	60·00

	Gram.
℞ Impure emetine	00·20
Syrup of ipecacuanha . .	60·00
Water	30·00

He administered these draughts by spoonfuls, every ten minutes, until there had been a sufficient number of vomitings; in this manner, he was always able to make them expectorate a certain quantity of false membrane.

He did not neglect, notwithstanding, the employment of leeches to the neck, calomel in fractional doses, and blisters to the nape of the neck; but, in his opinion, the vomitings had the efficacious action.

Remarks of M. Boudet.—I the more readily coincide with the opinion of this distinguished practitioner, as I arrived, nearly a year ago, at the same conclusions, after having observed 25 cases of croup at the *Hôpital des Enfans*. The only authentic case of cure which was observed among all these was effected by vomitives.

* *Gazette Médicale de Paris*, No. 1, 1842.

IODIDE OF POTASSIUM IN SECONDARY SYPHILITIC SYMPTOMS.*

M. Lafargue, of Saint Emilion, relates an observation which tends to confirm the advantages which Ricord attributes to this medicine, in some old venereal affections.

He administered it under the following form, recommended hy the last mentioned skilful physician:—

℞ Distilled water . . .	90 gram.
Iodide of potassium .	0·50 cent.
Syrup of poppy . . .	30 gram.

To be taken three times a day, in a glass of decoction of sarsaparilla. The doses should be increased every five days, ahout fifty centigrammes.

Under the influence of this treatment, continued during two months, (care heing taken, however, that the dose of one gramme and fifty centigrammes *per diem* be never exceeded,) and combined with Kluge's antiptyalic draught, the patient, who had deep ulcerations of the pharynx and fetid salivation, and who was reduced to moroseness and despair, from the impossibility of taking nourishment, and from the disgust which he caused to others and himself, completely recovered his health, and at the end of six months there was no return of bad symptoms.

* *Bulletin de Thérapeutique.*

EFFECTS OF COLD DRINKS ON THE BODY DURING PERSPIRATION.*

BY M. GUERARD.

In this work, the author shows, from numerous and authenticated facts, the danger inherent to the use of these drinks, whatever may be their nature, particularly during warm weather. He proves that these symptoms consist of serious phlegmasia of the digestive and respiratory organs, and of nervous discomposure, among which M. Guérard reckons instantaneous death, which too often occurs under such circumstances.

According to the author, the seriousness of the symptoms determined by the cause in question is connected with four conditions: —1. The previous heating of the body; 2. Emptiness of the stomach at the time; 3. The large quantity of drink swallowed in a given time; 4. The low temperature of that drink.

These conditions are ranged in the order of their importance: thus the lowness of temperature stands last, because observation has demonstrated that the cases of instantaneous death have occurred from the use of drinks, at $+12°$ C.; whilst ices most frequently produce less serious and more tardy symptoms.

The author explains their innocuity by the slowness with which they are taken into the stomach; he therefore recommends cold drinks to be taken in small draughts, and at long intervals, in order to diminish the bad effects. He likewise suggests the use, at the same time, of a solid substance, such as sugar, chocolate, bread, &c., according to the practice adopted in some countries.

On the appearance of these symptoms, forced exercise, or if that be impossible, warm local applications and aromatic drinks, may arrest their progress.

Finally, recurrence to cold drinks should be avoided while the stomach is empty, or while the body is heated by walking or dancing; and brisk exercise should be kept, as inactivity favors the accumulation of fluids in the serous cavities.

M. Guérard concludes by observing that, in a medico-legal point of view, the nature of the symptoms and the circumstances under which they occur, should put the physician on his guard against the suspicion of poisoning. Doubts, in some uncommon cases, must give way to microscopic examination and chemical analysis.

* *Académie Royale de Médecine*, Sitting of Nov. 30, 1841.

REVIEW:—

AN INVESTIGATION OF THE PRESENT UNSATISFACTORY AND DEFECTIVE STATE OF VACCINATION; *and the several Expedients proposed for removing the now acknowledged Defects of the Jennerian Process.* In a Series of Letters to Dr. George Gregory, Physician to the Small Pox and Vaccination Hospital, London. By Thos. Brown, formerly Medical Practitioner in Musselburgh. Edinburgh: Maclachlan and Stewart. London: Whittaker and Co.: pp. 139, price 4*s*.

The permanency of the preservative power of the vaccine virus has long been the subject of discussion among medical men, and the question has never been satisfactorily solved. Many and conflicting are the opinions of the most distinguished physicians concerning its efficacy: the practice of one man induces him to rely on it and to extol it to the world; that of another, to view it with distrust and to decry it. That it has not long since been established on a better basis is a disgrace to the medical men of the age. Surely the subject is worthy of searching investigation: its neglect can be attributed only to disinclination for the task, for there are many who are capable of undertaking it, were they willing to do so.

The work before us is, on the whole, an able and important one, and the arguments which it contains are, for the most part, good, although it contains some which are not quite so unanswerable as the author appears to suppose. The object is to call more pointed attention to the question than has hitherto been accorded to it, and we hope that this object will be answered, but we fear that it will not.

The author brings to bear on this work the experience of more than half a century, during which he had very extended opportunities of judging between the relative value of inoculation and vaccination. Some of his arguments, however, are by no means conclusive.

It merits the attentive perusal of all interested in this great subject, whether medical practitioner or parent.

ON GALBANUM.

BY M. LUDWIG.

There is some difference of opinion concerning the plants which produce galbanum, arising from the supposition that it comes from only one plant, notwithstanding that commerce furnishes two entirely different sorts.

The most common opinion relative to the origin of galbanum attributes it to the *Bubor Gummiferum*, L., *Ferula Galbanifera*, Com. Hort., indigenous to the south of Africa, and to the *Bubor Galbanum*, L., *Ferula Galbanifera*, Herm., a rather different species, frequently found in the botanical gardens of Europe.

M. Ludwig does not think that these are the maternal plants of galbanum, because they do not possess the slightest odor of that gum resin.

With greater probability, this drug is attributed to the *Ferula Galbanifera*, Lobel. M. Ludwig does not think that all the galbanum is procured from this plant, but that a quantity is also yielded by the *Galbanum Officinale*, which is found in great abundance in the Levant and Syria; this origin is not yet proved, and requires to be confirmed by local observations.

Three kinds of galbanum are distinguished in commerce:—

1. Galbanum in Grains (*Galbanum in granis*).—In separate grains, but attached to one another, from the size of a small pea to that of a nut, of a yellowish, whitish, or greenish color. The odor is strong, penetrating, and peculiar, but not disagreeable; the taste is sharp, resinous, and rather bitter; sp. gr. 1·212; it softens between the fingers.

2. Galbanum in Masses (*Galbanum in massis*).—Large pieces of a variable color, yellow, greenish, &c., mixed with hairs and other foreign matters, or stalks and petioles.

The odor resembles that of the preceding sort, but is sometimes more penetrating; it is also of a softer consistence.

These two kinds are called by M. Ludwig Levantic galbanum, and they are essentially distinct from the following.

3. Persian Galbanum. — Commerce furnishes it in large masses, packed in the skins of animals. This gum has a reddish brown color, with white lines; it is so soft that it becomes liquid at a low temperature. It is very impure. Its odor is quite different from that of the preceding: it is more penetrating and disagreeable, resembling that of assafœtida. The taste is disagreeable, bitter, and resinous.

From the following considerations, M. Ludwig infers that the last kind has a different origin from the two first:—

1. The color is never greenish, but always reddish brown.

2. The odor is quite characteristic.

3. In commerce it is always more impure, and is never met with in grains, or in the state of Levantic galbanum. The *débris* of the stalks are different and thicker.

4. It is procured from another country. Persian galbanum is always received from Astracan and Oremburg, and it is the kind employed in Russia.

Levantic galbanum is imported into Trieste and Marseilles, and it has but recently been known and used in Russia.

A large quantity of Levantic galbanum, which was deposited for a long time in Hanover, where it was entirely unknown, was recognised by M. Ludwig, and it was very soon sold with advantage, having been found as good as that received from Marseilles.

NEW PROCESS FOR OBTAINING THE RESIN OF JALAP IN A STATE OF PURITY AND PERFECTLY WHITE.*

BY M. A. NATIVELLE.

In a thesis presented to the *Ecole de Pharmacie*, in the month of July last, I pointed out a process for extracting this resin, deprived of all coloring matter; but as this process has since undergone several modifications, I have thought that it was necessary again to describe it, and that in a more detailed manner than I then did. M. Planche has already paid much attention to the preparation of this resin, which he obtained, of a very light color, by removing, as is here done by aqueous but cold solutions, the extractive matter of the roots. The process which I am about to describe therefore coincides with the ideas of that distinguished pharmaceutist.

PREPARATION.

Each root of jalap is cut into two or three pieces, and boiling water is poured on, in order to swell the roots, which would otherwise be too hard to be at first easily divided. Next day, when they have taken up nearly the volume which they might have contained in the recent state, they are cut into as thin slices as possible; water is again added, and they are boiled in a copper basin for about ten minutes, being occasionally stirred: this being done, the whole, liquid and roots, is poured into the box of a press; the deep colored liquor flows out, and the residue is submitted to a kind of pressure. Two similar decoctions are then made, being pressed each time; this precaution is indispensable for removing all the extractive coloring matter contained in the spongy tissue of the roots.

The liquors of these decoctions are rejected as useless; but they might be used for preparing an aqueous extract of jalap.

* *Journal de Pharmacie et de Chimie*, Mars, 1842.

After these three treatments, the water runs out quite colorless, and there remains only pure resin fixed in the ligneous matter. It is then heated by alcohol; for that purpose, the exhausted roots, as they come out of the press, are placed in the sand-bath of an alembic; a sufficient quantity of alcohol at 65° C. is then added. Alcohol at 85° C., which was first directed, has the inconvenience of hardening the roots by removing the small quantity of water which they retain, and consequently impedes the solution of the resin fixed in the interior of the ligneous slices. The sand-bath is then covered with its head; it is boiled for ten minutes; and when the whole is nearly cold, it is pressed in the same manner as directed for the aqueous decoctions. Two similar treatments are also made, after which the roots are entirely exhausted. All the alcoholic decoctions are then mixed; the liquor which results has a very slight amber tint, which soon disappears on a small quantity of finely divided animal charcoal being stirred with it; it is filtered, and then distilled in the sand-bath until nothing passes over. The resin remains fluid at the hottom of the sand-bath, under the water and alcohol: it requires only to be dried: if, instead of operating in a tin sand-bath, an untinned copper one be used, the resin has a blackish appearance, owing to an insoluble matter mechanically contained in it. This matter, which appears to he a comhination of the resin with the copper of the sand-bath, exists only in very small quantity; however, small as that quantity is, it is necessary to remove it for the resin to appear white; and as it is only interposed, it is sufficient, to separate it from the mass, to dissolve the resin in twice or three times its bulk of alcohol of 65° C., to add a very small quantity of animal charcoal and to filter. The alcohol passes off quite colorless; it is evaporated in a capsule placed on boiling water; as the alcohol evaporates, the resin is precipitated under the form of a thick and colorless turpentine; the supernatant water is separated, the resin is spread out on the sides of the capsule, and the heat is continued until it is perfectly dry, when it is so friable that it may easily be reduced to a fine powder. This powder is as white as starch; every kilogramme of jalap, of good quality, gives a hundred grammes of pure resin; a result which agrees very well with the quantitative analysis. This resin has been tried; it is as active as that obtained by the other processes, by which it is not produced in the white state. Three decigrammes, suspended in half a glassful of milk of almonds, acted as a powerful purgative. In the dose of even two decigrammes, its action was almost as energetic. Reduced to powder, and put in contact with cold water, it presents the peculiar characteristic of forming a semi-fluid, transparent mass, as if it had been melted; it has the same appearance as resin which has just been precipitated by water from its alcoholic solution. Under this form, it would seem to re-constitute a hydrate, hy again taking up the water which it had lost by desiccation; however, this character may he used to a certain point for distinguishing the resin of jalap from other resins, which remain pulverulent during their contact with water. If a certain quantity of colophaue be mixed with resin of jalap in powder, and treated by water, the whole mass combines, but with an opaque appearance, occasioned by the colophane, which, as it cannot be incorporated with water, remains interposed in the middle of the mass. However, this character would not be sufficiently accurate for determining a small quantity of foreign resin put in for the sake of sophisticating the pure resin. By an analogous means, the other resins, which, like that of jalap, are entirely insoluble in water, might be extracted.

SUBSTITUTION OF A SACCHARINE PRODUCT FOR MANNA.*

BY M. MEINER.

I AM about to lay before the *Société de Pharmacie*, not an adulteration of manna, but the *substitution* of a saccharine substance for that medicinal product.

This substitution should attract attention, for it is a fraud which appears to me to surpass all others at present practised. Indeed, there is introduced into commerce, under the name of manna, a product which does not contaiu the smallest quantity of it, and its resemblance to the *débris* of manna in tears, is such that one may be deceived unless it is very attentively examined. I some days ago escaped being cheated by an individual who came to offer me 1,100 kil. of this substance, at the low price of 1 fr. the kil., adding that he was compelled by want of money to dispose of it.

This struck me as being suspicious: I should have been deceived, at first sight, as to the nature of the merchandise, but its odor, its peculiar taste, which was that of heated sugar, led me to consider that this suhstance was the result of a mixture of a certain quantity of manna with other substances. I refused to buy it; but, wishing

* *Journal de Pharmacie et de Chimie*, Janvier, 1842.

to ascertain its nature, I took a large sample of it, and I made various examinations of it, which I lay before you.

PHYSICAL EXAMINATION.

This product has a white color bordering on yellow; it is glutinous, and adheres to the fingers; its taste is sweet, but leaves a slightly bitter after-taste; it has not the least flavor of manna; its most characteristic taste is that of partially candied sugar. If a few pieces of this manna be broken, it may be seen that this fracture is granular, but the small crystals which are perceived when manna in tears is broken are not remarked; exposed to the air, it attracts moisture.

CHEMICAL EXAMINATION.

Presented to the flame of a taper, this product did not inflame, as pure and dry manna in tears does; it turned black, and fell in the form of drops, which solidified by cooling; dissolved in water, it furnished a clear solution, whilst even the purest manna yields a solution which is always a little turbid.

The solution of this factitious manna presented, with re-agents, totally different characters from those furnished by genuine manna:—

1. Oxalate of ammonia caused a precipitate of oxalate of lime in the false manna, but not in the genuine.

2. Subacetate of lead, poured into the solution of factitious manna, did not render the liquid turbid; in genuine manna, it formed a precipitate.

3. Chloride of barium gave rise to a precipitate of sulphate of baryta, in a solution of false manna, but had no effect on the genuine.

4. Nitrate of silver, after a certain time, colored the solution of genuine manna; the solution of the false was scarcely changed at all.

5. Finally, by the acid nitrate of mercury, the solution of factitious manna was rendered slightly turbid, whilst this re-agent formed a flocculent precipitate in the genuine.

Fifty grammes of manna in tears of good quality were dissolved in five hundred grammes of distilled water; a small quantity of yeast was added, and it was then put in an oven.

The same operation was performed with fifty grammes of false manna.

Both the liquids, during their stay in the oven, underwent fermentation, which I had expected, since it is known that Bouillon-Lagrange, by experiments made in 1816 (*Journal de Pharmacie*, vol. 3, p. 11), demonstrated that manna, dissolved in water, may partly undergo alcoholic fermentation when placed in suitable circumstances but I ought to say that the fermentation is most marked in the liquid containing the false manna.

These two liquors, which had undergone fermentation, were afterwards filtered, and both left residues after evaporation, but not of the same weight.

The residue left by the genuine manna was twenty-five grammes; that left by the solution of the false weighed only eight.

The residue resulting from the genuine manna, having been treated by boiling alcohol, was dissolved in that vehicle; but, by cooling, this alcohol deposited mannite under the form of crystals.

The residue of the false manna, likewise treated by alcohol, gave not the slightest trace of mannite by cooling.

The true manna immediately treated by boiling alcohol, likewise, on cooling, allowed mannite to deposit; and the factitious manna, treated in the same way, deposited only a syrupy matter, and not the least portion of mannite.

From the foregoing, I am convinced—

1st. That the product offered to me under the name of manna, did not contain a particle of that substance.

2nd. That its appearance, its taste, its *modus operandi* with yeast, and the presence of sulphate of lime, seem to demonstrate that it was prepared with sugar of starch.

I am not aware whether any purgative matters have been added to this sugar, but nothing in my experiments has indicated their presence.

INSOLUBLE LABELS FOR BOTTLES.

To the Editors of The Chemist.

Newcastle, Dec. 1841.

GENTLEMEN,—

A correspondent of your periodical, signing himself John Fordred, intimated, in a recent number, his desire to be made acquainted with the composition of a varnish to cover chemists' bottles, of such a nature as not to be acted on by oils or spirit. Permit me, therefore, through the medium of your work, to inform him of a method which I have adopted with great success, and requiring but little trouble. It is as follows:—

Lay on a coat of albumen, or white of egg, over the label, and immediately put the vessel into the upper portion of a common steam-pan, until it coagulates and turns opaque, when it may be taken out and dried in an oven, at a temperature of about 212°. By this last operation the opaque white albumen will become quite

hard and transparent, and on which neither oils nor spirits have any effect.

I am, Gentlemen,
Your obedient Servant,
HENRY J. SWANBROOK.

HOULTON'S ACETUM OPII.

BY M. BUCHNER, SEN.

℞ Pure opium . . 2 oz. 4 scrup. (63 gram.)
Concentrated acetic acid . 1 oz. (29 gram.)
Distilled water, 9 oz. (263 gram.)

Digest, at a gentle heat, for four days: filter. Four drops of this tincture are equivalent to 1 grain of opium.

The medicinal action of this preparation is so remarkable, says M. Buchner, that the physicians who have used it cannot praise it too highly.

It calms and abates spasms and pains; it is somniferous, without producing constipation, as do pure opium and other tinctures of opium. The action of this tincture particularly resembles that of acetate of morphia, for, by dissolving acetate of morphia in distilled vinegar, almost the same effects are observed; but the acetic tincture of opium has the advantage of being cheaper, and of containing all the active principles of opium. It may be employed in doses of two, four, six, and eight drops.

ON THE USE OF VARNISH IN THE EMPLOYMENT OF THE STARCH BANDAGE IN THE FRACTURES OF CHILDREN.*

BY M. TAVIGNOT.

IT has hitherto been impossible to use the starch bandage in treating fractures of the femur in children, on account of its becoming soft and loose from their constantly passing their urine in bed. M. Tavignot, who is attached to the Hôpital des Enfans Malades, has latterly been able to employ it in the following manner:—The limb is first surrounded by a roll of varnished paper, the bandage is then applied, and the upper part of the varnished paper folded back upon the bandage like a frill. The apparatus is thus kept quite dry. When it is necessary to remove it, the child is put into a warm bath, which softens the bandage; it is then easily taken away.

† *Gazette Méd. de Paris*, 25th Sept. 1841; and *London and Edinburgh Monthly Journal of Medical Science*.

POISONING BY CYDER CONTAINING A SALT OF LEAD IN SOLUTION.*

WITHOUT going back to a very distant period, we may find numerous examples of the serious effects produced, not only in France, but in neighbouring countries, by beverages containing salts of lead. It is well known that wine merchants have the deplorable habit of sweetening, by the addition of litharge, wines which are still new or which have turned acid. It is to this fatal practice that the epidemic of colics observed at Paris, by Bourdelin, in 1775, must be attributed, as well as that described by Zeller in Germany, and Litois in France.

The introduction of litharge into cyder appears to have been frequently practised in Normandy; for the parliament of Rouen, in 1775, forbade that province to correct the acidity of cyder by means of lead.

Facts which have recently occurred prove that certain fermented drinks, such as cyder, may accidentally contain a salt of lead, when they have been kept for a sufficient length of time in vessels lined with that metal.

Six persons residing in the Quartier du Temple, at Paris, having for several days drunk cyder purchased from a neighbouring manufacturer, experienced violent colic, obstinate constipation, considerable disturbance of the digestive functions, pains in the limbs, and shaking of the hands. Most of them likewise had a blackish covering on the gums. The intensity of the symptoms was in proportion to the quantity of drink swallowed. Energetic treatment by purgatives having been employed, the symptoms immediately amended; but the patients having lodged a complaint against the manufacturer who had sold the cyder, a judicial inquest was held. It was ascertained that the fermented liquor had remained two whole days, when manufactured, in a reservoir lined with lead, and MM. Chevallier and Olivier (d'Angers) and Pagés performed some chemical experiments with the view of ascertaining the alterations which it had undergone.

The cyder contained in bottles, presented by the complainants, having been treated by hydrosulphuric acid, the iodide of potassium gave reactions characteristic of the presence of a salt of lead.

The same cyder evaporated, carbonised, and incinerated by means of nitrate of potassa, then converted into sulphate of lead, contained, for three pints of the liquor employed, 0·2394 gram. of metallic lead.

The residue existing in the barrel which

* *Annales d'Hygiène*, No. 53.

had contained it, showed a certain quantity of metallic lead which the chemists set free.

Thus, it was incontestable that the suspected cyder contained a salt of lead in solution. It remained to determine by experiment, whether the presence of this poisonous substance should be attributed to a fraudulent manœuvre of the manufacturer, or to the action of the cyder on the reservoir lined with lead in which it had been kept. M. Chevallier ascertained that unfermented apple juice, prepared in his presence, and different kinds of cyder, showed, by the ordinary test, the presence of lead, after having remained for only three hours in a small tub lined with that metal. That chemist likewise proved, by direct experiments, that malic acid readily combined with lead, and that the malate which resulted was soluble in cold water. This result, which appeared very probable *à priori* after the examination which we have just related, is opposed to the opinion of Thomson, who says that malic acid has no action on lead; and to that of Berzelius, who advances that malate of lead is almost insoluble in cold water.

On the report of the chemists, the cyder manufacturer was convicted of having, by his imprudence, occasioned serious symptoms in six people, and was condemned in damages and expenses amounting to more than 3,000 francs.

The punishment will appear rather severe when we reflect that the manufacturer could not foresee the action of the cyder on the lead; an action, moreover, which is denied by two of the most eminent chemists; but this prosecution will have the advantage of exposing a fact of importance to the public health, and of guarding manufacturers against a dangerous process.

HYDRATE OF PEROXIDE OF IRON.*

M. Legripe gives the following formula for obtaining, with arsenical sulphate of iron, hydrate of peroxide of iron which contains no arsenic. After having dissolved the sulphate of iron in water, he passes a long continued stream of sulphuretted hydrogen into the solution; he heats it to facilitate the disengagement of the sulphuretted hydrogen; he filters, and precipitates the oxide by the ordinary means. The sulphuretted hydrogen produces the twofold effect of bringing the protoxide of iron to the state of peroxide, and of converting the arsenic into the sulphuret: sulphur and a portion of sulphuret of arsenic are deposited; the latter is completely precipitated when heated, so that the hydrated peroxide prepared with purified sulphate of iron is perfectly free from arsenic. This process has been found very successful with the arsenical sulphate of iron of commerce.

Some regard it as indispensable that the hydrate of peroxide of iron intended as a counter-poison for arsenic, should be entirely free from arsenious acid: although this has not all the importance which is attached to it, since the patient must be made to swallow the hydrate much diluted, and numerous vomitings must be provoked, still it is better to use this oxide free from arsenic. I may also mention that Berzelius recommends the cold precipitation of the peroxide of iron, by adding to the solution of sulphate of iron bicarbonate of potassa or soda; the oxide does not then retain any portion of the alkali used in its preparation.

* *Journal de Pharmacie et de Chimie*, January, 1842.

TO CORRESPONDENTS.

A.—The iodide of copper sent by our correspondent is the subiodide which is well known; that described by Mr. R. Phillips in our last No., is the protiodide, which had not previously been obtained.

A Subscriber, Norwich.—Rose's work on Analysis, price 18*s*.

D. E. F., is referred to some elementary work on Chemistry.

Analyst.—Rose's: Liebig's work on Organic Analysis, forming Part I. of Griffin's Scientific Miscellany, price 2*s*. 6*d*.

Will "An Interested Party," enable us to communicate privately with him.

Nota Bene.—*All Communications and Books for Review must be addressed, "To the Editors of the Chemist, Care of Mr. Hastings, Publisher, 13, Carey Street, Lincoln's-inn Fields, London." Communications must be prepaid, and sent before the 15th of each Month; Books for Review before the 10th.*

THE CHEMIST.

I. CHEMISTRY.

ON THE MOLECULAR CHANGES WHICH SUGAR UNDERGOES UNDER THE INFLUENCE OF WATER AND HEAT.*

BY E. SOUBEIRAN.

(*Concluded from No. XXIX.*)

The alkalis combine with cane sugar; their presence has the effect of retarding its molecular transformation. This results from the experiments which I am about to detail, and was foreseen by M. Kuhlmann, who has given, in my opinion, a very satisfactory explanation of it, by saying that sugar, like all the organic acids, acquires stability by its combination with an alkaline base. The first experiments were made in contact with the air: in the following, the syrup was covered with a layer of oil:—

FIRST EXPERIMENT.†

The operation was performed in a sand-bath, the syrup being mixed with chalk.

Primitive syrup . . .	Rotation + 71 *r.*	
Syrup after 30 hours	id.	+ 60 *r.*
——— 108 hours	id.	+ 26 *r.*

SECOND EXPERIMENT.

Operation carried on in a salt-bath, in contact with the air, with syrup mixed with chalk.

		Color of the Syrup.
Primitive syrup	Rotation + 71 *r.*	white.
Syrup after 20 hours .	id. + 54 *r.*	slightly colored.
——— 110 hours .	id. ?	black brown acid.

THIRD EXPERIMENT.

Operation performed like the preceding, but in a bath of chloride of calcium.

		Color of the Syrup.
Primitive syrup	Rotation + 71 *r.*	white.
Syrup after 24 hours .	id. + 57 *r.*	slightly.
——— 48 hours .	id. + 29 *r.*	
——— 96 hours .	id.	black brown acid.

FOURTH EXPERIMENT.

Operation performed in a bath of chloride of calcium, in contact with the air, with syrup to which $\frac{1}{20}$ of liquid saccharate of lime had been added.

		Color of the Syrup.
Primitive syrup	Rotation + 62 *r.*	
Syrup after 24 hours .	id. + 53 *r.*	
——— 72 hours .	id. ?	black.

FIFTH EXPERIMENT.

Operation in a bath of chloride of calcium, with syrup to which $\frac{1}{20}$ of crystallised carbonate of soda was added.

Primitive syrup . . .	Rotation + 70 *r.*	
Syrup after 24 hours .	id.	+ 52 *r.*
——— 72 hours .	id.	— 4 *l.*

The syrup became acid and deep colored.

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* *Journal de Pharmacie et de Chimie*, Février, 1842.

† In these experiments the letter *r.* signifies the rotation to the *right*, and the letter *l.* to the *left*.

SIXTH EXPERIMENT.

Operation carried on, in contact with the air, with a solution of sugar saturated with lime.

Primitive solution . .	Rotation	+ 20 *r.*
Solution after 24 hours	id.	+ 20 *r.*
——— 48 hours	id.	+ 19 *r.*

SEVENTH EXPERIMENT.

Operation performed out of contact with the air, in a salt-bath. In the same bath two matrasses were immersed, one containing syrup of cane sugar, the other, the same sugar to which a concentrated solution of saccharate of lime obtained by the humid way had been added.

Pure syrup . . .	Rotation	+ 71 *r.*
—— after 48 hours .	id.	—7·4 *l.*
——— 98 hours .	id.	—1·5 *l.*
Syrup mixed with saccharate . .	id.	+ 68 *r.*
—— after 48 hours	id.	+ 64·6 *r.*
——— 98 hours	id.	+ 44 *r.*

These experiments prove that the alkalis, and lime in particular, have the effect of retarding the decomposition of cane sugar, when sugar is exposed to heat and water, with or without contact with the air; but the decomposition was completely prevented only in the experiment made with sugar saturated with lime. As might be expected, when once uncrystallisable sugar is formed in considerable proportion, its decomposition proceeds much quicker, the alkali exerting on this sugar all the phenomena of prompt destruction which are common to it.

These observations agree with an experiment of M. Bouchardat, who procured crystallised cane sugar from a syrup which, after having been saturated with lime, remained for eight months sheltered from contact with the air, exposed to a temperature of 60° C.

On an observation of this kind M. Kuhlmann had based the idea of a process of manufacture, in which the juice of the beet-root was to be evaporated only after having been saturated with lime; a project which he made public only with great reserve, and which, for my part, I consider impracticable.

I may relate two series of experiments made on the sugar of honey and on uncrystallisable sugar, in which are considered the alterations of these sugars alone, and with the addition of an acid or an alkali.

EXPERIMENTS ON THE SOLID SUGAR OF HONEY, ALL THE SYRUPS BEING PLACED IN THE SAME SALT-BATH AND IN MATRASSES.

Matters Employed.	Primitive Syrup.		After 40 hours heating.		After 80 hours heating.	
	Color.	Deviation.	Color.	Deviation.	Color.	Deviation.
2 sugar, 1 water; the syrup covered with oil	White	47 *r.*	Light Yellow	47 *r.*	Yellow	46·5 *r.*
2 sugar, 1 water; without oil	id.	47 *r.*	id.	47 *r.*	id.	47 *r.*
2 sugar, 1 water, $\frac{1}{50}$ of acetic acid; oil	id.	46 *r.*	Scarcely colored	46 *r.*	Very light Yellow	46 *r.*
2 sugar, 1 water, $\frac{1}{50}$ of acetic acid; without oil . .	id.	46 *r.*	id.	46 *r.*	Yellow	45·6 *r.*
2 sugar, 1 water, $\frac{1}{50}$ of carbonate of soda; oil .	id.	47 *r.*	Dark Brown	14·7 *r.*	Very dark Brown	11 *r.*
2 sugar, 1 water, $\frac{1}{50}$ of carbonate of soda; without oil	id.	47 *r.*	Very dark Brown	14 *r.*	Almost Black	10 *r.*

This experiment confirms the stability of crystallised grape sugar, when it is submitted to the influence of water and heat; it shows that the presence of the air does not modify the result; it bears witness to the resistance of this sugar to the influence of acetic acid, and adds a proof to the observations already known concerning the easy decomposition of this sugar in contact with the alkalis. I have found that the sugar in grains, which is produced when uncrystallisable sugar is transformed, acts in exactly the same manner as sugar of honey.

EXPERIMENTS MADE ON THE UNCRYSTALLISABLE SUGAR ARISING FROM THE ACTION OF SULPHURIC ACID ON CANE SUGAR, THE SYRUP BEING PLACED IN MATRASSES, COVERED WITH OIL, AND IN THE SAME SALT-BATH.

Matters Employed.	Primitive Syrup.	After				
		24 hours.	64 hours.	100 hours.	140 hours.	188 hours.
Uncrystallisable syrup alone	20 *l.*	16·17 *l.*	14·7 *l.*	7 *l.*	2·94 *l.*	3·66 *r.*
Syrup and $\frac{1}{50}$ of acetic acid	19·5 *l.*	14·7 *l.*	11·75 *l.*	5·88 *l.*	2·90 *r.*	0
Syrup and $\frac{1}{100}$ of carbonate of soda	20 *l.*	11·7 *l.*	8·5 *l.*	2·9 *l.*	7 *r.*	9 *r.*
Syrup and chalk	20 *l.*	14·5 *l.*	8·5 *l.*	0	0	0

This experiment furnishes two important and distinctive characteristics of uncrystallisable sugar; namely—its rapid alteration under the influence of acetic acid and under that of the alkalis.

The alteration which uncrystallisable sugar undergoes under the influence of the acids distinguishes it from grape sugar in grains, as also does its power of rotation to the left.

The rapid destruction which uncrystallisable sugar undergoes by the alkalis, renders it dissimilar to cane sugar; but it presents an extreme analogy, it might almost be said, a perfect identity, with that of syrup of cane sugar rendered alkaline, when once the power of rotation has been inverted and turned to the left.

The characters which I have just noticed in uncrystallisable sugar, confirm me in the idea that it is indeed that which takes rise during the alteration of the sugar. This action may be compared with the phenomena produced under the influence of dilute acids; only, as water has but a very feeble action, heat and time are necessarily required; the conversion is so much the more prompt, as the temperature is more elevated, and as the syrup is more concentrated.*

The coloring matters do not take rise during this transformation; indeed the syrup arrives at the first zero, without the coloration being sufficiently great to put an obstacle in the way of optical observations, and it already at that period contains uncrystallisable sugar to which must be attributed the formation of the colored matters.

At the moment when the liquor is for the first time deprived of rotation, it contains undecomposed cane sugar; for, on mixing it with hydrochloric acid, it immediately takes a rotation to the left.

If the decomposition be continued, the cane sugar is completely destroyed, and the proportion of uncrystallisable sugar augments; during this period, the coloration makes a more rapid progress. When the syrup has lost all rotation to the left, and when for the second time it has fallen to zero, a great part of the uncrystallisable sugar is destroyed; cane sugar no longer exists in the liquor. Indeed, I have taken syrup which had descended to — 2·5 *l.* near the second zero, for a length of 300 millimetres; I boiled it for a few minutes with a little sulphuric acid; the rotation for the same length of 300 millimetres remained — 2·5 *l.*

But, what is the nature of the sugar deviating to the right, and which suddenly appears at the end of the operation? All that I know of it is that its formation is uniform, and that it is not interrupted by the acids; but, as it is presented only at the end of experiments, when the syrup is almost exhausted, I scarcely know it: it requires a particular examination.

APPLICATIONS.

It results from experiments detailed in the first part of this Memoir (see THE CHEMIST, No. XXIX.) that water acts on cane sugar in the manner of a weak acid, determining its conversion into uncrystallisable sugar, which, in its turn, is decomposed, giving rise to colored matters, and afterwards

* M. Boucbardat relates some experiments in which grape sugar accelerated the conversion of cane sugar. The following are two observations of the same tendency:—On the 24th of July, I put into a cellar some transformed syrup of cane sugar, of which the rotation of the first zero was — 1·5 *l.* On the 10th of October following the rotation was — 7 *l.*; the proportion of the cane sugar had then diminished. On the 31st of April, I put in the cellar some syrup brought to the first zero; on the 20th of October, this syrup had deposited sugar in grains, and its rotation had become — 12 *l.* Would not pure syrup of cane have required a long time to undergo the same transformation?

to an insoluble brown carbonaceous matter. There remains at the same time a saccharine matter, having a power of rotation to the right like cane sugar, but much weaker, and which does not take a rotation to the left by contact with the acids.

In the processes of the arts for the extraction or refining of sugar, the alterations do not go far enough to reach the first period,—that when the syrup for the first time arrives at zero.

The existence of uncrystallisable sugar in cane and beet-root sugar is at least problematical; if it does not exist, lime can contribute only to preventing its formation: if it does exist, lime, like the other alkalis, will exert the most prejudicial influence.

It is especially during the extraction of the juice, during the time which elapses before it can be sufficiently heated, that fermentation gives rise to uncrystallisable sugar. M. Guignot says* that it is necessary to hasten to put the cane in the mill, and the "*vesou*" on the fire; the more slowly these operations proceed, the more the juice is decomposed and tends to give an excess of uncrystallisable sugar; it is to this point that the attention of manufacturers should chiefly be directed; the immediate addition of a little lime to the juice, would doubtless be advantageous.

The refiners employ sugars which contain a portion of uncrystallisable sugar; their art consists in extracting from it the largest possible quantity of crystallised sugar, and in making and leaving the least possible quantity of molasses. We may here endeavour to ascertain to what the attempts at improvement should tend.

REFINING OF CANE SUGAR.

I owe the products on which I operated to the kindness of M. Benjamin Delessert. They were taken in his refinery at Passy, where the syrups are boiled *in vacuo*.

1. I observed a syrup resulting from a mixture of raw sugar from Martinico and Guadaloupe. Its density was 1260; its power of rotation, $+54\,r$. The same syrup, on leaving the apparatus in which it was being boiled in vacuo, was brought, by means of water, to its original density, 1260; its power of rotation was still $+54\,r$.

2. The green syrup† arising from the preceding operation (in manufacture it would have yielded *lumps*), had a density of 1360, and a rotatory power of $+64\,r$. After boiling, the syrup having been brought to its previous density, its power of rotation was found to be 63·8.

3. The green syrup which had run out from the lumps of the preceding operation, had a density of 1398, and a power of rotation of $+57\,r$. In manufacture it should have furnished "*vergeoises.*" Brought, after boiling *in vacuo*, to the density of 1398, its power of rotation was found equal to $+56\,r$.

4. The molasses resulting from the preceding manufacture was bleached by charcoal; its power of rotation was about $+40\,r$. It was utterly impossible to crystallise it.

Another portion of molasses, taken at another time from M. Benjamin Delessert's manufactory, likewise marked $+40\,r$. MM. Say and Dumeril sent over another also procured from cane sugar; it marked $+38{\cdot}6\,r$.

The conclusion which we are authorised in drawing from this series of experiments is, that, in a well conducted refinery, and in a regular operation, uncrystallisable sugar is not sensibly formed; that the operation, it may be said, does but concentrate in the molasses the uncrystallisable sugar quite formed; and that molasses, when sold in commerce, still contains crystallisable sugar.

REFINING OF BEET-ROOT SUGAR.

It is to MM. Say and Dumeril that I owe the means of making the experiments which I am about to detail. The syrups were prepared in their refinery at Austerlitz, where the concentration of the syrups is carried on *in vacuo*. The operation is not quite so regular as in the case of cane sugar, because in the working the syrups of different operations are mixed for boiling, either together, or with raw sugar. The general results are not less conclusive.

1. A syrup of raw beet-root sugar, had, before boiling, at the moment when it was introduced into the evaporating pan, a density of 1286, and a rotatory power of $+62\,r$. After boiling, the syrup, brought to the same density, had not changed in rotatory power.

2. A syrup was made with $\frac{4}{5}$ of raw sugar and $\frac{1}{5}$ of syrup arising from loaves of 4 *cassons*, (it was the syrup which flowed out during the second earthing (*terrage*),) and the syrup which ran out during the draining of the loaves after the *earthing*, had, before boiling, a density of 1300, and a power of rotation of $+62{\cdot}5\,r$. After boiling, the same syrup, brought to its original density, had a rotation of $+63\,r$.

3. The third observation related to a syrup made with $\frac{1}{3}$ of vergeoise, and $\frac{2}{3}$ of syrup which had flowed from the lumps, composed of green syrup and of syrup run out during the first and second *earthing* and during the draining. This syrup, at

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* *Annales de Martinique*, 1839.

† That syrup is technically called green which flows out naturally when the moulds in which the sugar is crystallised are opened.

the moment of entering into the evaporating apparatus, had a density of 1264, and a power of rotation of $+48{\cdot}28\,r$. After the boiling, the syrup brought to 1264 of density, deviated about $+48\,r$.

4. The fourth observation was made with a syrup prepared with $\frac{1}{6}$ of raw sugar and $\frac{5}{6}$ of syrup arising from 4 *cassons*, and composed of green syrup from the first earthing. The density before boiling was 1291, and the rotation $+55{\cdot}7\,r$. After boiling, the density being brought to 1291, the power of rotation was $+55\,r$.

5. The syrup employed arose from the washing of the moulds in which syrup had been crystallised. Its density was 1280, its rotation $+42{\cdot}5\,r$. Its rotation after boiling was $+42\,r$.

6. Syrup procured from bastards; it had a density of 1374, and a power of rotation equal to $+62\,r.$; after boiling, and with the same density, the power of rotation was found to be $+57{\cdot}2\,r$.

7. Another syrup run out from the bastards had, before boiling, a density of 1398, and a power of rotation $+43\,r$. After boiling it was found to be $+42\,r$.

8. One sample of molasses, the product of the refining, had a deviation of $+54\,r.$; another had $+46{\cdot}8\,r$.

Here again we find that in the refining of beet-root sugar, as in that of cane sugar, very little uncrystallisable sugar is formed.

In the above experiments, I operated on syrups which had been clarified by means of blood and refined with animal charcoal. I wished to appreciate the changes which these two operations had effected in the sugar. For this purpose I made the following experiment:—

A syrup was prepared in the sugar-house of MM. Say and Dumeril, with 7 parts of raw beet-root sugar and 1 part of syrup arising from the second *earthing* of the sugar loaves, and the draining of those loaves. This syrup had a density of 1314, and a power of rotation of $+65\,r$.

I prepared a syrup without the aid of heat, with the same matters and in the same proportions, and a quantity of water sufficient to have a product of the density of 1314. I found that this syrup had a rotation of $+66{\cdot}5\,r$. Thus, the operations of clarification and of decoloration in manufacture had not sensibly altered the syrup.

The processes of refining at present in use are therefore very satisfactory when no accident occurs in the manufacture; they give from 4 to 5 of molasses for *cassonades* of light brown sugar, and always a larger proportion for beet-root sugars. It is because the molasses of beet-root sugar, notwithstanding the care used in exhausting it, always remains richer in crystallisable sugar, which is very naturally explained by the large proportion of salt which it contains, and which forms with the sugar a very soluble and very difficultly crystallisable compound.

That which is to be considered, and which is contrary to generally received opinions, is the slight alteration which sugar undergoes during the operations of refining. While in the continued action of heat we have been able to destroy successively all the cane sugar, and all the sugar which succeeds to it, the molasses always remains very far from the first term, and contains a very large proportion of crystallisable sugar.

This cane sugar contained in the molasses does not separate from it in the state of crystals; it remains worked up with the matters which accompany it; liquid sugar, and the coloring and saline matters contribute to this result. The influence of the uncrystallisable sugar is shown in the following experiment, which proves that it does not act solely in opposing crystallisation.

I made mixtures of well boiled syrup of cane sugar, and syrup of uncrystallisable sugar resulting from the reaction of sulphuric acid, in such proportions that the syrups might have the following powers of rotation to the right, 37°, 30°, 25°, 10°, 0. These syrups having been put in closed vessels in an oven, in circumstances favorable to their crystallisation, the two first gave crystals of sugar in a few days; the third furnished appearances of crystallisation; the last two did not crystallise. Thus, a mixture of liquid sugar prevents the crystallisation of cane sugar; but the molasses no longer furnishes crystals, while direct mixtures of the same richness still yield them.

I also made the following experiment:—

I exposed some cane sugar syrup to the heat of a salt-bath, free from contact with the air: as the decomposition proceeded, I removed products having a deviation to the right of 58°, 32°, 23°, 15°, 0. I bleached them by filtering through purified animal charcoal, and I brought them to a suitable consistence; the first two syrups alone yielded crystals.

As regards the influence of salts, it is sufficiently demonstrated by the comparison which I have established between the molasses of cane sugar and that which results from the juice of beet-root; the latter, containing a larger proportion of salts, always retains a greater quantity of sugar which cannot be crystallised.

It is evident that the present system of refining is satisfactory; its future improve-

ment will be relative to one of the three following points :—

1. To find a process of freeing raw sugar from the coloring matter, from the salts, and from the uncrystallisahle sugar which it contains, so as to reduce the operation to a simple solution and crystallisation. M. Desormes has already tried, with this view, the employment of alcohol.

2. To find a process which shall facilitate the crystallisation of *vergeoises*, and especially of hastards.

3. To extract the molasses either partially or entirely from the crystallisable sugar which contains it.

What is morc important still, is to prevent the sugar from remaining in contact with acids at any period of the manufacture. This is an accident which too often occurs. Some raw sugars are of themselves slightly acid, and some dilute syrups become acescent under the influence of the animal matter introduced into it hy the clarification with albumen; the revived animal charcoal also retains some of the acid employed in its purification. In either case, the proportion of uncrystallisahle sugar is increased, and the lime which is added to the syrup may stop the action of the acid; but it does not remedy the harm already done, and, in its turn, it hastens the decomposition.

The estimation of the real value of raw sugars, that is to say, of the quantity of crystallised sugar which it is possible to manufacture from it, has attracted all the attention of sugar rcfiners; they are still reduced to dependence on external characters, which frequently deceive them. M. Biot has proposed having recourse to circular polarisation. By dissolving in water a known quantity of raw sugar, and detcrmining the power of rotation of the liquor, the raw sugar will he judged to be so much the more rich, as the deviation the more approaches to that of pure sugar; but the problem is not so simple as at first sight would appear, for the difference between the power of rotation of pure and raw sugar arises from two causes: from the presence of water, which diminishes the quantity of sugar without injuring its purification, and from the prescnce of uncrystallisahle sugar, which exerts a rotation in a contrary direction to that of cane sugar, and whose influence will he perceived in a manner fatal to the richness of the product. Now, in the weakening of the power of rotation of raw sugar, it is impossihle to assign the part of each of the two matters foreign to sugar, which must so differently influence the operation of rcfining. I have made some investigations in order to arrive at a better process; hitherto my efforts have been fruitless.

I have also endeavoured to find an easy process, permitting refiners, and especially manufacturers of syrup, to protect themselves from a fraud which is beginning to become common. I allude to the adulteration of sugar hy the sugar of fecula; it is especially carried on in regard to the sugars which form the last products of the refining; the sweet taste is weakened, although the consumer can scarcely perceive it, the disagreeable taste of the sugar of fecula heing disguised hy the richer taste of the unrefined sugar.

When it is suspected that moist sugar is adulterated with sugar of fecula, if one part of raw sugar be added to two and a half parts of alcohol at 70°, and if they be kept in contact for from twenty-four to forty-eight hours, in a cellar of the temperature of 12° C., stirring several times during the day, then if the degree of strength of the solution be taken, if that degree is nearly 10° or lower, it is very prohable that the sugar is adulterated. Cane sugar gives 30°; sugar of fecula, 48°; the raw sugars employed by the refiners, 23° to 26°. A tenth of sugar of fecula makes the solution fall helow 10°, but the test is inapplicable to colored sugars, very much charged with uncrystallisahle sugar.

With the same view I tried the reduction of the salts of copper, without any satisfactory result; however, I also tried M. Trommer's process, (*see* The Chemist, No. XXIX. page 139,) but it succeeded no better. However, I relate my experiments because they prove a new fact, and rectify a confusion which it is heneficial to stop. Each time I operated on 5 grammes of sugar, which I dissolved in 100 of water, to which I had added 2 grammes of potassa and alcohol; I added 12 drops of a saturated solution of sulphate of copper; a precipitate was formed, which was dissolved in a few seconds. I operated on the following sugars.

Sugar of fecula.
Uncrystallisable sugar.
Sugar of honey.
Cane sugar.
Cane sugar and $\frac{1}{10}$ of sugar of fecula.
Colored lump sugar.
Colored lump sugar and $\frac{1}{10}$ of sugar of fecula.
Indian sugar.
Indian sugar and $\frac{1}{10}$ of sugar of fecula.
Martinico sugar.
Martinico sugar and $\frac{1}{10}$ of sugar of fecula.
Guadaloupe sugar.
Guadaloupe sugar and $\frac{1}{10}$ of sugar of fecula.

"Vergeoise" sugar.

"Vergeoise" sugar and $\frac{1}{10}$ of sugar of fecula.

The sugar of fecula and uncrystallisable sugar became turbid in five, and the sugar of honey in nine minutes; the liquor became green and formed a deposit of red oxide of copper, which incessantly increased.

Cane sugar gave a blue liquor, which was preserved without alteration; cane sugar mixed with sugar of fecula also acquired a fine blue color. At the end of twenty-four hours a red deposit began to be formed. The lump sugar acted like the preceding mixtures; lump sugar mixed with fecula began to be turbid at the expiration of an hour.

The solution of Indian sugar was of a light blue; it began to be turbid after sixty-five minutes; Indian sugar mixed became so after a few moments; at the end of twenty-four hours the liquor remained blue.

The sugars of Martinico and Guadaloupe, pure and mixed, assumed a green tint, and both became turbid in an hour.

Pure or mixed "Vergeoise" acquired a dirty green color, and became turbid in thirteen minutes after mixture.

Thus, the sugars which contain uncrystallisable sugar act, with the exception of the intensity of the reaction, like those mixed with sugar of fecula; it is impossible, therefore, by M. Trommer's process, to ascertain their purity. To the very interesting observations of M. Trommer, it must be added that uncrystallisable sugar performs the same part in the reaction as grape sugar. It remains undecided, until established by experiment, which of these two sugars precedes the formation of alcohol in vinous fermentation.

As regards the testing of sugar, chemists who may attempt to elucidate the question, will meet with difficulties which they do not, perhaps, expect. These are owing to the circumstance that the history of the chemical properties of different sugars is not very far advanced; they will disappear as we become better acquainted with the distinctive characteristics of the different varieties of sugar.

ON NICOTINE.*

BY M. BARRAL.

I HAVE long been occupied with a general work on tobacco, which I intend to submit to the judgment of the Academy when it is quite completed. However, as a Memoir by M. Ortigosa, on one of the products of tobacco, nicotine, has recently appeared in *Liebig's Annalen*, (*see* THE CHEMIST, No. XXIX. p. 143,) I now communicate the results at which I have arrived, and which M. Pelouze has known for above two months.

Nicotine was discovered by Vauquelin, in 1809, and was studied by Porsel and Rissman, in 1828. These chemists have given some of its properties, but they have not determined its composition. M. Ortigosa has not obtained it pure, but he has analysed the double salts which it gives with the chlorides of platinum and mercury, and he found that its composition is represented by the formula $C^{20}\ H^{16}\ N^2$, which perfectly agrees with my experiments.

I obtained about 16 grammes of pure nicotine by operating on 20 kilogrammes of the dried leaves of Alsatian tobacco. I digested these leaves, previously cut in small pieces, with water acidulated with sulphuric acid; after three days I submitted them to pressure. I repeated this treatment until the leaves had lost their acid taste. The liquor was evaporated to one half, and distilled over lime. The distilled water brought over the nicotine, which I removed in part by agitating it with ether. I again treated the residue with the water of the preceding distillation until it was no longer acid.

I thus obtained a brown solution of nicotine in ether; I drove off the ether, the water, and all the foreign bodies which were more volatile than nicotine, maintaining it for 15 days at temperatures successively increased to 140° C.

I distilled the liquor thus concentrated over slaked lime, in the midst of a dry current of hydrogen in a retort placed in an oil-bath heated to about 190° C., the neck of which was bent and drawn out, and inserted in a small dry flask. The nicotine, thus preserved from the alterations which contact with the air or too intense a heat might cause, passed over slightly colored; a second distillation gave it colorless and quite pure. It did not contain ammonia, for, when treated by a solution of chlorine, it did not give the least trace of nitrogen.

It is a transparent, colorless, very fluid, and anhydrous liquid; it is altered, becoming brown and turning thick, in contact with the air; it has an acrid odor, very little resembling that of tobacco; it has a hot taste. I was not able to congeal it by exposing it to a cold of — 10° C. It volatilised at about 250° C., leaving a carbonaceous residue.

It is an extremely violent poison. A middle-sized dog died in less than three minutes after a drop of nicotine of less than 5 milligrammes had been put on its tongue. It returns the color of reddened litmus paper; it acts like a fixed alkali. Thus it

* *Comptes Rendus*, No. 5, 31 Janvier, 1842.

combines, disengaging heat, with the acids, and it precipitates from their solutions alumina and all the metals.

It directly combines with the hydracids. Its simple salts crystallise with difficulty, because they are deliquescent; the double salts which they give with different metals crystallise better. All these salts are insoluble in ether.

By burning 0·867 gr. of nicotine with oxide of copper, I obtained 0·692 of water and 2·343 of carbonic acid.

By burning 0·466 gr. of nicotine, I obtained 65·6cc of nitrogen at the temperature of 16·2° C., and at the pressure of 0·7705.

Hence I conclude that the composition of nicotine is as follows:—

	According to Experiment.		*According to Theory.*
Carbon	73·33	73·04	$750·00 = C^{20}$
Hydrogen	9·42	9·72	$99·84 = H^{16}$
Nitrogen	17·04	17·24	$177·04 = N^{2}$
	99·79	100·00	1026·88

This alkali, therefore, does not contain oxygen, and has, with relation to other organic bases, a very great capacity of saturation. This capacity has been verified by analyses of the simple hydrochlorate, and of the chloroplatinate of nicotine.

The hydrochlorate is very deliquescent in the air; but it is obtained crystallised in long fibres, and anhydrous by forming it with dry hydrochloric acid, and passing it under the receiver of the pneumatic machine. It is white, more volatile than nicotine, insoluble in ether, very soluble in water and alcohol. By treating 0·733 gr. by the nitrate of silver, I obtained 0·860 gr. of chloride of silver, whence it would appear to have the following composition:—

	According to Experiment.		*According to Theory.*
Hydrochloric acid . .	29·74	455·12	$455·12 = Ch^{2}\,H^{2}$
Nicotine	70·26	1075·21	$1026·88 = C^{20}\,H^{16}\,N^{2}$
	100·00	1530·33	1482·00

Chloroplatinate of nicotine is obtained in a seedy yellow powder by precipitating the solution of chloride of platinum by nicotine. It is soluble in boiling water, and very soluble in a slight excess of nicotine.

By calcining 2·763 gr. of this salt, I obtained 0·961 gr. of spongy platinum, which gives 34·25 of platinum per cent.

By burning 1·100 gr. of this salt with oxide of copper, I obtained 0·293 of carbonic acid, and 0·313 of water.

By burning 1·550 gr. of the same salt, I obtained 59cc of nitrogen at the temperature of 15° C., and at the pressure of 0·765.

From these three analyses, it results that chloroplatinate of nicotine has the following composition:—

Carbon .	21·12	C^{20}	$Pt\,Ch^{4} = 2118·40$
Hydrogen	3·22	H^{18}	$Ch^{2}\,H^{2}$. 455·12
Nitrogen .	4·81	N^{2}	$C^{20}\,H^{16}\,N^{2}$ 1026·63
Chlorine .	36·60	Ch^{6}	
Platinum .	34·25	Pt	3600·15
	100·00		

The double chlorides of nicotine with the chlorides of mercury, tin and iron are obtained in the same way. The double salt of mercury and nicotine is white, as is that of tin; that of iron is brownish yellow.

All the preceding reactions show that the nicotine which I obtained was a well defined alkali. I have not yet been able to analyse the salts which it gives with the oxacids. I have yet to show what kind of alteration it undergoes in contact with the air, and at a sufficiently high temperature. In the Memoir which I shall submit to the Academy, I hope to add to the solutions of these questions, an explanation of the formation of nicotine in tobacco.

ON THE COMPOSITION OF THE AIR.*

BY M. DUMAS.

The Academy will learn with interest the first results of the great investigation relative to the analysis of atmospheric air, to which, at its request, several chemists have zealously devoted themselves.

Different stations having been chosen, certain dates and meteorological conditions for executing the experiments were agreed on. We publish the results as they have come to our knowledge.

I. STATION OF GENEVA.

M. De Marignac, formerly pupil at the Polytechnic School, now Professor of Chemistry at Geneva, after having made himself accurately acquainted with the means

* Communicated by M. Dumas to the Academy of Sciences, Paris, at its sitting of March 14, 1842. *Comptes Rendus*, No. 11.

used at Paris, executed at Geneva analyses quite comparable to those made at Paris, for he made use of apparatus of the same nature as ours, and for all the observations of temperature while weighing, he employed M. Danger's thermometers, indicating the $\frac{1}{100}$ of a degree.

By employing the processes used at Paris, he obtained the following results:—

For 10000 *of Air by Weight.*

	Oxygen by weight.
January 11, 1842 . .	2301
,, 18, ,, . .	2300
February 3, ,, . .	2297
Mean . .	2299·3

The mean of Geneva therefore is exactly the same as that of Paris.

II. STATION OF COPENHAGEN.

M. Levy, a young Danish chemist, after having spent some time at Paris in my laboratory, returned to Copenhagen to make analyses of the air corresponding to ours, and he has transmitted to us some very remarkable results, in letters from which we give extracts.

By the care of M. Œrsted, who was kind enough to take great pains to ensure the success of M. Lévy's operations, the latter found, on arriving at Copenhagen, very delicate balances, weights, the necessary meteorological instruments, the pneumatic machine, and the laboratory of the Polytechnic School, all arranged for his experiments; M. Lévy took from Paris the gauged flasks and the very sensible thermometers which he required.

M. Lévy was furnished at Paris with empty flasks analogous to those which we sent to the Faulhorn. He was to fill them at sea, at the greatest possible distance from the coasts, and to collect the air as near as he could to the level of the sea.

For some time we were fearful that M. Lévy's mission, notwithstanding all his care, would prove entirely fruitless. His first experiments had discouraged him, when he conceived the idea of submitting all the apparatus to a very strict verification. He found that two sets of the weights placed at his disposal, comprising the kilogramme and its divisions, presented discordances which might amount to several centigrammes. Tables of correction were prepared with great care, when the weights could be employed, and the experiments became regular.

M. Lévy very soon acquainted us with the first numbers resulting from his experiments, and he informed us that there existed an essential difference between the air taken on land and that taken at sea.

M. Lévy's investigations having thus acquired a high degree of interest, he required some further facilities, and, to obtain them, it was sufficient to present to the King of Denmark the letter in which the Academy directed him to make the investigations with a view to the advancement of science. That enlightened Prince, who cultivates science himself with great interest, wished to assist in the analyses, and immediately gave M. Lévy proofs of his desire to oblige the Academy of Sciences of Paris, by aiding the investigations with which it is occupied.

M. Lévy executed several analyses, the details of which he transmitted to us, and which will be published in a Memoir which he intends to prepare on his return to Paris. We will here content ourselves with giving the results.

I. ANALYSES OF THE AIR TAKEN AT COPENHAGEN, IN THE COURT OF THE POLYTECHNIC SCHOOL.

1841.		*For* 10000 *of Air by Weight.*
Nov. 17, 4 P.M.	Snow	2300 oxygen.
,, 30, Noon.	Sky overcast	2302
Dec. 12, 10½ A.M.	Clear sky, fine weather . . .	2296
,, 15, ½ P.M.	id.	2299
,, 22, 11½ A.M.	Snow	2301

II. ANALYSES OF THE AIR TAKEN AT SEA.

1841.		*For* 10000 *of Air by Weight.*
Aug. 4, 8½ A.M.	57° 46 N. lat. and 8° 22 long. E. of Paris .	2257 oxygen.
,, 3, 10¾ A.M.	55° 30 N. lat. and 5° 30 long. E. of Paris .	2258
,, 2, 1 P.M.	52° 36 N. lat. and 0° 58 long. E. of Paris .	2259
,, 3, 1¼ A.M.	54° 15 N. lat. and 2° 7 long. E. of Paris .	2256

III. ANALYSES OF THE AIR TAKEN ON THE COAST, BY THE SEA BREEZE AT THIRTY-FIVE FEET ABOVE THE LEVEL OF THE SEA, AT THE CASTLE OF KRONBORG, TWELVE LEAGUES FROM COPENHAGEN.

1842.		*For* 10000 *of Air by Weight.*
Feb. 18.	Sky cloudy; wind, NE.	2302 oxygen.
,, ,,	id.	2301
,, ,,	id.	2302

Thus the air taken at Copenhagen was the same as at Paris; the air of the sea was less oxygenous, and the difference so considerable and so uniform that there is no reason to suspect the slightest error. Indeed this difference of composition appears to be limited to a stratum of air near the surface of the sea, since, by taking air on the coast, from a sea breeze, at 35 feet above the level of the sea, the same composition as on land was obtained.

The following are the means of the above three experiments:—

Mean of the air at Copenhagen . 2298·8*
Mean of the air taken at sea . . 2257·5
Mean of the air taken on the coast, 2301·6

It is to be hoped that these first results will very soon be controlled by the analysis of the air, in some long voyage, at great distances from land.

ON ATMOSPHERIC ELECTRIC DISCHARGES.

BY REUBEN PHILLIPS.

Almost as early as any of the properties of electricity were discovered, the identity of this subtile agent with the cause of the thunder-storm was suspected; but Franklin first proved lightning to be an electric discharge. Although the cause has been so long known, yet are there still many points in this grand phenomenon which want explanation: for example, whence is it that the cloud possesses electricity of such high tension? The processes of condensation, expansion, &c., and the friction occasioned by the aërial currents, doubtless may occasion a stratum of air to become slightly electric; but these causes are utterly incapable of producing such powerful effects, unless by some process the electricity so produced be concentrated. The object of the present paper is to point out a very probable method by which this concentration may be effected.

It is commonly supposed that when gaseous matter is in a sensibly electric condition, every particle of that matter is surrounded by its own electric atmosphere. But such a state of things can have no existence, because bodies similarly electrified are mutually repulsive, therefore there ought to be an amazing expansion attendant on the development of an electric state in gaseous matter: hence the inference becomes that the electric atmosphere resides only on the surface of that volume of the gas in an electric condition; then any decrease of that surface would be productive of an increase of electric intensity; therefore a layer of air possessing electricity of very low tension, by becoming possessed of a figure which exposes less surface, may acquire a high degree of electrical excitement, which may be further increased by the cloud tapering to a point.

This in some measure shows how it is that the thunder-cloud has condensed its electricity. A stratum of air becomes slightly electric from any of the causes before mentioned; the forces of similarly electrified bodies come into play; the cloud has a tendency to form a sphere, which tendency may be augmented or decreased by winds, and by the position of the oppositely electrified mass; and by approaching nearer the form of a sphere, the surface becomes diminished, and the electric intensity proportionally increased.

This tendency to expand on the part of the electrified cloud must cause it and its contained vapor to be slightly rarified, whereby it is capable of taking up more moisture though saturated previous to electrical condensation. If, therefore, rain fell through a cloud thus conditioned, the rapid evaporation caused by the diminished pressure, aided by the motion of the aqueous matter, and the low temperature consequent on the elevation, may be quite sufficient to account for the production of hail, which frequently accompanies thunder.

* 2299·6?—Eds. Chem.

ON THE INSTINCT AND IRRITABILITY OF PLANTS.

BY JOHN S. HILEY, A.B., M.B.

(*Continued from No. XXVIII.*)

In treating of the instinct and irritability of flowers, I shall pursue the course which I adopted whilst discussing the same properties of leaves. In this way, the influence of light, the sun, touch, &c. will come before me, and I hope to adduce some of the most singular phenomena, in support of my position, that can attract the attention of the naturalist. The fact that the expansion of flowers is promoted by the light of the sun, together with variations in temperature, will account for the different seasons during which plants unclose their blossoms. Nothing surely can so fully command the admiration of the naturalist, or induce him more willingly to coincide with that beautiful exclamation of our Saviour, "Consider the lilies of the field how they grow; they toil not, neither do they spin; and yet I say unto you, that even Solomon in all his glory was not arrayed like one of these." Flowers are amongst the most beautiful ornaments

of nature, and consequently, if they all appeared at the same season, they would vanish too soon, and the world would be deprived for a long period of some of its greatest attractions.

In considering the instinct, &c. of flowers, it may be well to remark that they are usually composed of stamens, pistils, seed vessel, and floral integuments, but that the essence of the flower resides in the three first; the calyx and corolla being considered as only accessory. Hence the latter are frequently wanting, without their absence appearing to have any modifying effects on the phenomena and mutual action of the sexual organs. Notwithstanding this, it will be discovered in the more advanced stages of this Memoir, that not only are the stamens, &c. sometimes gifted with a strange degree of instinct and irritability, but likewise the calyx and corolla, in certain instances where they exist, possess similar properties. In fact, it is in these coverings where the motions, about to be treated of, are most generally observed. Here, too, the influence of light and shade is remarkable, particularly where the organs in question have much color.

That gentle and moderate heat also favors the expansion of flowers cannot be denied, though at the same time there are many which throw out their blossoms during the severities of winter. The Snow Drops,—Snow Flakes,—the Christmas Rose,—the Daphne, produce flowers while the earth is still covered with snow; but this is not so much the result of instinct or irritability as the consequence of the approach of that season during which every living thing is quickened and revived. In those regions of the globe, where the temperature continues moderate from one end of the year to the other, there appears to be a perpetual spring, whilst in localities subjected to extremes of heat and cold, it is only during the prevalence of warm weather that flowers unfold their beauties to the world. Again. In all northern and extreme southern latitudes, where the spring and summer are, in consequence of their situation as respects the sun, necessarily very transient, the rapidity with which vegetation springs up and flowers, has always been a matter of astonishment. In Sweden, Lapland, and Russia, summer bursts suddenly from winter; the valleys which were covered with snow are green in a few days. This lasts about three months, during which they sow, plant, and reap. In such regions it is but natural to suppose that plants are of the most hardy kind, and so constituted as to endure with perfect safety the rigors to which they are exposed.

There is another instance of the beautiful adaptation of plants to seasons and places, which it may not be out of our course to mention. I allude to the formation and protection of buds. Since these contain the rudiments of a plant or part of a plant, for a short time in a latent state, here of course the vital principle is dormant, and its excitability accumulated. For such reasons it was necessary that all buds liable to be injured by cold and wet should be shielded from their action. Hence they are often furnished with an additional external guard of gum, resin, or woolliness, and accordingly where gum, resin, or gluten coat the bud, we may infer, as in the case of the horse-chestnut, that such protection will be most effectual against moisture, whereas when down or wool constitutes the covering, it must serve to secure the innermost and more delicate parts from the inclemencies of the weather.

Nor must I forget to consider those appendages of plants denominated Phyllodium, Stipules, Tendrils, Spines, &c., though doubtless these ought to have been studied in the preceding article, when treating of the instinct and irritability of leaves. I would omit them altogether, were not the remarks I have to offer very important, as bearing upon those powers which vegetables possess of adapting certain parts of their system to peculiar circumstances, and thus providing against many exigencies of both climate and temperature. In certain plants, as the Acacias of New Holland, where the leaf is not developed, the petioles themselves are laterally compressed so as to assume the appearance of the limb of the leaf, and to fulfil its purposes. In such cases, the expanded petiole is termed a phyllodium, but unlike a leaf, it affects a vertical in place of a horizontal position. In the Sagittaria sagittifolia, a monocotyledonous aquatic, the limb is only expanded at the summit of those leaves which reach above the surface of the water. The phyllodium, however, is not the only substitute provided by nature for supplying the absence of a perfect leaf. In cases where the latter is either entirely abortive, or becomes a dry scale, then the branches themselves expand, and assume the appearance and properties of leaves. Such often occurs in the Ruscus aculeatus, or Butcher's Broom, and others of this genus.

The stipules met with at the origin of the leaf upon the stem, and peculiar to dicotyledonous products, would seem also in certain instances to fulfil the office of leaves. In the Galium, Rubia, and Asperula of our own climate, this appears to be especially the case, whilst amongst exotics we meet

with still more numerous examples. Their peculiar use consists in defending the leaves before their development, as is clearly shown by their relative situation in the buds of the Amentaceæ, of the Rosaceæ, &c.

The tendril, a sort of filamentous cord, and originating either in some modification of the leaf or petiole, as in the Lathyrus, Vicia, &c.; or from a transformation of the branches or peduncles, as in the Vine; or, lastly, from some altered condition of the stipules, as in the Cucumber, is an appendage which serves to support the stems of feeble and climbing vegetables, by attaching them to more firm and sturdy ones. In tropical regions, such climbers, by the aid of their tendrils, often reach to the summits of lofty trees, which they crown with adventitious blossoms.

In the Vine, Passion-flower, Pea, and Vetch tribe, spiral tendrils are well exemplified; which plants, but for the aid afforded them by these supports, would constantly trail and creep along the ground. The most remarkable tendril is, however, found in the Annona hexapetala, as described in the Linnæan Supplement. The flower-stalk of this tree forms a hook, and grasps the neighbouring branch, serving to suspend the fruit, which is very heavy, and resembling a bunch of grapes.

As the appellation tendril is given to certain modifications of the petiole, floral peduncle, or stipule, so that of claw is ascribed to the roots of those sarmentaceous and climbing plants which sink into the bodies that support them. The Ivy and the Bignonia radicans afford examples; and the fine filaments noticed on the claws are termed suckers, and not only serve to attach them to suitable supports, but appear destined to absorb the nutritious fluids contained in the bodies in which they are implanted.

The hair of plants, though often, as in the Nettle, forming excretory ducts, is repeatedly a security against cold, insects, &c. Such is the case in the Verbascum or Mullein, &c., whilst bristly hairs may sustain a slender plant, as in the Galium Aparine. The mechanism of the sting of the Nettle has attracted the attention of most botanists, in consequence of its simplicity and singularity. I need not, however, describe it here, since it is well explained in most treatises on botany.

With these general remarks it now devolves upon me to speak particularly of the instinct and irritability of flowers; in doing which, all the parts usually composing this important structure will, if furnishing any facts illustrative of the subject, be treated of in detail. It may be well to observe that by the flower, I mean the Calyx and Corolla, the Stamens, Pistils, and Seed-vessel, or some modification of these divisions, by which seeds are brought to perfection, and new plants generated.

The reproductive organs, which are peculiarly the stamens and pistils, are most delicate in their structure, and hence the principal use of the beautifully colored floral integuments, the calyx and corolla, would seem to consist in defending the organs of reproduction, until fully perfected. Different plants shed their blossoms in various seasons of the year, and hence we have Vernal, Æstival, Autumnal, and Hibernal flowers; and from a consideration of the period at which plants may flower, Linnæus has formed his Calendar of Flora. In fact, there are a great number of vegetables whose flowers always appear regularly at the same time. Thus, in the climate of the French capital the Black Hellebore flowers in January;—the Hazel, the Daphne Mezereon in February;—the Almond, Peach, Apricot in March;—the Pea, Tulips, Hyacinths in April;—the Lilacs and Apples in May, &c. From this certainty as to the months in which many plants flower, it was formerly the practice to calculate particular family or national events, and poets as well as historians have often followed this antiquated method of calculating. The land too was subjected to particular operations during certain seasons, which seasons, in savage states of society, were ascertained by the unfolding of various flowers. Some such method is occasionally alluded to in Scripture. In addition, Linnæus likewise noticed that many species open their flowers only at certain hours. Of these, some are diurnal and others nocturnal. It will be seen, however, that his ideas were rather fanciful than otherwise, for the changes did not so much depend upon the approach of any particular hour, whether of night or day, as upon the influence of light and shade, which I shall enlarge upon immediately.

ON SOME INCONVENIENCES ATTENDING THE USE OF THE SULPHURIC ACID OF SAXONY OR NORDHAUSEN AS A TEST.*

BY A. DUPASQUIER.

Sulphuric acid poured into a liquid in which it is wished to detect the slightest traces of an iodide, after having previously added to it a small quantity of the solution of starch, is, to obtain this result, the most sensible means that can be used: this, at

* *Journal de Pharmacie et de Chimie*, No. 3, March, 1842.

least, is the result of comparative experiments which I have made, and which have demonstrated that sulphuric acid was preferable for liberating iodine, to chlorine or chlorides of oxides, to nitro-muriatic acid, to the voltaïc pile, &c.

Since making those experiments, accident taught me that it was not a matter of indifference, in detecting traces of an iodide, as to what quality of acid was employed. In one of my last lectures at the *Ecole de Médecine*, I wished to show the sensibility of the reagent, and I was much astonished at not obtaining the violet blue coloration that I expected. At the moment when I poured the sulphuric acid into the liquid, I thought that I perceived a very faint violet tint, but it immediately disappeared.

In investigating the cause of this unexpected result, I learnt that the assistant, having no more ordinary sulphuric acid, had given me the bottle of sulphuric acid of Nordhausen. From this explanation, I at once concluded that the failure in coloring the starch of a violet hlue, should be attributed to the sulphurous acid which sulphuric acid generally contains when it has been obtained by the distillation of sulphate of iron. We know, indeed, that free iodine, in contact with a solution of sulphurous acid or of a sulphite, immediately disappears, passing to the state of hydriodic acid. The following experiments have also proved the correctness of this mode of explanation.

1. I poured common sulphuric acid into a glass of water, containing one drop of a solution of iodide of potassium, and a little solution of starch: the liquid immediately acquired a violet blue tint.

2. The same experiment varied by adding, with the sulphuric acid, a few drops of a solution of sulphurous acid; the coloration was not manifested, even by adding a great excess of sulphuric acid.

3. The same experiment repeated, but the sulphurous acid being replaced hy a few drops of a solution of sulphite of soda, gave the same result: the liquor remained colorless.

4. The same experiment with a very large quantity of iodide of potassium: the same result; only, at the moment when I added the sulphuric acid, I thought that I perceived a faint violet tint, but it immediately disappeared.

From these experiments, it cannot but be concluded that the existence of sulphurous acid in the sulphuric acid of Nordhausen is the true cause of failure, when that acid is employed to detect the presence of an iodide in a liquid.

This influence of sulphurous acid on iodine gives a satisfactory explanation of the following fact, which is in some measure analogous to that which I have just related.

In my chemical experiments made five or six years ago, on the employment of the protiodide of iron, I took care to order that the urine passed by the patients should be collected, in order that I might ascertain, by chemical testing, whether it really contained that medicine. This testing consisted in diluting the urine with an equal bulk of water, then in adding to it some solution of starch, and finally, sulphuric acid. The liquid immediately acquired a blue tint, when the patient had swallowed the iodide a short time before passing water. The addition of water was indispensable, for the coloration was not manifested, or was scarcely perceptible, when the urine was very strong, or when it was not rendered more clear or less abundant in organic matter, by means of that liquid. Is it not evident that the non-coloration of starch, in the case of strong urine, is owing to the formation of a small quantity of sulphurous acid, by the reaction of the concentrated sulphuric acid on the animal matter? The addition of water had the effect of diminishing the reaction of the acid on the organic substances contained in the urine.*

The presence of sulphurous acid in the sulphuric acid of Nordhausen should likewise cause it to be rejected, in acting with Marsh's apparatus, since MM. Fordos and Gélis have lately shown that, in this case, sulphurous acid was converted into hydrosulphuric acid, which precipitated the arsenic in the state of sulphuret, and might diminish, if not altogether stop, the production of the arsenical stains. It is known, however, that the sulphuric acid of Nordhausen has been recommended in preference for Marsh's apparatus, on account of its never containing arsenic.

In conclusion: the sulphuric acid of Nordhausen or of Saxony, employed as a reagent,

* I had long since ascertained that the protiodide of iron is decomposed in passing through the circulation, and that the iodine arrives in the urine only in the state of an alkaline iodide, when M. Gélis published his Memoir to show that iron does not pass into the liquid separated from the blood by the kidneys; long prior to his experiments, mine had likewise completely confirmed them: I had always found iodine in the urine when I had administered the protiodide; never, on the contrary, could the reagents detect the presence of iron. It is therefore an important fact of chemistry and of medical practice, which I have frequently demonstrated, for five or six years, to my pupils at the Hôtel-Dieu of Lyon.

instead of common sulphuric acid, may become, in many chemical investigations, a cause of very grave errors. This inconvenience arises from its generally containing sulphurous acid.

ON THE COMBINATIONS OF SULPHUR WITH CHLORINE.*

BY R. F. MARCHAND.

M. MARCHAND undertook, on the occasion of the study of the reaction of sulphuret of chlorine on alcohol, ether and acetic spirit, some investigations concerning the composition of chloride of sulphur; the following are the results:—

It is very easy to obtain the lowest degree of combination with chlorine, by driving chlorine into an excess of sulphur, and keeping cold the vessels in which the reaction is carried on. Whilst all the sulphur is not yet dissolved, the liquor is decanted and submitted to distillation. It ordinarily acquires a dark color, and there passes into the receiver a liquid likewise of a dark amber color.

Distillation does not decompose it: it is therefore distilled several times, until its boiling point becomes constant. M. Marchand found it to be 139° C.; its specific gravity is = 1·686.

The composition of the chloride of sulphur is that which was indicated by MM. Rose and Dumas, and previously by Bucholz, namely: S Cl. The specific gravity of the vapor has been found to be = 4·77; calculation gives 4·65.

If a greater quantity of chlorine be driven into this combination, the yellow color disappears, as is known, and is changed to red, whilst the volume of the liquor increases in a surprising manner. It is only after having continued the current of gas for a very long time that absorption ceases; but the sides of the vessel and the gas tube are then covered with a solid substance. The supernatant liquor allows chlorine to be disengaged without interruption, and for that reason possesses a very considerable tension of vapors. The light of the sun augments it in an extraordinary degree, and strong glass vessels, in which the liquor is contained, have been violently broken, through having been imprudently exposed to the direct rays of the sun. The specific gravity is, according to M. Marchand, 1·625; and 1·62, according to M. Dumas. Its composition answers to the formula $S Cl^2$: the specific gravity of the vapor has been found to be 3·86, while theory requires 3·549.

If the combination be distilled, it is decomposed without interruption, and it is impossible, or very difficult, to volatilise it without alteration, even in a very rapid current of chlorine. The boiling point of the liquor is from the commencement about 50° C.; at first, scarcely any chlorine is disengaged, then the boiling point rises beyond 64° C. This degree may be attained by driving in, during the distillation, a strong stream of chlorine.

The liquor which first passed over without the current of chlorine, contained 33·66 of sulphur per cent.

After a great portion had been collected in the receiver, it was returned into the retort and distilled, until no more chlorine was disengaged.

The boiling point was constant at 78° C., and the proportion of sulphur was ascertained to be from 37·58 to 37·78 per cent., which answers to the formula $S^2 Cl^3$; this sulphuret may be regarded as a combination of the first degree of sulphurisation with the second, $S Cl + S Cl^2$.

If a continuous stream of chlorine be driven through chloride of sulphur, there is deposited, at a low temperature, a solid crystallised combination, already mentioned by M. Millon. It fumes strongly in the air; when thrown into water, it causes a hissing noise like red-hot iron, and forms hydrochloric, sulphuric, byposulphurous and sulphurous acids, allowing sulphur to deposit. Analysis gave 30·93 of sulphur per cent.; it is therefore the same chloride of sulphur as the liquid combination, and it is probable that the latter would deposit crystals at a low temperature.

There are, therefore, five combinations of sulphur with chlorine, three of which may be isolated: 1. $S Cl$ or $S^2 Cl^2$; 2. $S Cl^2$ or $S^2 Cl^4$; 3. $S^2 Cl^3$ or $S^2 Cl^2 + S^2 Cl^4$; 4. $S Cl^4$ in combination with metallic chlorides; 5. $S Cl^6 + 5S O^3$.

* *Annalen der Chemie und Pharmacie.*

LONDON ELECTRICAL SOCIETY.

TUESDAY, MAY 17th.

A NOTE from Mr. Weekes was read, in which it was stated that apparatus was prepared precisely similar to that in which the Acari had been developed, but without passing through it a voltaïc current; and that the most careful examination could not detect the appearance of insect life. A long and carefully written paper on lightning conductors, by the Secretary, was then read, in which he entered at some length into the intricate question of the "lateral discharge," and showed with some show of reason, that where provisions for this are not made, a lightning-rod is an

enemy, and not a friend. Extracts from several letters to the Secretary on "Electrotype Manipulation" were then read. There is some trouble in causing solder to adhere to the back of electrotypes; this is obviated by warming the subject of experiment, and applying to it a small piece of steärine; the solder after this will run readily. A better compound for moulds is obtained by introducing black lead among the wax, steärine, &c. The Rev. Mr. Lockey employs steärine, wax, and black lead, and produces with them moulds of a character almost equal to Mr. Clichée. From the same source a plan was suggested, which in practice had been found successful, of pouring wax about the edge of a plaster cast, when prepared as a mould, and by this means providing an *extra* rim. A copper electrotype was submitted to the Society by Mr. Clarke, having the relief in silver. He has prepared some thus, with parts of the surface bronze, silver, and gold. The next notice was from Mr. Mayo, who uses wax and flake white for moulds. Those submitted to this Society surpassed any others we had before seen, and might be estimated as the acme of mould-making. Mr. Weekes's Register was read as usual.

II. CHEMICAL MANUFACTURES.

ABUSES OF THE ALKALI TRADE.

In the course of our duties to the public it has been our misfortune to have to record and expose many gross frauds and malpractices with regard to most of the great commodities of life, as well as of such as constitute the chief sources of our commerce; thus it will be within the memory of our readers, that we have given minute accounts of the adulterations of bread, beer, spirits, medicines, and various other articles. It has recently devolved on us to notice one of a character equally culpable, as far as the objects of those who are engaged in the trade are concerned, although aimed only at the purse and not at the health. For our observations on, and our exposition of, the modes in which these frauds are effected, we have received the highest praise and thanks from the press, as well as from those who are too honourable to practise them, and too firm-minded to regard the censure or obloquy of those who do. Our late remarks upon the abuses existing in the alkali trade, and particularly among the brokers, have been productive of the greatest benefit, except to those who have so long and vilely profited by those abuses. Six per cent. is certainly no trifle to lose, even though gained by malpractice; but the affair is now at an end: the salutary and never failing cure which publicity produces, has been amply manifested in this case, and far more sweeping at all times are these than legal punishment, although both are equally and well merited. Ignorance cannot be pleaded as the sole excuse, although it abounds in an ample degree; yet it is so convenient, and works so well for them, and all one way, that it is most fortunate and well contrived—such ignorance, indeed, is bliss. We did not expect our statements to have escaped the ire of those who are interested in the continuance of these practices, and who are baffled by their exposure and consequent termination; but we had no idea,—and if we had we should not have heeded it,—that we should draw down upon our heads the wrath of an inconsiderable firm, so much as to cause them to "try their hands" at "circular letter"* writing, and to issue to the trade a specimen of composition, having in it but one sentence devoid of numberless instances of ignorance and bad grammar, and even that is a sad bungle. The document is just worthy of the place of its emanation—the back slums, or common cloaca of Lancashire—St. Helen's,—and some of the observations and expressions are such, that for meanness, low grovelling attempts at sarcasm and wit, we are confident that in the regions of Billingsgate, Saffron Hill, or the purlieus of St. Giles, there is not such another mouthful of dirt. It is quite unusual to find the "Friends," the sleeky, eel-like Quakers, involving themselves in such matters; but we touched a tender point,—the purse, and "Joseph and his brethren" felt it.

Joseph saith that we stated that we found one per cent. of lime in carbonate of soda, and for this he and his brethren would order

* A circular addressed *To the Editors of* The Chemist, by Messrs. Gamble and Crosfields, of the St. Helen's Chemical Works. These persons had the impertinence to write on the one sent to us "For insertion in The Chemist;" we beg to inform them that we do not insert any thing in this work which is incorrect in grammar or chemistry, or which contains any untrue statements.

"a boy in the junior class of chemistry a flogging;" now, thou must be a very simple person to think we are ignorant of the incompatibility of lime with a carbonated alkali. Thou knowest full well that we were quite aware of it, therefore, it is only that thou choosest to be impertinent. Thou it is who deservest to be flogged; but it will answer all our purposes, if, instead thereof, thou "writest thyself down an ass." We thought that the anti-slavery tribe were enemies to the lash, but we find thee, Joseph, "preachee and floggee too." What will Sambo say, Massa broad-brim?

Thy pity, Joseph, is estimated by us at as small a value as is thy opinion of our Journal, which has been honored by the highest testimonials, as high as, if not even higher than, any modern Journal of Science, from all parts of Europe. Measure then our affliction at thy disapprobation. We set equal value on thy censure and thy praise.

It is but too easy for us to convince you that you have been worse than stupid in publishing this circular. You have thereby furnished us with fresh matter for comment, and to the fullest extent shall we at all times avail ourselves of it in protection of the soap trade, whenever any malpractices come to our knowledge.

We will now proceed to notice the portions of this circular, which more particucularly bear on the question at issue.

In refutation of our statements,—that the alkali brokers are ignorant, and unfair in their dealings,—Messrs. Gamble and Crosfields say;—"First as to their ignorance; the art of ascertaining the per centage of alkali in a given sample, is not so great a mystery, even to the uninitiated in chemistry. There are two methods in common use. Doctor Farrady's [Dr. Faraday?], wherein sulphuric acid of a certain specific gravity is taken as a standard, and that wherein crystallised carbonate of soda is made the standard." With regard to the simplicity of the operation, we need only observe, that if it be so very easy, it is most astonishing that so many *mistakes* occur: of course, they now leave us no alternative but to attribute it to their cupidity. "What impurities it may contain is altogether immaterial to the soap-maker," (in these few words, so classically strung together, which most predominates, truth or elegance?) "the quantity of alkali he obtains for his money, being all that it is material for him to know." Does not the sulphite of soda suffer decomposition by the test acid?

The next paragraph, that undertaking to prove the honesty of the alkali brokers, is, as might be expected, a complete and signal failure: a task so difficult of accomplishment, was certain to prove abortive in such hands.

Leaving this puerile attempt at a refutation of our accusations, we now proceed to examine their attempts at proving us at fault, in a chemical point of view. They contradict our assertion that crystals of soda may contain quantities of water, varying from 1 to 10 equivalents, according to the temperature at which the crystallisation was effected; * they do not admit that carbonate of soda can be crystallised with less than 10 equivalents of water. They proceed—"Again, you say it is stated that the per centage of soda in crystallised carbonate, ranges from 21·5 to 22·2, and you assert, that from extensive experience, you have found it between the two, say 21·8. Do you not see that these assertions contained in the same paragraph contradict each other; [a note of interrogation would have been more in place here than a semicolon;] if crystals of soda contain from *ten to one per cent.* of water, your extensive experience should have taught you, that the same crystals should have contained from 21·5 to 50·3 per cent. of soda." We never stated that these crystals contain from 1 to 10 *per cent.* of water: our assertion was, that they contain, according to the temperature at which they are produced, (see last note,) from 1 to 10 *equivalents:* our readers will understand their object in making this mis-statement. If the crystals of soda contained from 1 to 10 *per cent.* of water, they would contain from about 52·85 to 58·15 per cent. of real soda, and not, from 21·5 to 50·3. Truly, their arithmetic is as bad as their grammar and chemistry.

Joseph and his brethren again go on to assert that soda ash contains only a trace of oxide of iron, and not 1 or 2 per cent. as we have stated. In answer to this we will merely say that the quantity of that compound which we declared it to contain was the result of many analyses made by ourselves and others, and we repeat, that it does contain that quantity. We may here remark that we have detected large quantities of arsenic in soda ash, prepared from iron pyrites. (See the following article.)

Again, "White ash generally contains a much larger quantity of sulphite than you

* Our reason for making this statement was, that we are aware that some persons are in the habit of recrystallising the carbonate of soda previous to using it as a standard, and it is intended as a caution to them: we did not for a moment fear that the manufacturer would send it out without its *full quantum* of water.

have recognised,"—and—"When the solution under trial is filtered or decanted, as in every case it ought to be, the impurities can nowise affect the acid or disturb the test." Do they mean to say that sulphite of soda is to be got rid of by filtration or decantation? Will not this salt "affect the acid or disturb the test?" We repeat our former statement, that sulphite of soda is one of the impurities for which allowance ought to be made to the purchaser; it is worse than useless to the soap-maker, as he pays for it without deriving the slightest advantage from it.

In the same paragraph we are falsely represented to have said that we had found *one per cent.* of lime in carbonate of soda, whereas we merely asserted that it contained "a portion of lime," without stating the quantity. We omitted to mention that this lime was in combination with carbonic acid, and we thought that our meaning would be sufficiently clear without doing so.

The object which Messrs. Gamble and Crosfields had in view in sending forth to the world this pitiful production, evidently was to curry favor with the houses named at the commencement of their circular.*

The writers of this trashy production have, in their impotent rage, dared to assail us with personalities; they have vented their filthy spleen on us because we have, on public grounds, and on no other, laid the axe to the root of a most diabolical system of fraud. We will not descend to their level.

The most extraordinary part of this circular is, perhaps, the concluding words—"We cannot give you a better parting advice than,

'GO THOU AND DO LIKEWISE;'"

thus prostituting a passage in the Scriptures, and recommending us to commit fraud. If we were to make a point of committing all the frauds we expose, we should find it very inconvenient to lay them before the public. We leave Messrs G. and C. to follow their own advice; we decline it.

In conclusion, we refer all interested in this subject to the remarks contained in Nos. XXVIII. and XXIX. of THE CHEMIST, and we promise the soap trade, and all interested in the subject, that we will use every endeavor to put down this infamous system: if we do but receive assistance from those for whom we are waging war, its days will soon be at an end. We cannot do better than beseech our readers to follow out the advice given in the concluding paragraph of our last article.

* Messrs. Tennant and Sons sent the circular to all the trade in London, with the following remarks in writing:—

"C. Tennant, Sons, and Co. beg to state, that not being 'alkali brokers,' and never having sold soda ash of their own, or as agents for others, at 'their own' or their 'broker's analysis,' but subject to the analysis of a neutral party, mutually chosen by the buyers and themselves, they did not consider the article in 'THE CHEMIST' referred to them, and, therefore, they think any mention of their names in the above unnecessary."

They have taken a most unwarrantable liberty with the names in question. The parties concerned may indeed exclaim,—Save us from our friends!!

ARSENIC IN THE SODA OF COMMERCE.

BY CHARLES WATT, JUN.

DURING some recent experiments on the soda ash manufactured by means of iron pyrites, I have detected large quantities of arsenic in combination with the soda, as arseniate of soda.

As I believe that it is not generally known that the soda of commerce contains arsenic, I think it right to call the attention of the public, and more especially of chemists, to the important fact.

In consequence of the introduction of the simple and ingenious apparatus invented by Mr. Marsh, we are enabled to detect very minute quantities of arsenic; and as soda is frequently taken into the stomach in articles of food and medicine,* I can conceive it not impossible that a person dying suddenly might be declared to have been poisoned, and which might be the cause of an innocent person suffering. Such, I say, might be the case. Let us suppose that a medical man, who is called in makes an examination of the stomach and its contents, and an analysis of what remains of the aliment or medicine of which the deceased last partook; if he finds arsenic he immediately states that the death has been caused by that substance, and some innocent person may be implicated in its administration. Now, should some of this soda have been used in the preparation of the food or medicine, although the arsenic is not in sufficient quantity to produce death, yet there is enough to be detected by our delicate means, and may lead to a wrong conclusion.

* There are many articles of food, such as pastry, in which soda is used: it is likewise extensively used in medicine, as in Soda Powders, &c. &c. &c.

There are many other serious evils which may, and probably do, arise from this: we know that a few grains of arsenic are sufficient to produce death; and who is prepared to say that a quantity, however small, when frequently repeated, is harmless? Indeed, if any one had the hardihood to make such an assertion, who would place reliance on him?

The soda in which I have detected arsenic is, as I before stated, that manufactured by iron pyrites, and which soda (in the state in which it is sold in commerce) may be known by its assuming a brownish color when exposed to the air, in consequence of its containing iron, which passes into the state of free oxide, generally amounting to 1 or 2 per cent.*

The arsenic in the soda may be detected in the usual way, first saturating the alkali by dilute sulphuric or some other acid. I have found a stout flask, made of green glass, with a stop-cock tightly fitted into its neck by means of a cork, answer fully as well as the tube, &c. invented by Mr. Marsh, and it has one great advantage over it, that of not being nearly so liable to be broken. Care should be taken to let the hydrogen drive all the atmospheric air out of the flask before the light is applied to the stop-cock, otherwise an explosion will take place, the apparatus will be shattered to pieces, and the operator exposed to injury from the fragments of glass and acid which are scattered in every direction. In conclusion, I must express a hope that the gentlemen who manufacture soda by means of iron pyrites will turn their attention to this subject, with a view to the discovery of some plan by which they may get rid of this deleterious substance from their soda ash.

PRESENCE OF IODINE IN THE NITRIC ACID OF COMMERCE.†

BY M. LEMBERT.

Among the substances which render the nitric acid of commerce impure, there is one which is not unimportant, and whose existence in this acid has not hitherto been suspected; I allude to iodine. The following are the means by which I have been enabled to prove its presence.

The first time that I recognised it, was in purifying nitric acid by the following process:—

When I desire to obtain pure and concentrated nitric acid, I take commercial nitric acid, add to it nitrate of silver, and leave it for a few days to settle; when the liquid is clear, I decant, add sulphuric acid at 66°, in weight equal to that of the nitric acid employed, and distil. Now, one day, after having distilled all the nitric acid, I wished to concentrate the sulphuric acid which remained in the retort; when it was near its point of concentration, violet vapors filled the retort, and I obtained iodine crystallised in the glass tube traversing a vessel filled with water, which served as a refrigerant.

Saturate the nitric acid containing iodine by potassa, soda or a carbonate of those bases; when the liquid is neutral, add to it a small quantity of a clear solution of starch, then a few drops of sulphuric acid, taking care not to add another drop of sulpuric acid, until it is ascertained that the preceding one has caused no coloration. The presence of iodine will be evinced by the blue or violet color which the liquid will acquire.

In reflecting on the origin of the iodine in nitric acid, it was natural to think that it arose from the native nitrate of soda used in the manufacture of this acid. I therefore tested native nitrate of soda by the process indicated above, that is to say, I added to the solution of this salt, a little solution of starch, then a small quantity of sulphuric acid, and I obtained the coloration indicative of the presence of iodine.

It is to be remarked: 1st. That concentrated nitric acid, namely, at 41°, contains iodine, and that weak acid, from 35° to 36°, does not; this at least, is the result of my observations as regards different samples of acid, at 35° or 36°, and 40° or 42°: 2nd. That neither chlorine nor sulphurous acid indicate the presence of iodine, in either the native nitrate of soda, or in the neutralised acid.

TINTING OF PHOTOGRAPHIC DRAWINGS.*

M. Bisson, who had previously sent various very remarkable photographic products to the Academy of Sciences, Paris, presented to it at its sitting of the 7th of February, 1842, new photographic proofs, whose color, which was different from that of those obtained by the ordinary process, seemed in certain cases to be preferable, as giving a more agreeable appearance to the drawings.

* Soda *ash* is here alluded to, not crystallised soda. The crystals of soda, however prepared, contain a mere trace of iron; therefore, the method by which they have been manufactured cannot be detected by this means.

† *Journal de Pharmacie et de Chimie*, April, 1842.

* *Comptes Rendus*, No. 6, 7 Fevrier, 1842.

This result was obtained by placing in the cup which contains the mercury, a small quantity of iodine in the state of alcoholic solution: the iodine and the mercury were vaporised at the same time, and gave to the image, which previously was invisible, the desired color.

III. PHARMACY, &c.

SIR JAMES GRAHAM'S MEDICAL REFORM BILL.

We may now congratulate those who are interested in the well being and respectability of the medical profession, that a Government 'measure of Medical Reform' is to be brought forward during the present session of Parliament, by Sir James Graham, as he stated to the House on Monday, May 23d. The nature of this measure, and the various clauses which are comprised in the Bill, are, of course, matters of mere speculation, which will not be revealed until the Act is before the House. We trust, however, that the better education and a far more searching method of ascertaining the acquirements, application, and moral habits of the pupils will form the chief object of the contemplated enactment, and next to it the total suppression from practice and the administration of remedies in any but such as are duly qualified and licensed. If this measure is carried to its proper extent, quackery, in and out of the profession, will at once be prevented, and the morality and decency of the public no longer be sullied by the licentious and filthy obscenity with which the public prints abound. Under changes such as these, we might hope soon to see the medical profession take a respectable and dignified rank among the higher pursuits of civilised man.

The great question, as we have often before stated, seems to rest between the Apothecaries and the Chemists, the former wishing to suppress the counter practice of the latter, and the latter, of course, to be still allowed to exercise it, and receive its profits. The apothecaries, with good reason, think that for the trouble, expense, and labor in acquiring professional knowledge, they are entitled to be protected in the practice of it, and not to be put merely on a par with persons who have acquired no scientific knowledge at all, and have never attended at any medical school. Is this, we would ask, anything more than reasonable and fair? If the Chemists and Druggists desire to retain the privilege they now exercise with perfect impunity to a very large extent, we contend that the only condition on which they ought to be allowed the liberty, is that of due and proper qualification; and by this, we mean not the mere joining a Society and paying an annual fee, and reading the trite and commonplace articles contained in their trumpery "Transactions," as they are styled; but an education calculated to guard the public against the errors of ignorance, and to prevent cupidity from being gratified at the expense of human health and life. We shall exert all our energies to cause the total extinction of practice by uneducated persons, and shall press on the Legislature the necessity of much higher qualification than is to be derived from their motley Society, and its still more heterogeneous production.

What is to be the degree of education they are to be expected to receive, must be left until we see the Bill. If we were to judge by what we see of their desire for knowledge, we regret to say that we are convinced that a very stringent clause will be required to enforce their studies.

Long has the public experienced how little their zeal for science has been manifested—and therefore a much more powerful motive, than their own body can put into operation, is necessary to compel them to obtain a suitable acquaintance with such

of the medical branches of knowledge as may be requisite for the practice of those branches they intend to pursue, and we fear not that a proper and wholesome restriction will be introduced into the Act, to satisfy the public mind on the subject, and to set at rest this long unsatisfactory matter.

Among other points we desire to see settled, are the exclusive and monopolising powers of the Colleges: we trust that it will be enacted that any qualified person, wherever and by whomsoever taught, so long as he has acquired the proper knowledge of his subject, will, upon examination, and after being proved competent, be fully entitled to receive an authority to practise.

REFUSAL OF THE PHARMACEUTICAL SOCIETY TO RECEIVE THE SUBSCRIPTION OF A CHEMIST AND DRUGGIST.

Commercial Road, London,
May 21, 1842.

GENTLEMEN,—

I feel it to be my duty to acquaint you with the following facts, trusting that in your hands they will not fail to become useful in opening the eyes of a large number of Chemists and Druggists who are now in the dark with regard to the spirit and tendency of the Pharmaceutical Society as at present managed.

I must premise my statement by observing that the Pharmaceutical Society professes to be instituted "for the purpose of uniting the Chemists and Druggists into one ostensible, recognised and independent body—for protecting their *general* interests, &c. &c." The first principle in the "Constitution" of the Society is as follows.

"The members of this Society shall consist of Chemists and Druggists who are, or have been, in business on their own account, and who shall severally subscribe the sum of two guineas annually."

Now, Gentlemen, I am a Chemist and Druggist in business on my own account, and on Monday last I went to the office of the Society, in Bloomsbury Square, for the purpose of being registered in accordance with the constitutional principle; when I was politely informed by the Secretary that he had received instructions from the Vice-President to refuse my subscription, and not to enter my name until it should be decided by the *Council* whether or not I should be admitted at all!! Of course, I could only protest against this proceeding as most arbitrary and unjust, inasmuch as it is a breach of the constitutional principle which invests *every* Chemist and Druggist with the *right* of becoming a member if he should choose to pay the subscription. But I was very coolly told that Chemists and Druggists are only *eligible*, and must be *elected by the Council* before they can become members!!

You will naturally inquire what can be the reason of this extraordinary conduct? Why, Gentlemen, instead of paying my money and holding my tongue, I had previously asked of one of the "Council" a few awkward questions. I was anxious to know what other advantage would accrue to me by becoming a member of this Society, besides the honor (?) of affixing to my name the magniloquent M. P. S. G. B.

As the protection of the right to prescribe behind the counter was the vital principle which called the Pharmaceutical Society into existence; in order to test the value of it, I put my own case thus:—"I served an apprenticeship to a Chemist and Druggist, and I have always acted under the impression that I had both a moral and legal right to prescribe in any case which came before me, and which I understood; and when an assistant, in order to exercise this right in a more efficient and scientific manner, I entered one of the public Schools of Medicine, and attended Lectures on Chemistry, Materia Medica, Anatomy and Physiology, and the Principles and Practice of Medicine; and likewise attended one of the Dispensaries in company with the Physicians and Surgeons, in order to become practically acquainted with the nature and treatment of cases similar to those which so frequently come under the observation of Chemists and Druggists. Now, if an individual with inflammation of the lungs were to apply to me for advice, and if I were to bleed him and treat him properly, so that he becomes perfectly restored to health; in such a case, suppose the Apothecaries' Company should commence an action against me, for practising behind the counter, will the Pharmaceutical Society defend that action if I were a member and did not go out to visit patients?" The reply I received was,—"No. I do not think the Council would defend such an action. Moreover, if your object in joining the Society is merely for PROTECTION, *we had rather be without you!!!*"

After this, it requires but little sagacity to perceive that this "general interest" Society will soon end in a lame attempt to establish another odious monopoly,—a bad imitation of the Apothecaries' Company. It is pretty evident, that when the crisis arrives,

those who prescribe behind the counter to any extent will be thrown overboard, to enable the dispensing establishments to obtain an Act of Parliament for their own exclusive benefit. Now that the excitement produced by Mr. Hawes's Bill is fast dying away, I think the great bulk of Chemists and Druggists will see their way clear before they continue their pecuniary support to such a monstrosity, which bids fair to be the most effectual means of depriving them of that very right to defend which the Pharmaceutical Society was originally instituted.

I remain, respectfully yours,

HILDRETH KAY.

CHEMICAL JURISPRUDENCE. — CASE OF COOPER THE FOOTPAD.—IMPROPRIETY OF ADMITTING, IN COURTS OF LAW, THE ANALYSES OF MEDICAL MEN, IN CASES AFFECTING LIFE AND PROPERTY.

BY JOHN WATT.

OUR readers, at least that portion of them who are given to the perusal of the newspaper accounts of murders, will recollect that the "unfortunate man," Thomas Cooper, the footpad, who now awaits his trial at the Central Criminal Court for the murder of the policeman Daly, confessed to having poisoned himself, when he found capture inevitable, by swallowing arsenic and laudanum.* Shortly afterwards, a bottle† was found in a field at Hornsey, where the murder was perpetrated, in which there remained a very small portion of liquid. This liquid was analysed by a *medical man*, whose name we forget, who reported to the magistrates that it contained "a metallic poison."

We allude to this subject merely with the object of expressing our opinion, that in cases of poisoning, the analyses of medical practitioners should never be relied on. These gentlemen, however eminently they may be qualified for the performance of those important duties which fall within their legitimate province, are very seldom, if ever, sufficiently capable of conducting chemical analyses—especially those on which the life of a fellow-creature may depend. In the present instance, we believe, the respectable surgeon in question was really competent to the task: it is not the individual that we object to; it is the principle—or rather the absence of principle.

It may be said that the circumstance of Cooper having taken poison may not very materially affect his case; possibly not; still, as Mr. Horry, his counsel, in applying for the postponement of his trial until the ensuing session, stated that evidence of his insanity would be adduced, this may be brought forward as collateral evidence.

In cases wherein an analysis affecting life has to be performed, or chemical jurisprudence, as it is not unfrequently and not unaptly termed by the French Chemists, that analysis should be executed by an experienced practical chemist. Medical men, from the extent to which they are required to study and practise other sciences, are seldom able to attain a decent proficiency in chemistry, and very few of them ever attempt to attain more than is sufficient to enable them to answer the ignorant questions of a Board of Examiners, who know little, if any, more about it, nay, very often much less, than those whom they catechise. No chemist should be allowed to give his report in any case of this kind, unless he can satisfy the court of his qualification. Now, the only satisfactory evidence that could be adduced would be a diploma. It would be a most beneficial measure to the public, to compel scientific chemists to undergo an examination, and to give them a certificate on their passing, previous to allowing them to come forward as witnesses or otherwise, in cases where life or property is concerned. Of course, those who are already in practice would not be affected by any such enactment.

It will be understood that we do not mean to suggest that the certificate granted should be that of the Apothecaries' Company. The proper diploma would be that of the London University.

Our observations likewise apply to those cases in which property is concerned, and which are more important than at first sight may appear.

In a future Number we may take up this subject at greater length and with more effect: for the present, we dismiss it, as it is here our wish only to throw out a few hints.

* For an interesting paper on poisoning by arsenic and opium, *see* THE CHRMIST, Vol. II. p. 190.

† It is a circumstance not unworthy of remark, that the bottle, when picked up, was corked: Cooper must have taken the trouble, after swallowing the poison, to cork it previous to throwing it away.

ON CLINKERS, A NEW REMEDIAL AGENT.*

BY CONWAY J. EDWARDS, M.R.C.S., BATHEASTON, BATH.

THE addition to our very extended catalogue of medicines, to which I would wish to draw the attention of the profession, is of a nature so simple, that medical gentlemen will rather be inclined to doubt its possession of any medicinal properties than to believe that it contains those which can effect great and beneficial changes in diseases, which frequently baffle the most eminent of the profession. Indeed, the very name of "clinkers" (under which title I heg leave to introduce it) is calculated to set the seal of condemnation on the suhstance, without affording it the benefit of further consideration; nevertheless, in face of the great obstacle to a "fair hearing," which such a name must of necessity impose, I havc no hesitation in advancing an opinion, that its powers as a tonic and colorific are of no mean order; and this statement is supported by the experience of Dr. Watson, of Bath, who was the first initiated in the mystery of its preparation, and the first to sanction its use.

"Clinkers" is the refuse of the hlacksmith's forge, and differs from common ashes and coke in its greater specific gravity, component parts, and external appearance. As a medicine in cachectic disorders, particularly those of females, it has been used for many years, by "knowing old women" in certain manufacturing districts; and the success which attends its exhibition, particularly in chlorotic disorders, is such as to have won for it the title of "specific."

The following is the formula for its preparation:—The bluest and heaviest clinker, heing selected from a mass, is reduced to an impalpable powder (a work of no small difficulty, on account of its metalloid nature). Any quantity of this powder may be mixed, with a sufficiency of treacle, to form a stiff paste; and to every eight ounces of the mass, half an ounce of magnesia, and the like quantity of ginger, must be added. Thus formed, it is any thing but inviting to the eye; but this can be remedied by using honey in lieu of treacle, and adding half a drachm of the peroxide of iron to the compound.

The directions which accompanied the formula were as unique as the formula itself. It must be taken three successive days and nights (twice a day,) for three days; omitted for a like period, and so continued until the course which has heen decided upon should he finished. The dose is a teaspoonful. Absurd as these directions seem, they are not so ridiculous as they appear, for experience has demonstrated that constitutional irritation supervenes, unless some decided interval is allowed at stated periods, during a course of this remedy.

The first time I heard of this medicine, was after it had effected a happy change in the constitution of a young lady, who had heen suffering from some internal but ohscure disorder, for several years. The circulation, through the minute ramifications of the hlood-vessels, was reduced to its lowest ehh, while the fluid which circulated in the larger channels was a turbid brown, instead of the rich color for which hlood is remarkable. Under such a state of things, the exsanguineous appearance of the skin may be readily conceived. She had been under the professional care of two medical men before I was consulted, and, when called in, I found her suffering from all those symptoms of dehility, which characterise uterine and ovarian disturbance. From some remarkable symptoms, I was led to fix the seat of the disease in the ovaries. During my attendance I tried every medicine, and combination of medicines, which bore upon the case; but no permanent benefit resulted from any of them: the only preparations which produced a tolerable effect were those of iron, and among them none possessed such power as the ioduret. The constitution hecame evidently impaired; the feet swelled; there was distension; and disorganization appeared to be making rapid advances. A very distinguished physician was called in, but could give no opinion on the nature of the case. He attended for several weeks with no success; during this gentleman's attendance, most agonising pains in the head came on; they were neuralgic; for the relief of these a veratrine ointment was applied; then veratrine delphia and morphine, suspended in oil; æther and ammonia were rubhed in: all these failed. I then proposed passing an electric current through the affected part, to which assent was given, but with a similar result. Le Fai's ointment was advised by some friend, and, I must certainly admit, with partial success. I never witnessed sufferings so acute—so continued. Another eminent physician was now consulted. "We must," he said, "build up the constitution; it will be a work of time, but it must be done." And a work of time it might have

* *Provincial Med. and Surg. Journal*, Feb. 5, 1842; and *London and Edinburgh Monthly Journal of Medical Science*, May, 1842.

been, for after a considerable attendance no radical good was effected. Still, the principle on which this gentleman acted was sound, and although, as regarded the original affection, things had not improved, there was no retrograde movement, and possibly there might have been an important and visible advance, had the attendance extended over three or four years. Brighton was resorted to and failed; the Isle of Wight, Clevedon, and several other maritime places, tried; but in vain. Medicine, diet, and change of air, were alike unavailing. Dr. Watson, of Bath, was called in, but no decided opinion could be pronounced, and, consequently, no very decided plan of treatment adopted. The case seemed hopeless, when a young lady, whose suffering had been similar to those of this patient, and whose case had baffled the skill of her professional advisers, recommended her to have recourse to the remedy which had restored her to health; a large number of cases were also adduced in favor of its efficacy. The offer was eagerly embraced, and, Dr. Watson's consent being obtained, the trial of the "clinkers" commenced.

The result exceeded all expectations. In two months the feet ceased to swell; the tumidity of the hypogastric and umbilical regions subsided; the functions of the stomach were restored; the small wiry pulse of above 100 was converted into a healthy beat of 80; the neuralgic pains ceased; and, what was most extraordinary and pleasing, the capillary circulation became so improved, that the skin assumed a vital hue, and the cheeks denoted returning health; every morbid symptom disappeared, except an excess of uterine secretion. The happy change surprised the professional attendants, as well as the patient's friends and relations; and without being at all enthusiastic in praise of the "clinkers," it may be fairly admitted that the beneficial results in this interesting case were owing to its medicinal properties.

Its success here made me anxious to test its powers further; and the lady having been so obliging as to give me the formula for its preparation, several patients were collected in whom the exsanguineous state of the skin, and wasting of the muscular fibre, were indicative of the morbid manner in which the functions of the stomach, alimentary canal, and uterus were carried on. Within a month from commencing the use of the clinkers, a striking improvement took place in the appearance of the patients, and before the termination of two months every unfavorable symptom disappeared. One case, in particular, is well worthy of especial notice, by reason of the scrofulous condition of the submaxillary glands, and the ulcerated state in which one of them had been for several years, and which healed during the course of the new medicine.

I may observe, *en passant*, that previously to a trial of the clinkers, this young woman had been under professional treatment for some time, without deriving any benefit from it.

The pulse was weak, and nearly 100; catamenia irregular in appearance, variable in quantity, and unnatural in composition; appetite and sleep bad; tongue foul; and what, perhaps, may be termed hysterical hypochondriasis, existed to a great degree. A gentle cathartic of infusion of senna, with tartrate of potass, was given for a few successive days before the clinkers were commenced. She had not taken the new medicine six weeks, when most of the distressing symptoms disappeared, and her personal appearance became so improved, that her friends scarcely recognised in the ruddy girl before them, the pallid and unhealthy creature she had once been.

It appears greatly to benefit cases of simple indigestion, a few doses being capable of removing the most distressing symptoms. In that peculiar condition of the secretion of the bowels, which is said to favour the formation or propagation of worms, it has proved advantageous in two ways—one, by its mechanical action; the other, by its tonic properties. This was an accidental discovery made during its trial in a case of leucorrhœa.

When the medicine is taken for the first time, a train of symptoms frequently supervenes, which would induce a stranger to its *modus operandi* to regard it as a dangerous compound. A great weight is felt in the epigastric region, accompanied with a burning sensation; sensations of sickness, followed by those of fainting, come on; these are soon relieved by eructations of flatus. Some complain of pains in the limbs, and particularly the joints; others of tightness across the forehead, with giddiness; while all are troubled with heat, dryness of the mouth, and great thirst. At the second dose the symptoms are moderated, and the third is generally taken with impunity. After it has been persevered with for a short time, sensations of a different character arise; these are hunger, and a feeling of health and energy, to which, perhaps, the patient has been a stranger for many years; then the complexion, if pale, commonly receives a ruddy tint, and the muscular fibre becomes firm and enlarges. After the first dose the fæces are like pitch, the urine generally pale, and large in quantity; the bowels,

if previously costive, become regular in their action; the pulse gets full, and the skin pleasantly relaxed.

To sum up its medicinal properties, it may be said that the clinker is tonic, stimulant, anthelmintic, and colorific, adapted generally to a leuco-phlegmatic habit, and where dyspeptic, chlorotic, and scrofulous complaints exist. "Its use would be contra-indicated where an inflammatory diathesis prevails."

The quantity of metal which clinker contains varies considerably; the best is obtained from a blacksmith's forge, and the most ponderous, darkest, and metallic in appearance, is that only on which dependence can be placed. The light slate-colored clinker is inert.

I need not offer any remarks on the magnesia and ginger, which are added in the formation of the linctus; but I may observe, that if the ginger be omitted, violent griping ensues. After the medicine has been given for a few weeks, no bad symptoms arise if it is exhibited more frequently than at the beginning of the course.

From the imperfect analysis which I have made of the clinker, I should say that, with the usual substances found in coal partially decomposed by heat, a metalline appearance pervades the mass, which seems to be iron combined with carbon, so as to form steel; no doubt the metal exists likewise, both as a sulphuret and a carbonate of the protoxide, but neither of these would afford the blue tint for which the clinker is remarkable. That it is not titanium is evident, for that metal is "like burnished copper," and so little fusible, that the heat of the oxyhydrogen blowpipe, when in action, is scarcely able to touch it—at least, if we may believe our professors. If, therefore, the heat of highly compressed gases intimately mingled be just capable of oxidising titanium, can we expect that the fire of the common forge would do so? From whence, then, do we get titanic acid? Surely not in or from the various new compound bodies which are formed during the decomposition of the coal! Here we have first a mass of small coal, with, perhaps, charcoal, kindled into a flame; over this water is thrown, the decomposition of which increases the heat. When the temperature is at its maximum, a bar of soft iron is plunged into the centre, and rapidly reaches the temperature of the surrounding substances; particles of highly ignited metal are thrown off; some in a semifused state unite with particles of carbon, and perhaps form steel; others combine immediately with the oxygen of the water, and become a protocarbonate of the protoxide, while other portions form a sulphuret, and appear through the whole mass as new compounds endowed with new properties, which had no existence before the interchange of elementary principle. But this is not all; as the coal is decomposed it is raked off and thrown on one side, fresh supplies being added, until every component particle of the iron is raised to a white heat; it is then taken from the fire, placed on an anvil, and subjected to a succession of blows from the sledge hammer; this drives off large *flakes* of metal, many of which bury themselves in the masses of coke which have been cast aside, and are known as "clinkers." Now, if a portion of this be mixed with nitrate of potass, and strongly heated, a brown powder is formed, in which (had titanium existence) would be mingled with the white peroxide; a solution of gall nuts would then produce the orange red colour, which is said to be characteristic of titanium; and were a rod of zinc suspended in the solution, a purple-colored powder would be precipitated. In making the experiments, I strictly adhered to the rules laid down for testing its presence, but perhaps, as I did not discover titanium or its oxide, it was owing to want of dexterity in the manipulations.

That the singularly beneficial effects which the clinker produces in certain conditions of the system cannot result solely from the iron or steel which it contains, the experience we have of those metals, and their preparations, sufficiently declare: some new combination must exist, to effect such singular changes; what it is remains to be proved. Electricity has been advanced as the cause, but even admitting that galvanic currents could be generated, they would be so weak in power and small in quantity, as to be incapable of producing any results, while their source would prove so limited, that the electric evolution would cease before the linctus could be formed.

This is all which I have to advance on the subject at present. I must apologise for the imperfect manner in which it has been treated, yet hope that those members of the profession who have honored these observations with a perusal, will divest themselves of all prejudice against the use of so humble a preparation, and give it that impartial trial which it would seem to deserve.

EXAMINATION OF A POWDER SOLD AS A CURE FOR THE DISEASES OF DOGS, CALLED KUSIC POWDER (*POUDRE KUSIQUE*).

BY E. HABERT.

This powder, which is administered to diseased dogs, or to prevent the disorders to

which these animals are subject, is sold in very large quantities in the provinces: it is brought from Paris, where it is sold at 30 centimes the three packets, each containing two grammes of powder. Each dose, consisting of three packets of two grammes, is accompanied by a prospectus.*

This powder, which is very fine, is of a dirty yellow color; when thrown into the fire, it burns with scintillation, and gives rise to a disengagement of sulphurous acid; its taste is cooling, and it has not much odor.

Five grammes of this powder were introduced into a phial, and treated with water and heat; the filtered residue, evaporated, furnished a saline residue weighing 2 grammes and 50 centigrammes. The examination of this residue showed that it was pure nitrate of potassa.

The residue, insoluble in water, treated by alcohol, in order to ascertain whether it contained any resinous matters, gave not the slightest trace of those substances.

The part which was insoluble in water and alcohol was evidently sulphur, but it was mixed with a foreign substance; when dried, it weighed 2 grammes, 45 centigrammes. It was introduced into a glass retort, and submitted to sublimation This sulphur, thus treated, was volatilised, and left a residue weighing 5 centigrammes, and which, on examination, was found to be charcoal, containing traces of iron, which was detected by calcination, hydrochloric acid, and prussiate of potassa.

From these trials it results that 100 parts of this powder are composed of

Nitrate of potassa . .	50 parts.
Sulphur in powder . .	49
Charcoal in powder . .	1
	100

We think that charcoal is added to the mixture only to disguise the color of the sulphur, and in order that the nature of the powder may not be suspected.

If it be wished to prepare the packets of Kusic powder, 100 parts of nitrate of potassa, 98 of washed sulphur, and 2 of charcoal in fine powder, must be taken.

Mix accurately and divide into doses of 2 grammes; three doses of 2 grammes each form the packet.

* The following is the prospectus:—

"Poudre Kusique."

"Three doses of this powder are given, one every day, taking care, for the last, to allow a day to intervene: the treatment is one packet for a common sized dog, and two for a large dog.

"It may be administered in a ball of butter or paste, or in a spoonful of milk, by insinuating it into the mouth.

"If the animal be not quite cured, the same treatment must be recommenced eight days afterwards.

"Those who are careful of their dogs may make them take two packets every three months, in order to prevent the disorders to which dogs that work are liable."

OINTMENT OF NITRATE OF MERCURY.

BY A. J. COOLEY.

The Unguentum Hydrargyri Nitratis of the London Pharmacopœia has been very generally and deservedly objected to, on account of the great hardness and friability it acquires after the lapse of a few days, and the loss of pots which it frequently occasions by its subsequent expansion. Various schemes have been proposed to remedy these evils, without lessening or altering the properties of the preparation; but in most cases the parties have contented themselves with looking at effects instead of causes, and have thus been led to recommend plans, which have proved either troublesome or expensive, and not unfrequently inconvenient and useless.

In The Chemist, No. XV. page 90, it is stated by a correspondent, that if the solution of the mercury in the nitric acid be allowed to cool before admixture with the oil, the ointment will *always* retain the consistence of spermaceti ointment. This I have found to be incorrect; for whether the union be made *hot* or *cold*, or whether oil be wholly employed, instead of lard and oil as directed in the Pharmacopœia, the ointment is still the same, and the operator runs the risk of the precipitation of minute crystals of salt, by allowing the solution of the mercury to become *quite cold*. This has frequently happened in my experiments, which have been conducted on a very extensive scale.

In The Chemist, No. XVI., a very intelligent correspondent, who signs himself "Gulielmus," asks the gentleman above alluded to, to explain his process more minutely, as he was evidently mistaken as to the advantage of his proposed alteration of the Pharmacopœia formula. This person then quotes Dr. Paris on the process of testing olive oil with acid nitrate of mercury; but having thus hit upon the only correct mode of conducting an inquiry into the subject, as I shall presently show, at once closes his letter.

In The Chemist, No. XVIII., Mr. Harvey, of Penzance, has very kindly published his form for this article, which certainly produces an ointment of the finest quality, but is objectionable, from containing less acid than directed by the College; and cocoa-nut oil not being always procurable, especially in the country, or in winter. This inconvenience has, however, been proposed to be remedied, by the addition of about one twelfth part of white wax and spermaceti to any given weight of olive oil, and then employing this mixture instead of the lard and oil of the Pharmacopœia, or the cocoa-nut oil recommended by Mr. Harvey. The particulars of the process will be found in The Chemist, No. XXIV., and would be quite satisfactory, though more costly than the above, were it not that the ointment thus made soon becomes rather harder than is desirable, or than it may be maintained in by other means. The color is also inferior to what is generally met with in trade. Other methods have been proposed which it is here unnecessary to mention, though it may not be uninteresting to state that the subject has frequently engaged the attention of the pharmaceutist.

The practical chemist, in following out the object of this Memoir, should immediately direct his attention to the properties of the ingredients employed in compounding this ointment.

Nitric acid. } Acid nitrate
Mercury. } of mercury.
Olive oil.
Lard.

What are the effects of an admixture of the whole, and also of the solution of the salt with the oil and lard, each separately? Your correspondent in The Chemist, No. XVI., has answered the question—has, in fact, furnished data for the whole inquiry. He quotes Dr. Paris (he might also have quoted a thousand others,) to show that an acid nitrate of mercury combines with and changes *pure* olive oil, in a few hours, into a solid body of great firmness. Other oils have not this property, then why may they not be substituted? The answer is evident. They want the power of intimate combination and consistence.

The College, with the intention of preserving both these properties, introduced an excess of lard into the last Pharmacopœia, but with what effect, need hardly be mentioned. There is scarcely a journeyman tallow-melter in the United Kingdom who could not have told these gentlemen that acids (especially strong acids,) uniformly harden solid fats. Such was the result in this ointment. The consistence for keeping was not improved.

It appears astonishing that the College should have staggered into such a blunder, as a knowledge of the properties of olive oil have at once suggested a suitable remedy. There is scarcely a work on the subject of Pharmacy or Chemistry that does not inform us that if the above oil be adulterated, it loses its hardening property in proportion to the amount of the adulteration. But with what oil is this article usually lowered? We know it is seed oil,* though this is of little consequence, as all the mild fatty oils would answer this purpose. The United States Pharmacopœia orders a certain proportion of neat's foot oil, for instance. We have then at once our remedy. Our next inquiry must be directed to ascertain the proper proportions to produce a good color, and a suitable consistence.

I am in constant practice of using the following form, which makes an ointment, which acquires, in the course of a few hours, a great brilliancy and beauty of color, and maintains an excellent consistence.

Hydrargyrum 8 parts } The strength
Acid Nit. . 11 ditto } as ordered
Ol. Oliv. Opt. 26 ditto } in the
Ol. Nucis . 54 ditto } Pharmacopœia.

As soon as the metal dissolves, add it to the oils, previously mixed together, and stir as often as convenient until nearly solid.

From the above remarks it will be seen that instead of nut oil, any other fat oil, as that of almonds, cocoa-nut oil, &c., may be employed, observing to keep the proportion of olive oil always the same. The nut oil gives, however, a finer color. I frequently add to the above composition $\frac{1}{24}$ part each of wax and spermaceti (by weight) of the two oils employed, at the same time omitting this weight of the oils, to keep the strength uniform.

The form would then stand as follows:—

Hydrargyrum . . . . . 8 parts.
Acid Nitrat. . . . . 11 ditto.
Ol. Olive Opt. . . . . 24 ditto.
Cera Alb. et Cetaceum, āā . 3 ditto.
Ol. Nucis vel Amygd. &c. . 50 ditto.

The wax and spermaceti must be dissolved (by heat) in the oils, and the mixture allowed to cool before adding the solution of mercury.

When thus made, the ointment becomes sooner fit for use, but requires some hours longer to acquire a bright color. Either process may be relied on.

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* Very commonly poppy oil.—Eds. Chem.

PROCESS FOR THE PREPARATION OF IODIDE OF GOLD.*

BY M. A. MEILLET.

This process consists in treating a solution of chloride of gold, as neutral as possible, by pouring neutral hydriodate of ammonia into the solution, until no further precipitate is formed. The solutions used should not be very dilute.

When solution is effected, a small quantity of alcohol, about one-third, is added. It is left to settle; after standing for some hours, it is decanted, and we then have a blackish precipitate composed of iodide of gold and iodine; it is washed by decantation with alcohol, which dissolves the iodine, and almost white iodide of gold is obtained; it is dried in the open air on plates, and it is preserved from the light in corked glass flasks.

The advantages of this process, says M. Meillet, are, 1st, that the gold is completely precipitated, which is never the case with the iodide of potassium; 2nd, that the iodide thus obtained, has an invariable composition.

ON THE TREATMENT OF VARIX BY MEANS OF VIENNA CAUSTIC.†

BY M. A. BERARD.

The mode in which M. Bérard applies this kind of treatment has some peculiarities, which he holds to be of great importance in conducing to its success. In the first place, the point he selects for applying the caustic is below the knee, over the course of the *vena saphena interna*. This will generally be found sufficient to remove any varicose veins that may exist in the thigh; and even though they should not be removed, experience proves that they will cause no further inconvenience if the operation succeeds in respect to the leg; while any dangerous accidents after the application are much more likely to occur when it is used above the knee than below it. The obliteration of the vein at this point is usually sufficient to cause the disappearance of the other varices in the leg, though it may sometimes prove necessary afterwards to apply the caustic to one or two other veins, should they continue enlarged. In the next place, instead of making several successive applications of the caustic upon the same point, he applies it in sufficient quantity, or for a sufficient space of time, to burn at once down to the coats of the vessel. This will be effected by allowing a thin layer of the substance, brought to the consistence of a paste by the acid of alcohol, to remain on from a quarter of an hour to half an hour. Lastly, he produces a long cicatrix, instead of a circular one, by applying the paste for some way downwards along the course of the vessel. M. Bérard states the advantages of the application used in the manner here prescribed to be, that it requires little or no suspension of the patient's ordinary occupations, that in general one single operation will prove sufficient, that it is seldomer liable to be followed by serious accidents than the modes commonly in use, and that it hardly ever fails in effecting a permanent cure.

* *Journal de Chimie Médicale*, January, 1842.

† *Gazette Médicale de Paris.*

SOLUTIONS AGAINST INFLAMMATION OF THE BACK OF THE MOUTH AND THE DIGESTIVE TUBE.

BY M. NARDO.

I. ℞ Oxalic Acid from 3 to 8 decigr.
Solution of Gum Arabic 93 gram.
Simple Syrup 31 gram.
Mix well.

To be taken by the spoonful at short intervals, retaining it in the mouth and swallowing it slowly.

It is used in the varieties of angina, gastritis, gastro-enteritis, &c.

II. Decoction of barley 500 gram.
Oxalic acid, from 12 decigr. to 4 gram.

It must be well mixed, and may be used for scorbutic or venereal ulcers situated in the mouth.

ON THE PREPARATION OF SYRUP OF SAPONARIA (SOAP-WORT).

In M. Gricbourt's Pharmacopœia there is the following formula for this syrup:—

Saponaria . . 62 grammes.
Boiling water . 1000 grammes.

Make an infusion; strain, after 12 hours, and add

White sugar . 1000 grammes.

Boil until the syrup acquires a density of 30°.

M. Cousserau, apothecary at Toulouse, thinking that there was not in any pharmacological work a formula for the preparation of this syrup, has occupied himself in seeking some practical means of obtaining it. After being convinced, by several trials and experiments, that the root of the officinal saponaria, previous to flowering, was the part of this plant which contains most the more active principle, he proposed the following mode of preparation:—

I. ALCOHOLIC EXTRACT OF SAPONARIA ROOT.

℞ Root of saponaria, gathered before the flowering of the plant, and reduced to a coarse powder	1000 gram.
Alcohol at 21° (Cartier) .	6500 gram.

Macerate the root for twenty-four hours in four kilogr. of alcohol; boil and filter while hot. Subject the residue to the action of two kilogrammes of alcohol at the same temperature; place the whole on a filter: add by degrees the remaining five hundred grammes of alcohol; subject it to strong pressure.

The filtered liquors should afterwards be submitted to distillation in a sand-bath, to remove the greater part of the alcohol, and the residue, evaporated at the same temperature, should be dried in an oven in order to obtain a dry extract.

II. SYRUP OF SAPONARIA.

℞ Simple syrup . . .	60 gram.
Alcoholic extract of saponaria	1000 gram.
Distilled water . .	120 gram.

Dissolve the extract in the water, filter and add to the syrup, previously concentrated, to 900 gram., in order to be brought to 1000 gram. by the addition of the extractive solution.

The author having obtained from 240 to 250 gram. of extract per kilogramme of root, thought it necessary to fix the proportions at 60 gram. of extract for each kilogr. of syrup: so that this quantity represents 250 gram. of root, and so that one spoonful of syrup contains the extractive and medicinal matter of 7 or 8 gram. (about 2 drachms) of root.

ON THE STYPTIC EFFECT OF CREASOTE.*

BY DR. BURDACH.

A ROBUST countryman divided the ulnar artery with a sharp knife, the consequence of which was repeated bleedings, which, however, were staunched by surgical aid, Three weeks afterwards, hemorrhage returned, and Dr. Burdach of Luckan was sent for. He found the wound, which at first was a simple puncture, livid at the edges, and expanded to the size of the palm of the hand, by a spongy growth from the bottom. This spongy mass was in a gangrenous condition, and prevented the examination of the wound; the arm was swollen from the shoulder to the finger-points; it could not be moved, and was excessively painful. Dr. Burdach had only the choice between actual cautery and creasote left, for in such a state of the arm, tying the artery was out of the question. He poured 3ss of creasote, (*freed from eupion*) into the wound. This gave the patient no pain; nay, after it he enjoyed refreshing sleep for the first time since the accident. There was no more hemorrhage; the pouring in of the creasote was repeated morning and evening, and the spongy mass gradually diminished, and three days afterwards, under the co-operating influence of bandages moistened with creasote, Ol. Tereb. and Balsam. indic., loosened itself from the bottom of the wound. The divided artery was no more visible, the swelling of the arm decreased, and the complete cure shortly followed without any relapse.

* *Medizinische Zeitung*, Jahrg. 1840, N. 31, and *London and Edinburgh Monthly Journal of Medical Science*, May, 1842.

HÆMOSTATIC WATER.

M. DESCHAMPS' FORMULA.

℞. Turpentine	500 grammes.
Water	600 grammes.

THE turpentine and the water are weighed; they are put into a porcelain capsule and boiled for a quarter of an hour, the mixture is weighed and must give a total of 1000 gram., consisting of water and resin; it is cooled and filtered.

It is to be prepared by digestion acting in close vessels.

This hæmostatic water appears to retain its properties for a long time; employed five months after its preparation, it was still efficient, although it had deposited a flocculent precipitate.

TREATMENT OF STRANGULATED HERNIA BY OPIUM.*

BY GEORGE COOPER, ESQ., SURGEON, GREENWICH.

I. John Brown, aged fifty, with a large inguinal hernia, was seized with symptoms of strangulation on the 16th of November. He applied to Dr. Mitchell on the 18th, at whose request I saw him. The symptoms were most urgent; and having failed with the taxis, we proposed an operation, which he refused to submit to. On the 19th Mr. Busk saw him, and recommended me to try large doses of opium, as Mr. Bransby Cooper had informed him of a surgeon in Somersetshire, who for the last eighteen years had so treated such cases. I immediately gave four

* *London Med. Gazette*, 18th Feb. 1842.

grains, which relieved the pain and sickness, but produced no change in the tumor. In four hours I repeated the opium, and five hours after gave a third dose of four grains. 20th. Free from pain and sickness; the hernia in the same state; he had occasional sickness; and a few doses of opium were given at intervals, till the bowels acted, without any apparent alteration in the hernia, from which time he quickly recovered, and the hernia returned by degrees.

II. Mrs. Woodhouse, aged seventy, was taken with symptoms of strangulated hernia on the 22nd of January, and sent to me on the 25th. She had a femoral hernia, which I could not reduce: she is sure it was not present before the 22nd, when she had a violent fit of coughing. I gave her two grains of opium, with the effect of relieving the pain and sickness; five hours afterwards two grains more opium were administered. The following morning she said the hernia was gone, and that she was quite well. There was no return of bad symptoms, but the bowels did not act till the 29th.

FORMULÆ FOR A SYRUP OF SUBCARBONATE OF IRON AND FOR A FERRUGINOUS BEER.*

BY W. LEISTNER.

℞ Pure sulphate of iron . .	6·00
Pure subcarbonate of potassa .	6·00
Simple syrup	250·00
Tincture of orange peel . .	6·00
Powdered gum tragacanth .	0·50

After having separately treated the two salts, they are mixed with a little water, so as to form a liquid paste; they are again triturated, and the syrup is added. The gum is separately dissolved in a portion of the vehicle, and the whole is kept in a well corked flask. It is essential that the operation be promptly performed, in order that the subcarbonate of iron may not be reduced to the state of oxide.

A teaspoonful of this syrup (5 grammes) contains 5 centigrammes of subcarbonate of iron in the hydrated state,—a state in which this medicine is most easily dissolved in the stomach.

Dr. Tavernier frequently prescribes a *ferruginous beer*, which, in certain cases, may be advantageously substituted for the chalybeate waters.

M. Leistner prepares a gaseous water surcharged with hydrated carbonate of iron, of which he adds 50 grammes to each bottle of beer. This dose retains exactly 0·05 of subcarbonate of iron in solution.

I at first thought that the tannin contained in the beer would precipitate the iron; but it appears that the excess of carbonic acid gas in the beer retains it in solution, for it may be kept a very long time without alteration.

M. SOUBEIRAN'S OBSERVATIONS ON THE PRECEDING.

M. Mouchon, of Lyon, like M. Leisner, conceived the idea of suspending carbonate of iron in syrup. The following is a comparison of their two formulæ :—

Leistner.		Mouchon.		
Crystallised sulphate of iron .	2·24 = $\dot{F}\ddot{O}\ CO^2$		.	2·05 = $\dot{F}\ddot{C}$ 0·85
Carbonate of potassa .	2·24			2·05
Simple syrup	93·08	Gum syrup	.	95·90
Tincture of orange peel .	2·24			
Gum tragacanth . . .	0·20			100·00
	100·00			

It would be better to raise the proportion of sulphate of iron to 2·42; the mass would then contain exactly 1 per cent. of its weight of carbonate of iron. The most simple process consists in separately pulverising the two salts, and in triturating them with the syrup. M. Mouchon wished to put the sulphate of iron in an oven, in order to deprive it of its water of crystallisation. This practice, which is advantageous, when the object is to make a pilular mass, is useless when the salts are to be formed into a syrup.

M. Mouchon thinks that this syrup may be substituted for the pills of MM. Blaud and Vallet; for those of Blaud, certainly; but, as Vallet's pills do not contain an excess of alkaline carbonate, in order for the syrup to be analogous, half of the alkaline carbonate must be retrenched. Moreover, these syrups have not a very agreeable taste, and the pills, which are intended as a substitute for them, are almost always preferable to them.

* *Journal de Pharmacie*, Février, 1842.

PECULIAR SYMPTOMS AFFECTING AN ENTIRE FAMILY, AND TERMINATING IN DEATH.

BY JOHN WILSON, M. D., PHYSICIAN TO THE MIDDLESEX HOSPITAL.

At the meeting of the Royal Medical and Chirurgical Society, held on Tuesday, May 10, 1842, Dr. Wilson read the following interesting statement :—

The family, consisting of father, mother, and three children, were in good health previous to the evening of Dec. 30, 1840. On the first of January, 1841, the father, Giovani Arzoni, a Neapolitan, and a color manufacturer, was seized with griping pains and purging, which did not cease until death. He was occasionally sick, but never vomited. The motions were black and offensive; he had cold fits during the days, followed by fever. The joints from the first were swollen, but not red, and so painful that he could not be put into a warm bath. He was considered by the medical man who attended him to be laboring under low typhoid fever. He died on the 20th of January.

On a *post mortem* examination the stomach was found to be healthy, but slightly discolored in one part, attributable only to a natural cause; it contained a small quantity of homogeneous fluid, consisting chiefly of animal matter; intestines healthy; the lungs and plenra presented strong evidence of inflammation, quite sufficient to account for death.

The youngest child, two years of age, died on 16th January. On a *post mortem* examination there was no morbid condition of any of the viscera except the lungs, which presented only a little congestion; the stomach and intestines were healthy.

On the 26th January, the survivors, the mother and two children, were admitted into the Middlesex Hospital, under the care of Dr. Wilson. The mother, who was four months advanced in pregnancy, was taken ill on the 2nd of January, with pain in the head and sleeplessness. During the night she lost consciousness. The bowels were affected with severe pain, and there were frequent evacuations, sometimes only of fœtid gas, at others of matter like putrid flesh. Three days after she lost the use of her limbs, and had pains in the joints. On the sixth day œdema of the feet, legs, and thighs supervened, and the urine was scanty, high-colored, and offensive; water flowed from the mouth, which had a "cankery" taste. On the 15th of March she gave birth to a very small infant, which lived only twenty-four hours; she herself died on the 21st of March, laboring under symptoms of puerperal fever. The two children also died in the hospital.

"In all the three patients the most prominent features of their sufferings were the general soreness of the fleshy parts as well as of the joints, exquisite sensibility of the skin when touched, and the pain produced whenever they were moved or their position changed, according to their so frequently expressed wishes.

"Next may be noticed the œdema, and particularly that of the inferior extremities, which was present in all the three. The alkaline state of the urine when admitted, and their desire for acid drink, the faces of all being pale, haggard, and so careworn, as to give to the children the aspect of aged dwarfs, from their hollow cheeks, sharp features, exsanguined and emaciated bodies. At the same time their appetites were great, that of one of the children ravenous, but the mother's never was good; on the contrary, at times it was difficult to induce her to take even some trifling sustenance. The mother at different times was much exhausted by diarrhœa, and both the children suffered from it, but less severely. During their afflictions the intellects of all preserved their integrity. Lastly, there was the 'cankery' taste of the mouth; the 'metallic' taste of the boy; the watery state of the mouth and lips in each; the teasing, hacking, dry cough, common to all, and affecting the children, particularly towards the last. Then their lying sometimes without any covering, even during the coldest season, when at night the external atmosphere was 10 or 15 degrees below the freezing point, and when dying objecting to even a sheet, while their flesh was so greatly wasted, and their lungs becoming less and less capable of absorbing oxygen from the gradual infiltration of blood into parts of their vesicular tissue, diminishing proportionally the respirable portions. The death of the mother was accelerated by parturition, and examination after death showed very extensive disease in both the abdomen and chest, yet probably differing considerably from what might have resulted had parturition not occurred, and had she died like the children of the affection common to all of them, and not been sooner carried off by a new disease supervening on the original one.

"The abrasions or ulcerations of the sulci of the stomachs of the children have been noticed, but they were so slight that in ordinary cases they might have been overlooked, without attention having been particularly directed to that organ. Neither the mucous membrane of the intestines, nor other parts of the abdominal viscera, showed any particular change from the normal state. But the organ by which death finally entered appeared in these two to have

been by the lungs, for the blood at last seemed to have infiltrated into their tissue, rendering portions of them dark, and so heavy as to sink in water, and resembling considerably the state of the lungs of those who died of the spotted fever in 1837, and which I have described in the 'Medical Gazette' as 'non-circumscribed pulmonary apoplexy,' caused by the blood becoming so altered as to escape from its proper vessels into the tissue of the lungs, thus rendering portions of them (as in these) black, and so heavy as to sink in water; yet these parts were not circumscribed by healthy lung, but they gradually shaded off from the heavy dark parts to the respirable portions."

After the admission of this part of the Arzoni family into the hospital, the coroner caused the bodies of the father and infant to be disinterred and examined. A chemical analysis was made, an inquest was held, yet no conclusive evidence resulted; so that legal, chemical, and medical investigations combined, have not been able to unravel the mystery which envelopes the cause of death. The author offers no opinion as to what this cause may be.

Dr. Seymour was of opinion that the cause of death in all these cases must have been identical. It was a question of much interest to decide upon the nature of this cause, which was certainly enveloped in mystery.

Dr. Addison conceived from the detail of the symptoms in these cases, that all the persons had died of an obscure form of scarlet fever. The sore throat, the erythematous redness, the œdema of the lower extremities, the puffiness about the eyes, and albuminous condition of the urine which existed in one case, he believed, the pallor of the features, the rheumatic affection of the joints, and the tenderness of the surface, all indicated the disease to have been scarlet fever in a somewhat obscurer form. He was the more inclined to this opinion from the fact of every person in the family having been affected, and there being no poison which he knew of that could produce similar results.

Dr. Wilson was not aware that in either of the cases sore-throat existed, and the eruption was not at all similar to that of scarlet fever. There was no evidence of disease of the kidney; nor was the urine albuminous.

Dr. Seymour could not coincide with the opinion given by Dr. Addison; he thought the cases bore much more relation to the instances of secret poisoning which occurred in the 16th and 17th centuries, in which cases the health and strength, and eventually life itself, were destroyed by the long-continued administration of small quantities of some deleterious drug. Some medicines act as poisons in this manner. Thus, lead, in long-continued minute doses, affects the muscles, the bronchi, and eventually the brain itself. Now it was stated that the father of the family in the present instance had prepared a color, the materials composing which he kept secret, but of which, probably, one or more were poisonous.

Dr. Merriman could not suppose that the patients who were the subjects of the present communication had died of scarlet fever; for, although we occasionally saw this disease, in a single case, assume an anomalous and puzzling character, it never existed so in all the members of a family. In some of them there would be indubitable evidence of the real nature of the disease. He thought it was more likely to be a case of poisoning. Did the family live in an impure atmosphere, or were they in straitened circumstances?

Dr. Wilson said that the family lived in Charlotte-street, Fitzroy-square, and were in good circumstances. From information which had been furnished him by the coroner's clerk, he understood that the men employed by the deceased had stated that there was no poisonous material in any of the colors, the composition of which they were acquainted with. The deceased was celebrated for a preparation of *ultramarine*, the mode of preparing which he kept a profound secret. There might be poison in it.

Mr. Dalrymple remarked that the genuine *ultramarine* was prepared by calcining the *lapis lazuli*, and contained no deleterious matter. Nine tenths of the ultramarine sold, however, was prepared by factitious means. Of one mode of preparation this person, Arzoni, appeared to possess the secret. He thought the symptoms detailed in the cases under discussion, were likely to have resulted from slow poisoning.

Dr. C. J. B. Williams thought that it was not improbable that arsenic might be present in the materials used by Arzoni in preparing his ultramarine. Some imitations of this color were made from cobalt, which it was known occurred in conjunction with ores of arsenic. There was no poison, perhaps, with which we were acquainted which produced such various and anomalous symptoms, and which were referable to so great a variety of functions, as arsenic, according as it was slowly or more quickly introduced into the system. In the present instance the symptoms were similar to those produced by malaria.

Mr. Arnot had seen these patients soon after their admission into the hospital. His

first impression as to the nature of the cases was, that they were instances of slow poisoning. This impression, however, was immediately done away with by the fact that they were all in good health, and were taken ill suddenly. The symptoms presented differed from those of any disease with which he was acquainted.

Dr. Addison had heard nothing to shake the opinion which he had at first expressed. He had known scarlet fever to be confined to one family, as in this instance, and the infection to be sustained in one house for eighteen months, in some cases affecting the same person twice, and in one instance three times. The cases under discussion bore no resemblance to cases of slow poisoning. The symptoms were those of acute disease and its sequelæ.

DANGER OF MAKING USE OF THE FLESH OF POISONED ANIMALS.*

The following fact comes in support of those already published, and which have demonstrated that the flesh of fish caught in rivers by means of cocculus Indicus, and that of crows taken by aid of nux vomica, may cause more or less serious symptoms and even poisoning.

At the end of October, 1841, the Sieur Regnaud, farmer at Chapelle d'Huin, left in his field about a double decalitre of vitriolated wheat, intended for sowing. A girl, S. R., who found this wheat, gave it to a pig which she was fattening. The animal became so ill that the owner, seeing that it was very near dying, sold it. A dealer, of Chapelle d'Huin, bought and killed it; but the persons, seventeen in number, who ate the flesh of this animal, were attacked with violent colic; such was their state, that five of them received the last sacrament; none of them died, however. It was observed that those who ate of the pudding made from the blood of this pig suffered the most.

This reminds us that, in 1827, a physician of Aurillac informed the Royal Academy of Medicine that he had been called in to attend fifteen persons who had experienced symptoms of poisoning after having partaken of the milk of a goat which had drunk some very sour broth kept in an untinned copper saucepan. The goat which had furnished the milk, died on the fourth day.

On the occasion of the report made by M. Ollivier d'Augers, a discussion arose on the question of the absorption of acetate of copper—a question which could not be decided, the milk not having been analysed. It is the same in the case which we now report: it would have been of the highest interest if the flesh and blood of the pig had been examined, which might have been done with the greatest ease by the local medical men.

* *Journal de Chimie Médicale*, Janvier, 1842.

BOOKS RECEIVED.

Memoire sur l'Absorption des Poisons Métalliques par les Plantes, en Réponse à la Question suivante:—Déterminer par des expériences si les poisons métalliques, tels que l'arsenic blanc (acide arsénieux), enfouis dans un terrain cultivé, pénètrent également dans toutes les parties des végétaux qui y croissent, et entre autres dans les graines des céréales, et s'il y a, d'après cela, du danger pour la santé publique de répandre de l'acide arsénieux et d'autres poisons analogues dans les champs, pour détruire les animaux nuisibles. Par P. Louyet, Professeur du Chimie à l'Ecole Centrale de Commerce et d'Industrie de Bruxelles, &c. &c. &c. Bruxelles: Société Encyclographique des Sciences Médicales, Rue de Flandre, 155. 1841.

The Structure and Distribution of Coral Reefs. Being the first part of the Geology of the Voyage of the Beagle, under the command of Captain Fitzroy, R.N., during the Years 1832 to 1836. By Charles Darwin, M.A., F.R.S., F.G.S., Naturalist to the Expedition. Published with the approval of the Lords Commissioners of Her Majesty's Treasury. London: Smith, Elder and Co., Cornhill. 1842.

[This important work will be noticed in our next.]

ERRATUM IN No. XXVIII.

In Mr. Hiley's paper, page 105, column 1, line 22, *for* "Evergreens perspire much more than other shrubs," *read*—Evergreens perspire much less than other shrubs.

TO CORRESPONDENTS.

A Subscriber.—The formula shall be given in our next.

A New Enquirer.—We regret that we cannot give the required information; it is out of our province.

A Faithful Subscriber, Lampeter.—We do not know: nothing but actual experiment will decide.

A Subscriber, Brighton, is thanked.

Nota Bene.—*All Communications and Books for Review must be addressed, "To the Editors of the Chemist, Care of Mr. Hastings, Publisher, 13, Carey Street, Lincoln's-inn Fields, London." Communications must be prepaid, and sent before the 15th of each Month; Books for Review before the 10th.*

THE CHEMIST.

I. CHEMISTRY.

FORMATION OF FERRIC ACID BY THE GALVANIC WAY.*

BY J. C. POGGENDORFF.

Some recent investigations concerning the primitive chemical action of the galvanic pile, gave me an opportunity of observing a fact interesting equally in a physical and a chemical point of view.

The iron employed in these experiments was wrought iron, iron-plate of the best quality. Iron of this nature put into a solution of potassa and united to the platinum, placed in nitric acid, gives oxygen gas without being oxidised.

It is the same with graphite, platinum, palladium, gold, nickel, cobalt and tin, substituted for iron.

Silver, copper, antimony, bismuth, lead, cadmium, and, which is a remarkable thing, zinc itself, likewise furnish oxygen; but with this disengagement of gas, there is also a perceptible oxidation, an alteration of the metals.

This phenomenon is particularly striking with silver and lead; these two metals are promptly covered with a black layer, (probably formed of peroxide with the silver), and it is only after the formation of this layer that the disengagement of oxygen takes place.

It is not the same with cast iron, which, by chance, I at first employed in my experiments.

It is immediately enveloped in an atmosphere of a beautiful wine-red, which is spread throughout the liquor in thick clouds; the latter in a short time acquires so deep a color that it appears almost black, and allows the beautiful color of Medoc to be perceived only at the edges, looking through a transparent object. The concentration of the solution of potassa is of little importance in this case. I have obtained the phenomenon in all its beauty with a solution of one part of hydrate of potassa, in four parts of distilled water.

If the solution of potassa be set aside and carefully examined, a slight sparkling may be observed in very small bubbles, and at the same time the liquor changes color. It becomes brown red, turbid, and, at the end of a short time, sometimes half an hour, it is completely decolored, forming at the bottom of the vessel a brown deposit.

The electric current does not prevent this change, for it begins when the iron forms the pile with the platinum; but it is produced with much greater rapidity, and even instantaneously, when the solution of potassa is heated to boiling. The red liquor is also completely decolored, in a short time, when a plate of zinc is immersed in it.

I at first endeavored to attribute this phenomenon to the presence of manganese in the iron; but a more attentive examination of the circumstances, and especially a chemical analysis of the brown precipitate, in which I recognised only oxide of iron, convinced me that the coloration of the liquid could arise only from ferric acid, or rather ferrate of potassa.

The formation of ferric acid in these circumstances, is very easily explained; it must be attributed to the predisposing affinity which, under the influence of the electric current, here combines the oxygen, either immediately with the iron, as the presence of the potassa immediately offers to the acid a way of formation, or previously with the potassa, forming first peroxide of potassium, and afterwards ferrate of potassa.*

But the phenomenon is not the less interesting in a double respect; firstly, because there is here formed with great facility

* *Journal de Pharmacie et de Chimie*, May, 1842.

* One fact seems to favor this opinion; it is that ferric acid cannot be obtained with ammonia.

by the galvanic way an acid, which the author of its discovery, M. Frémy, and other chemists, have hitherto been able to obtain only with great difficulty by purely chemical means, and secondly, because it is produced only with cast iron, and not with wrought iron.

This last circumstance is certainly very enigmatical. I have made numerous experiments to find out the reason of this, but all have hitherto proved fruitless. I have never succeeded in preparing ferric acid with wrought iron nor with steel. All sorts of cast iron have not furnished this acid; of four kinds which I tried, only two reproduced the phenomenon.

I at first attributed the difference to the force of the electric current, and I consequently made a comparison between a pile formed of wrought iron and another made with cast iron.

But the comparison gave for the pile of cast iron, with which ferric acid was produced, only a very slight superiority in the force of the current and the electro-motive power, over a pile of wrought iron of equal dimensions.

I was also able, by elongation of the terminal wire, to weaken the current of the pile of cast iron much more than that of the pile of wrought iron, and the former no less continued to form ferric acid, while the latter yielded none.

The great similitude in the force of the current of the two iron piles, of equal dimensions, is interesting in a theoretical point of view; for it shows that it is immaterial for the force of the current (and also for the electro-motive power) that the isolated substances, (in this case the oxygen,) are disengaged in the free state, or are combined with the metals. It contradicts the opinion of Mr. Grove and others, that water is more easily decomposed by the electric current, if the oxygen has an opportunity of combining with the positive metal of the pile. In general, it would afford no more support to that opinion which admits that, in the chemical combination of these bodies there is a great quantity of electricity developed or absorbed.

However, the current requires a certain intensity for the production of ferric acid. I steeped two plates of cast iron in some solution of potassa, and I afterwards connected them with the metals of a simple Grove's battery; but, on account of the dimensions of the system, the current was too weak: only a small quantity of ferric acid was formed in this case. There was, on the contrary, oxygen produced on the positive plate, a thing which I have never observed with wrought iron, and which proves that, in these circumstances, cast iron has a greater tendency to oxydation than wrought iron.

In general, cast iron, especially that which gives ferric acid, is more soluble in the acids than wrought iron, perhaps on account of the mixture of particles of carbon, which may exert a galvanic action on those of the iron, and may thus increase their solubility in the same manner as ordinary zinc, which the mixture of foreign substances renders more soluble than distilled zinc.

On the other hand, the increase in the intensity of the current, at least to a certain extent, is not prejudicial to the formation of ferric acid. If two Grove's piles be connected in a battery, and if the current of this battery be conducted into a solution of potassa, by aid of cast iron plates, ferric acid is obtained very easily, and in abundance.*

I even think this mode of preparation far preferable to that which I at first employed, because there is no necessity for putting the potassa in contact with an earthenware vessel, and because it does not make the former dirty, nor break the vessel, which never fails to happen after a certain time.

This mode of preparation has one slight inconvenience, that of a portion of the ferric acid formed being lost by reduction at the negative plate. But this trifling loss is fully compensated for by the greater quantity of acid which is formed at the positive plate.

I say *slight* loss: this assertion may appear surprising, for it might be thought that as much ferric acid should be decomposed of the negative plate, as is formed at the positive; but it is not the case, (probably because ferric acid is evidently not an electrolyte). There is always disengaged at the negative plate more hydrogen than is separated from the iron: consequently there is more water decomposed than ferric acid.

On the other hand, besides the formation of ferric acid, there is also developed at the positive plate, *in all circumstances*, with a simple pile, or with a battery, *oxygen* in the gaseous form, but more with the battery than with the pile.

It would, therefore, be possible that, by *a sufficient increase* of the intensity of the current, even with cast iron, *oxygen* gas alone,

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* A curious modification of this process consists in taking, instead of two plates of cast iron, one plate of cast iron and another of wrought iron. If they are at first connected with a battery in such a way that the oxygen is conducted to the latter, it is there disengaged under the form of gas. If the places of the two plates be afterwards changed, ferric acid is obtained.

and no *ferric acid*, may be obtained in the last place.

This accessary disengagement of oxygen is vexatious, inasmuch as it prevents the determination of the composition of ferric acid by galvanic means.

It is clear, indeed, that the quantity of oxygen conducted to the positive pole is equivalent to that of the hydrogen developed at the negative pole. Now, if the former were completely combined with the iron, no matter whether temporarily or permanently, the quantity of hydrogen developed might serve for calculating that of the oxygen of the ferric acid; the oxide of iron would give the proportion of iron.

But the liberation of the oxygen, and the uncertainty as to whether only *one* ferric acid is formed do not admit of the employment of this method.

The extreme facility with which the acid is decomposed prevented me from attempting its isolation, or that of its salt. If this isolation be possible, it may probably be effected at a very low temperature.

In conclusion, I may be allowed to express the supposition that ferric acid, which is also formed, as is known, although in very small quantity, at a high temperature, may be found quite formed in nature, for example, forming the coloring matter of the amethyst, in which former analyses, besides silica, have detected iron, with a trace of manganese.

ON THE INSTINCT AND IRRITABILITY OF PLANTS.

BY JOHN S. HILEY, A.B., M.B.

(*Continued from No. XXX.*)

But besides this power which the months and seasons exert over the blossoming of plants, there are other agents whose effects are still very peculiar, and which are brought into operation during the time that the vegetable is arrayed in all its glory. I allude to the influence of light and shade, the sun, touch, and any poisonous and irritating matter. The influence of light and shade will be first discussed. In their effects these are not inferior to the direct action of the sun, notwithstanding it cannot be denied that light, in all its modifications from brightness to shade and darkness, is derived from that body. In the catalogue numerous instances, illustrative of these observations, are furnished, and consequently I must now confine myself to a few general remarks, inviting at the same time the attention of the reader to the more particular cases. In the great families of the Compositæ and Papilionaceæ we noticed repeated examples of the effects of light upon flowers, of which the Daisy, Sunflower, Marigold, Pea, Vetch, &c. are but a few. These are most open during the bright parts of the day, and according as the shade becomes more masked, so is there a proportionate unfolding of their blossoms. Here the light would seem to have the property of developing the colours of these and of other families, for it will be found that flowers most subjected to its influence are arrayed in the brightest dresses, a fact peculiarly exemplified in warm climates. The Ranunculaceæ, &c., furnish other instances of a similar character. The common Veronica Chamædrys, the Arenaria rulera expand their flowers in bright weather only, and were it necessary, and space and time permitted, I might enlarge much more on this head. It is not, however, light alone which operates upon the reproductive organs. The shade, together with certain states of the weather, may, and often does, affect them in a singular manner. Hence such flowers have been denominated, like the leaves, Florestristes and Meteoric flowers.

It is notorious how some of the more ordinary indigenous specimens of this country discover the cloud passing over the sun, and how correctly they can predict the approach of rain. Most Syngenesious and Papilionaceous specimens close during the shade, whilst in several members of other families, the order is reversed, for they open at this time, or as night approaches, and close while the day is the brightest. The Ænothera biennis, and many more, have this character. Gloomy weather and the approach of night are more marked in their effects, because they are more singular. The beautiful Anagallis arvensis constitutes a most extraordinary weather-glass, as do also several of the Stellariæ, particularly the Stellaria media, whose flowers close on the approach of rain. The Convolvulus arvensis, a delicate-looking specimen, can determine the nature of the changing weather with all the correctness of a barometer, for, like the Pimpernel and the Chickweed, it is fully expanded in fine, partly closed in gloomy, and entirely shut in wet states of the weather. The Sonchus oleraceus and the Carlina vulgaris are not less remarkable, and there are few who have not seen this property exemplified in a most correct and curious manner by one or both. The Sonchus also closes at night. The Carlina vulgaris does not merely shut its corolla before rain, but what is perhaps more worthy of attention, the scales of the calyx are hygrometrical, changing their position according to the moisture of the atmo-

sphere. The fact that several plants open during the day, and expand at night, and *vice versâ*, has occasioned them to be divided into diurnal and nocturnal flowers, and many which are influenced so materially by light and shade, are merely of this description. This order of things has now and then been reversed by the experiments of botanists. Thus a flower has been made to unfold during the night by the aid of a bright lamp-light which usually remained closed at this season; and flowers also have been induced to shut their corollas by bringing them into darkened apartments. Add to this, we have instances of the effect of different states of the atmosphere upon flowers which ought not to be unnoticed. I allude to the Calendula pluvialis and Campanula glomerata. When the sky is covered with clouds, or when a storm is impending, these close their corollas completely. On the other hand, the Sonchus Sabiricus never opens its flowers, but when the weather is misty, and the atmosphere loaded with vapours and clouds. Formerly much greater attention was paid by the rustic to Flora's weather-glass, his occupations being regulated by the expanding and shutting of certain flowers; and the traveller more than once has quickened his pace when he perceived by the road-side the contracting flowers of the Pimpernel and the Chickweed. The effect of the sun is analogous, but superior to that of light. By his beams the Compositæ are much influenced. The great annual Sunflower, one of this large family, turns its face to that quarter of the heavens in which the sun may happen to be placed. In the morning the flower is directed towards the East, at noon to the South and to the zenith, and in the evening towards the West. The Dandelion and the Apargia autumnalis expand their bosoms to the sun, as do also the members of several other natural orders. Here I would make especial mention of the Ranunculaceæ, extensive fields of whose elegant flowers are often observed facing the rising and setting sun. The Nymphæa alba, the Convolvulus Soldanella, and many others, unfold their flowers in the sunshine and middle of the day.

It now becomes my duty to notice the effects of touch and irritating gases, &c. upon flowers, and to show in what a wonderful and useful manner the instinct and irritability of plants is occasionally brought into play, not only for the purpose of promoting the increase and growth of different specimens, but also to the end that various consequences which would prove highly injurious to the plant itself may be avoided. The Flora of this country furnishes us with several instances of irritability from touch, and I need not go further than to the Berberis vulgaris or Common Barberry. This, however, is a remarkable case, and its irritability doubtless contrived for a beneficial end. The six stamens which surround the pistil in this flower are each gifted with a singular degree of irritability in one small spot on the inner side near the base. When this is touched with the point of a penknife, with a pin, or by an insect, the stamen connected with it suddenly contracts, and shoots with some degree of force towards the stigma. It must be observed that all the stamina are spread away from the stigma. The object of this singular motion is obviously for the purpose of projecting the pollen upon the female part of the flower, and thus causing proper reproduction, nor does it occur until by the visit of some insect, &c. when the pollen is ripe, the spot in question is irritated. This contraction, in which we have a beautiful instinctive provision for the fulfilment of a necessary end, very nearly resembles muscular contraction in animals. When touched, the flowers, and even other parts of the Cynoglossum officinale, emit a pungent and nauseous scent, like that of mice, or, as some say, the urine of dogs, indicative of a marked degree of irritability. Neither ought the Verbascum pulverulentum to be passed over unnoticed. When the stem of this specimen is smartly struck three or four times with a stick, all the flowers then open will, in a few minutes, throw off their corollas, the calyx closing round the germen, so that after eight or ten minutes, none will remain on the plant. What can be the object of such a singular result it is very difficult to explain. In the Cistus Helianthemum, whose flowers expand in the sunshine only, if the stamens be touched by any foreign body, they spread slowly, and ultimately lie down upon the petals. We are furnished with another remarkable case of irritability in the Lactuca virosa. But here the effect is not limited to the flowers, inasmuch as it extends to all the green parts of the plant. On the slightest touch, the juice springs out suddenly in large drops from the calyx and tender leaves, evincing an excitability of a very peculiar kind. The stings of the nettle may likewise be mentioned under this head, each of which is supplied with a bag of liquid poison at its base. This liquor, by a gentle pressure of the hand, causing the bristles to perforate the cuticle, is at the same moment transmitted to the skin, thus occasioning a great degree of irritation. The effects of irritating gases, &c. on plants generally is too well known to

need any comment. If persevered in, they destroy the plant, as they would animals, by abstracting its moisture, or by attacking it throughout its whole system at once.

There are, however, another set of motions in plants which cannot be classed under the head of irritability, but rather deserve the epithet of instinct; and which do not result so much from the operation of foreign causes, as touch, &c., as they arise from actions going forwards in the vegetables themselves. These motions are particularly connected with the functions of the stamens and pistils, and illustrate in a prominent manner the agents and means which nature has brought into operation for the purpose of promoting fecundation in the vegetable world. Nothing, probably, in the range of their economy is more striking or beautiful, or evinces stronger evidence of design. The essential nature of these provisions will be seen at once; for vegetables being destitute of the power of locomotion, and irrevocably fixed to the place which has given them birth, it was not only desirable that the sexual organs should be united in the same individuals, and generally in the same flower, but also that some contrivance should be provided by which the pollen could, with the utmost certainty, reach the stigma. Facilities should be afforded in both organs for the furtherance of this end, which required a larger share of carefulness and skill.

I need scarcely remind the reader, that the stamens and pistils of flowers have, from the most remote antiquity, been considered as of great importance in perfecting the fruit. This fact was originally learnt from the Date Palm. That beautiful exotic, cultivated from time immemorial, with great care in the more temperate regions of the globe, bears barren and fertile flowers on separate trees. In fact it belongs to the diœcious class of the Linnæan system. The ancient Greeks soon discovered, that in order to have abundant and well-flavored fruit, it was expedient to plant both trees in the neighbourhood of each other, or to bring the barren blossoms to those which were to yield fruit. This artificial mode of fecundation was also known to the ancient Egyptians, and it is at present practised in other parts of Africa, where the Date Palm is abundantly cultivated. At the time of flowering, it is customary to climb to the summit of the female plants, and to shake over them panicles of the male flowers, in order to cover them with pollen. M. Delile tells us, that during the recent campaigns in Egypt, from the constant hostilities of both parties, this practice could not be resorted to, and that therefore the Date harvest completely failed. The same has been practised for ages, and is continued to this very day in the Levant, upon the Pistacia and the Fig. Again, there had been long in the Botanic Garden of Berlin a diœcious female plant of the Chamærops humilis, which flowered annually, but never produced fruit. Gleditsch procured from Carlsruhe panicles of the male flowers, and following the mode adopted from time immemorial in Egypt and Greece, by shaking them over the female flowers, perfect fruit was afterwards obtained. This experiment has been often repeated. Remarkable, however, as it may appear, it is only what might have been expected. We must look to the difficulties which nature will overcome for the purpose of effecting the fecundation of diœcious plants, and here we discover a wonderful instance of instinct. The enormous distances at which it has been accomplished, is the most singular, and of this we have several well authenticated examples. In the Garden of Plants, at Paris, two trees of the female Pistacia had been many years cultivated, and were annually loaded with flowers, but never produced fruit. How great was the astonishment of Bernard de Jussieu, when one year he observed the fruit setting in both, and come to maturity. He immediately conjectured that there must be in Paris, or near it, a male plant of the same species, bearing flowers. On inquiry, he found that at this period a male Pistacia had flowered in the nursery at Chartreux, near Luxembourg. In this case, the pollen, conveyed by the wind, passed over the houses of a part of Paris to fecundate the female flowers. The Valisneria spiralis, a diœcious plant, presents another instance of what nature will accomplish to promote fruitfulness. The phenomenon in this case is most interesting. The Valisneria spiralis grows in the canal at Languedoc, and in the rivulets about Beaucaire, and is there attached at the bottom of the water, and is wholly submerged. The male and female plants grow together. The female flowers, borne upon peduncles of about two or three feet long, which are twisted spirally so as to resemble a corkscrew, appear at the surface of the water for the purpose of expanding. The male flowers, on the contrary, are enclosed, many together, in a membranous spathe, which is borne upon a very short peduncle. When the period of fecundation arrives, they burst the spathe, are detached from their support and from the plant they belong to, and come to the surface of the water in order to expand, and to fecundate the female flowers. Soon after, the latter, by the retraction of their spiral peduncles, sink again below the surface, where they arrive at perfect maturity.

ON A PROCESS OF ANALYSIS APPLICABLE TO THE SALTS OF BARYTA, POTASSA, AND SODA, WITH ORGANIC ACIDS.*

BY M. GAULTIER DE CLAUBRY.

The analysis of a salt of silver or lead with an organic acid, does not present any particular difficulty, the base being incapable of retaining carbonic acid at a high temperature.

The case is quite otherwise with the salts of potassa, soda, and baryta, the carbonates of these bases being formed at a red heat; and those of the two first resisting the most elevated temperature that we can produce.

Hence it results that, in analysing a salt of potassa, soda, or baryta, with an organic acid, to ascertain the proportion of carbon, it is necessary to calculate the quantity of carbonic acid which the base retains.

In a work on a series of new salts with which Dr. Brame and I are occupied, and whose principal results we have deposited with the Academy, not being able to analyse the salts of lead or silver, on account of their instability, we were constrained to operate on those of potassa and soda, which presented anomalies that I could attribute only to the state of the base after the operation, and I was then led to ascertain whether the carbonic acid was not more or less retained in it, according to the temperature and duration of the operation, and the means of avoiding this cause of error.

The employment of a fixed acid very naturally presented itself to my mind, and among those which might be used, I at first adopted boracic acid, the effect of which I have pointed out in a packet, marked No. 1, deposited with the Academy on the 11th of October, 1841. †

This acid, however, employed in a proportion rather exceeding the equivalent of the organic acid, presents inconveniences which have induced me to relinquish its use; especially the hardness of the product and the impediment which this physical state sometimes opposes to the complete combustion of the carbon.

Stannic acid did not furnish more accurate results.

Bichromate of potassa may be employed for salts of potassa, but it does not appear to give uniformly exact results.

Borate or sulphate of copper, may also be used; but in the first case the inconveniences which attend the use of boracic acid are again met with, and in the second, the operation is rendered more complex by the presence of sulphurous acid gas.

With the exception of the latter salt, it appeared to me that none of the bodies whose actions I tried, could completely fulfil the object which I had in view; because the acids were not sufficiently powerful, or were too easily decomposed at the temperature required by the operation, and under the deoxidising of the hydrogen and carbon, or indeed as silicic acid especially, because they gave rise to products too compact to be easily penetrated by the gases arising from the operation.

Setting out, then, from the data that the alkaline sulphates may furnish, when they are decomposed by heat, poly- or monosulphuret, according to the temperature to which the mixture has been brought, the acid being more easily decomposable than the oxide, I thought that phosphate of copper, which, in ultimate analysis, yielded phosphorus by the action of hydrogen and carbon, might, in given circumstances, furnish protoxide of copper, or copper and phosphoric acid; a fact which is realised in the same conditions in which the bodies in an organic analysis are placed, and of which I availed myself for disengaging all the carbonic acid in the combustion of a salt of baryta, potassa, or soda.

Thus, when phosphate of copper is submitted to the action of an excess of hydrogen or carbon, at a high temperature, it is entirely converted into phosphuret; but if an excess of an organic substance be mixed with it, and if the temperature be raised, the copper is more or less completely reduced, and the residue contains free phosphoric acid.

It then became, if not certain, at least extremely probable, that by employing phosphate of copper for analysing a salt of baryta, potassa, or soda, with an organic acid, the base could not retain the carbonic acid. This new fact was completely verified; but the physical state of the salt has great influence on the results obtained, and analysis may fail when it is employed in a state of great adhesion, its decomposition being then capable of taking place by furnishing the phosphuret.

From this it results that it is not right to endeavour to determine at the same time, when this salt is used, the hydrogen and the carbon of an organic substance, because, in order to have the phosphate of copper quite free from water, it requires to be too strongly heated, in which case it acquires too much cohesion and is too difficultly decomposed.

The best state in which it can be mixed

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* *Comptes Rendus*, No. 18, May 3, 1842.

† The packet, No. 2, contains results foreign to this work, and I wish it still to be kept.

with the organic salt, is that which is presented when it is heated in a capsule until it becomes of a yellowish green; before this, it often retains water which occasions projections which are dangerous to the operator; afterwards it acquires too much cohesion.

The organic salt is mixed with at least five or six times its weight of phosphate; the pestle and mortar are several times cleaned with a fresh quantity of salt, and afterwards with oxide of copper, and the operation is conducted in the ordinary way.

It proceeds with much facility, and requires only, especially for the salts of baryta, a more elevated temperature at the end of the operation than when the salts of lead or silver are analysed.

The mixture of phosphate and of organic salt almost constantly fuses, and leaves a considerable vacuum in the tube.

Oxidised copper turnings did not appear to be the best oxide for this kind of analysis.

Towards the end of the operation, I always drive a current of oxygen into the tube, either by means of a gasometer, or by adapting to the apparatus a retort containing chlorate of potassa mixed with oxide of copper, and I placed after the potassa tube, a tube of chloride of calcium, whose weight increased from one to five or six millegrammes.

ON CERTAIN COMBINATIONS OF SILVER.

BY REUBEN PHILLIPS.

It is well known that when the moist chloride of silver is exposed to the action of solar light, it becomes black, which is usually explained by saying that the water is decomposed, the equivalent of hydrogen uniting to the chlorine, producing hydrochloric acid, while the oxygen combines with the silver.

But there are many objections to this theory; hydrochloric acid is indeed formed, but no oxide, as may be proved by adding nitric acid, which does not affect the color of it. It is soluble in a concentrated solution of ammonia, but far less so in dilute solutions; the dark substance can thus be separated from the common chloride. A solution of iodide of potassium renders it much darker at first, but slowly forms the white iodide. This darkened chloride, when moist, is entirely reduced to the metallic state by a piece of zinc.

Supposing, from what has been above related, that the dark substance was silver combined with an inferior proportion of chlorine, I tried to subtract chloride from the common chlorine, and at length succeeded in the following manner:—

Let the precipitated chloride of silver be boiled in a concentrated solution of potassa for half an hour, adding more water as it evaporates; wash the chloride with water; add an excess of nitric acid, and again wash with water, and afterwards with dilute ammonia. There remains a brown powder, which possesses all the qualities of that procurable by light.

An iodide of a dark olive hue may be obtained by pouring on this powder a dilute solution of the iodide of potassium, and, after a few minutes have elapsed, washing it well with water. The iodide of potassium in this process becomes converted into the chloride of potassium.

Nitric acid acts on this iodide, forming the white iodide and nitrate of silver. Nitro-hydrochloric acid acts on it powerfully; iodine is separated. When moist, it is rapidly reduced by zinc, as is also the common iodide. It is slowly transformed, into the white iodide, by a solution of iodide of potassium. This dark iodide absorbs iodine with avidity.

The white iodide of silver is unalterable by light, but it becomes highly sensitive to this agent when moistened with a solution of nitrate of silver. The same blackening effect is produced, when a solution of nitrate of silver is boiled with the iodide; this experiment succeeds better if some oxide of silver be added.

Iodide of potassium in solution acts on metallic silver; the oxygen is probably derived from the air. The white iodide of silver is soluble in a concentrated solution of iodide of potassium; it is precipitated by water. It fuses at a dull-red heat, and acquires a red color, but regains its original color as it cools.

These facts were made out as far back as October 1840. I should have finished the work long ago had I not learnt from a chemist that it was previously known; but as no system of chemistry which I have seen mentions one word about it, this affords to me a strong presumption that a description of these bodies has not hitherto been published, which induces me to make known this imperfect account of them.

PURIFICATION OF SULPHURIC ACID WITH ONE ATOM OF WATER, FOR ACCURATE ANALYSES AND MEDICO-LEGAL INVESTIGATIONS.*

BY M. V. A. JACQUELAIN.

I mix sulphuric acid, purified by sulphur distilled with a small quantity of an aqueous solution of chlorine, and, in order to drive

* *Comptes Rendus*, No. 18, May 3, 1842.

off the whole of the latter, as well as the hydrochloric acid produced under these circumstances, I boil the mixture for a few minutes. When the acid has been submitted to these tests, it is completely freed from nitrous, sulphurous, and hydrochloric acids.

The following experiments are confirmatory of my statement:—

100 gr. of sulphuric acid diluted in 500 gr. of distilled water did not lose limpidity by the addition of nitrate of silver.

100 gr. of distilled zinc were converted into nitrate, and the solution, sufficiently diluted with water, was not rendered turbid by chloride of barium.

This done, I treated 75·95 of the same zinc by 200^{gr} of sulphuric acid (purified by sulphur and chlorine). I used a washing apparatus composed in a single piece of 7 bulbs communicating together, above and below, by curvatures very near to each other. The apparatus contained a solution of chloride of gold; and the hydrogen gas, after having traversed the solution in all these bulbs, had not reduced the smallest portion of the salt of gold.

To give to this result the greatest possible certainty, I repeated the experiment with an equal quantity of the same zinc, and 200^{gr} of sulphuric acid purified by sulphur alone; scarcely had 60^{cc} of hydrogen gas traversed the chloride of gold of my apparatus, when I saw formed in the first bulb a black precipitate of sulphuret of gold, reducible at the end of a day or two into metallic gold, on account of the excess of chloride of gold. At the end of the operation, I took to pieces the apparatus, then testing the solution of chloride of gold by chloride of barium, I obtained insoluble sulphate of baryta, notwithstanding the addition of a little hydrochloric acid.

From all this we may conclude, that there existed neither sulphur in the zinc, nor sulphurous nor hydrochloric acid in the sulphuric acid purified by means of sulphur and chlorine which I used.

I will now give a summary of the conclusions which flow from the very accurate experiments detailed in the subjoined table:—

1st. Protosulphate of iron in solution is a test of extreme sensibility for detecting, in sulphuric acid, traces of nitrogenous acids, a fact which we owe to the observation of M. Desbassayns; but it is not a special means of quantitative analysis, seeing that nitric acid, nitrous acids, and the nitrates act in the same manner in the presence of sulphuric acid and of the protosulphate of iron. Moreover, the combination of binoxide of nitrogen and protosulphate of iron with which Mr. Peligot is occupied, also possesses characteristics of coloration, when perfectly pure sulphuric acid is gradually mixed with it.

10000 parts of $SO^3 H^2 O$ and 7 of		Ponitric or nitrous acid, or its equivalent of a nitrate.		Intense wine red by protosulphate of iron. Intense red with narcotine.
1000000	+ 8	...	...	Red with the protosulphate. Amber yellow with narcotine.
1000000	+ 7	...	...	Weak red with the protosulphate. Amber yellow with narcotine.

2nd. On the contrary, when it is required to detect appreciable quantities of free or combined nitrogenous acids, the sulphuric acid may be replaced in preference by hydrochloric acid and gold leaf, boiled with the suspected liquor, with the exception of afterwards making protochloride of tin or sulphate of indigo to intervene. Good indications are also arrived at, when copper turnings are gently heated in contact with concentrated sulphuric acid, whose action is aided by a small quantity of water with which the substance to be examined has been mixed.

If it should happen that the nitrous vapours, by this process, do not become very apparent, it is only necessary to direct them, by an aspiratory apparatus, into concentrated sulphuric acid, containing a few drops of protosulphate of iron in solution.

3rd. As to the sulphate of copper, it does not exert, in the same conditions as sulphate of iron, any action, even at the expiration of twelve hours, either on the binoxide of nitrogen, or on the oxyacids of nitrogen, or on their saline compounds; if M. Desbassayns observed any coloration, it was only because his sulphate of copper was not free from iron, or because the bell glass filled with binoxide of nitrogen had not been freed from air by washing, and because its solutions had not been deprived of air by a suitable elevation of temperature.

4th. The sensibility of protosulphate with respect to sulphuric acid mixed with nitric or nitrous acid, or metallic nitrates, detects $\frac{1}{1000000}$ of those compounds.

1000000 parts of $SO^3 H^2 O$ and 4 of	Nitric or nitrous acid, or its equivalent of a nitrate.			Perceptible red coloration with the protosulphate. Amber yellow with narcotine.
1000000	+ 3	...	...	Very weak rose color with the protosulphate. Amber yellow by narcotine.
1000000	+ 2	...	...	Scarcely visible rose color. Amber yellow by narcotine.
5000000	+ 3	...	...	Coloration indistinct. Amber yellow by narcotine.

At the first inspection of this table one is astonished, and with reason, at not seeing narcotine present graduated colorations as the proportions of nitrogenous acids were smaller.

It is to be remarked besides, that the shades of the liquids treated by narcotine, became considerably deeper in a few hours, whilst those of the liquids tested with protosulphate of iron became lighter.

Notwithstanding this difference of results between the two liquids, it is easy to make a choice by means of the following experiment. If we take sulphuric acid purified by sulphur and chlorine, and if we add to it, with the precautions indicated further on, some protosulphate of iron, nothing is produced; whilst, by dropping powdered narcotine on the surface of a similar quantity of sulphuric acid covered with a few drops of water, and slowly mixing them, an amber yellow tint is obtained, which passes to orange red in about an hour.

This reagent must therefore be abandoned, and protosulphate of iron retained.

The following precautions are best calculated to insure success in the experiment:—

Operate on at least 50 grammes of sulphuric acid, pour on the surface 0·5 gr. of distilled water, allow the heat produced by the contact of the two liquids to dissipate, then add ten drops of protosulphate of iron, and mix slowly, in order to avoid an elevation of temperature capable of destroying the violet compound formed.

ON THE COMPOUNDS OF THE METALLIC BROMIDES WITH AMMONIA.*

BY M. RAMMELSBERG.

In studying these compounds, the author employed liquid ammonia, or else put the anhydrous bromides in contact with dry ammoniacal gas.

The resemblance which bromine bears to chlorine, which is much greater than that of either of them to iodine, is especially manifested in its combinations with the metals; for not only do the bromides resemble, in a high degree, in their external properties, the corresponding chlorides, and contain, in many cases, the same atomic number of water of crystallisation; but the resemblance of the two classes of bodies also extends to their combinations with ammonia; the author has indeed found that the bromides almost always combine with the latter in the same proportion as that which H. Rose's experiments have sufficiently proved as regards the chlorides, and these ammoniacal compounds also present in all cases a complete similitude in their physical and chemical properties.

It is thus that bromide and chloride of barium crystallise with two atoms of water of crystallisation and are completely isomorphous. Bromide and chloride of strontium each take six atoms of water.

Two atoms of bromide of strontium combine with one atom of ammonia; and one atom of bromide of zinc combines, in the humid way, with two atoms of ammonia. The bromide of cadmium takes by the humid way two atoms, and by the dry way four atoms; bromide of nickel absorbs in both cases six atoms, precisely as much as chloride of nickel. Bromide of cobalt in the anhydrous state absorbs six atoms of ammonia, and the bromide of copper absorbs three atoms by the humid way, and five by the dry way. Bromide of mercury absorbs one atom of ammoniacal gas, that is to say, as much as the chloride; the chloride and the bromide of the same metal absorb the same quantity, namely, one atom.

The bromide of barium, and the corresponding chloride do not combine with ammonia, and it has not been possible to

* *Journal für Praktische Chemie.*

combine it with the bromides of lead and silver; for although the latter dissolves in liquid ammonia, it crystallises from that solution without being combined.

The basic compound of bromide of mercury also contains three atoms of oxide, like the corresponding compound of chlorine.

ON THE ACTION OF METALS AND SOME OF THEIR COMBINATIONS ON AMMONIA, AT A HIGH TEMPERATURE.*

BY M. SCHRŒTTER.

The author has proved, by new analyses, that ammonia, in passing over copper heated to redness, is decomposed into its elements, without the metal increasing in weight, and consequently without its retaining ammonia, as was supposed. Besides, ammonia undergoes the same kind of decomposition, in passing through a porcelain tube filled with fragments of porcelain, and heated to redness.

The nitruret of copper, which is obtained by making ammonia act on oxide of copper, begins to be formed towards 250° C., whilst the reduction of the oxide of copper by hydrogen is manifested only at the temperature of boiling linseed oil. This nitruret is a black powder, and is decomposed at 300° C., disengaging nitrogen, and leaving copper for residue. Its decomposition is accompanied by a red light: put in contact with sulphuric acid, it is converted into nitrogen and metallic copper. The reaction is much more quick with nitric acid, which attacks the metal. With muriatic acid, it gives bichloride of copper and sal ammoniac; with chlorine, it forms chloride of copper, and nitrogen is set free; with ammonia, there is no disengagement of gas, but a blue color is gradually produced.

This body does not contain hydrogen; it is represented by $N^2 Cu^3$.

The author has also obtained the nitruret of chromium.

ON THE ASH WHICH REMAINS AFTER THE COMBUSTION OF THE DIAMOND.†

BY M. PETZHOLD.

MM. Erdmann and Marchand, in their investigations relative to the atomic weight of carbon by the combustion of the diamond, have obtained, like MM. Dumas and Stass, very slight and scarcely appreciable residues from the fragments of diamond, formed of a reddish substance, whose particles sometimes present a shining surface, and which seemed to be contained quite formed in the mineral submitted to combustion.

From a more attentive examination M. Petzhold found that these residues are formed in general of an infinite number of scales, of very small bracteæ, among which it is extremely uncommon to find small fragments harder or more round. By analysis before the blowpipe, he ascertained that they were formed of silica with traces of iron.

NOTE ON OZONE.*

Professor Schönbein, in his paper on *ozone*,† regrets that we have not been able to gather a sufficient number of observations, and especially those of philosophers, upon the nature of the odor developed by a stroke of lightning. The term sulphuric or phosphoric, which is commonly applied to it, by those who chance to have been present, originates, in his opinion, in "vulgar prejudice," which gives these familiar names to every pungent odor. "If," says he, "the gaseous body disengaged by the lightning penetrates the nose of the observer in a concentrated state, he will imagine he perceives an odor of phosphorus." It is the Professor's opinion that the odor is really that exhibited in the vicinity of electrical machines, when in good action; and that the odorous principle is a gaseous body, developed by electric action, and to which he has given the name of *ozone*. Prepossessed, rather than otherwise, in favor of the *electrical character* of this odor, I made very careful inquiries; and when the attendants of the church told me that the flash was attended by a very strong smell of sulphur, I began to think with Schönbein, that the odor of ozone, being more akin to this than to any other they know, was at once described as sulphurous. But on inquiring of gentlemen who were present, and who were well acquainted with the electrical odor, I find that it was "*decidedly* sulphurous, and *not* like that which follows common electricity. A portion of stone, which was grooved out by the lightning, retained a strong sulphurous smell even until the time of evening service. It was passed from hand to hand, *on that very account*, and caused many remarks at the time. The

* *Annales de Chimie et de Physique.*

† *Annalen der Chemie und Pharmacie*, No. 3, March, 1842.

* From Mr. C. V. Walker's "*Effects of a Lightning Flash*," &c.

† Vide *Bibliot. Univ.* No. 56, p. 356, &c., Aug. 1840.

smell at the bottom of the tower, within the porch, where the lightning descended, was precisely the same, though in a lesser degree." Whether, therefore, this odor proceeded from ozone, highly concentrated, or was developed by the mere heating power of the lightning upon some of the substances among which it passed, I am not prepared to say; but that it did not possess the peculiar electrical character is very evident.

ON THE BICHLORIDES OF HYDROGEN AND LEAD.*

BY M. MILLON.

M. MILLON obtains bichloride of hydrogen by means of the following reaction :—

If concentrated hydrochloric acid be poured on pure oxide of lead, at the ordinary temperature, there is an abundant disengagement of chlorine, and chloride of lead is deposited: but if the acid be placed in a glass tube with thin sides immersed in a frigorific mixture of ice and salt, and if the oxide be slowly and gradually added, chlorine is no longer disengaged; the liquor assumes a deep yellow tint, and an abundant deposit of chloride of lead is formed.

This liquor, left to itself, disengages chlorine for several days, gives with oxalic acid a powerful liberation of carbonic acid, and, treated by a metal, such as mercury or zinc, it makes it pass to the state of chloride, reproducing hydrochloric acid.

M. Millon has established the composition of bichloride of hydrogen, and the formula of the reaction of which it is the product, proportioning the quantity of pure oxide and hydrochloric acid employed.

The following is the formula :—

$$3HCl + PBO^2 = PBCl + 2HO + HCl^2.$$

The formation of the compound HCl^2, and its constitution, are confirmed by the quantity of arsenious acid necessary for destroying the decoloring power of a given quantity of hydrochloric acid which the pure oxide has caused to pass to the state of bichloride; it is thus found that one third of the acid has become decolorant.

M. Millon tried to separate the bichloride of hydrogen from the water in which it was dissolved: all his attempts were unavailing.

There exists also in the solution of bichloride of hydrogen a certain quantity of lead which is separated from the liquor in the state of binoxide, when a great quantity of water is added. The author thinks that lead exists there in a peculiar state of combination, constituting a bichloride hitherto unknown: however, he has not yet been able to wash and dry this new compound, whose elements appear to be very unstable.

* *Recueil des Mémoires de Médecine, de Chirurgie et de Pharmacie Militaire.*

REVIEWS :—

THE STRUCTURE AND DISTRIBUTION OF CORAL REEFS: *being the First Part of the Geology of the Voyage of the Beagle, under the command of Capt. Fitzroy, R.N., during the Years* 1832 *to* 1836. By CHARLES DARWIN, M.A., F.R.S., F.G.S., Naturalist to the Expedition. Published with the Approval of the Lords Commissioners of Her Majesty's Treasury. London: Smith, Elder, and Co., Cornhill. 8vo. pp. 214.

A PERUSAL of this valuable work will immediately confirm the propriety of placing a notice of it in the Chemistry division of our work, treating as it does of the grand phenomena involved in the mighty laboratory of Nature.

The opinions of previous writers on the subject of coral formations, however plausible, and however much entitled to weight from the scientific attainments of their propounders, must, for several reasons, be deferred for those laid down by Mr. Darwin. Firstly, he has had the advantage of the personal survey of the greatest number of localities, with a specific object in view; in the second place, he has benefited by the review of the labor of his predecessors in this path of research, in addition to valuable communications from contemporary living scientific observers; and lastly, his conclusions are founded on such bases as to leave the mind in more repose.

Mr. Darwin's pages are not *extractable*. His arguments are so logical and so consequential, that an extract would be unsatisfactory and even useless, if given as an attempt to explain his theory. Chap. I. treats of Atolls or Lagoon Islands; II. of Barrier Reefs; III. of Fringing or Shore Reefs; IV. of the Distribution and Growth of Coral Reefs; V. of his Theory of the Formation of the different Classes of Coral Reefs; VI. of the Distribution of Coral Reefs with Reference to the Theory of their Formation :—And the Appendix contains a detailed description of the Reefs and Islands in his coloured Map. There are three Illustrative Maps admirably explaining his theory; besides sectional wood engravings.

It is with considerable diffidence, and in the fear of doing injustice, that we venture to give an interesting extract; but we trust that its merits will be our excuse. It is from the sixth and concluding chapter :—

"It is very remarkable, on reviewing these details, to observe in how many in-

stances fringing-reefs round the shores, have coincided with the existence on the land of upraised organic remains, which seem, from evidence more or less satisfactory, to belong to a late tertiary period. It may, however, be objected, that similar proofs of elevation, perhaps, occur on the coasts colored blue in our map: but this certainly is not the case, with the few following and doubtful exceptions.

"The entire area of the Red Sea appears to have been upraised within a modern period; nevertheless I have been compelled, (though on unsatisfactory evidence, as given in the Appendix,) to class the reefs in the middle part, as barrier-reefs; should, however, the statements prove accurate of the less height of the tertiary beds in this middle part, compared with the northern and southern districts, we might well suspect that it had subsided subsequently to the general elevation by which the whole area has been upraised. Several authors have stated that they have observed shells and corals high up on the mountains of the Society Islands,—a group encircled by barrier-reefs, and, therefore, supposed to have subsided: at Tahiti, Mr. Stutchbury found on the apex of one of the highest mountains, between 5000 and 7000 feet above the level of the sea, 'a distinct and regular stratum of semi-fossil coral.' At Tahiti, however, other naturalists, as well as myself, have searched in vain at a low level near the coast, for upraised shells or masses of coral-reef, where if present they could hardly have been overlooked. From this fact, I concluded that probably the organic remains strewed high up on the surface of the land, had originally been embedded in the volcanic strata, and had subsequently been washed out by the rain. I have since heard from the Rev. W. Ellis, that the remains which he met with, were (as he believes) interstratified with an argillaceous tuff; this likewise was the case with the shells observed by the Rev. D. Tyerman at Huaheine. These remains have not been specifically examined; they may, therefore, and especially the stratum observed by Mr. Stutchbury at an immense height, be contemporaneous with the first formation of the Society Islands, and be of any degree of antiquity: or they may have been deposited at some subsequent, but probably not very recent period of elevation; for if the period had been recent, the entire surface of the coast-land of these islands, where the reefs are so extensive, would have been coated with upraised coral, which certainly is not the case. Two of the Harvey, or Cook Islands, namely, Aitutaki and Manouai, are encircled by reefs, which extend so far from the land, that I have colored them blue, although with much hesitation, as the space within the reef is shallow, and the outline of the land is not abrupt. These two islands consist of coral-rock; but I have no evidence of their recent elevation, besides the improbability of Mangaia, a fringed island in the same group, (but distant 170 miles,) having retained its nearly perfect atoll-like structure, during any immense lapse of time after its upheaval. The Red Sea, therefore, is the only area in which we have clear proofs of the recent elevation of a district, which, by our theory, (although the barrier-reefs are there not well characterised,) has lately subsided. But we have no reason to be surprised at oscillations of level of this kind having occasionally taken place. There can be scarcely any doubt that Savage, Aurora, and Mangaia Islands, and several of the islands in the Friendly group, existed originally as atolls, and these have undoubtedly since been upraised to some height above the level of the sea; so that by our theory, there has here, also, been an oscillation of level,—elevation having succeeded subsidence, instead of, as in the middle part of the Red Sea and at the Harvey Islands, subsidence having probably succeeded recent elevation.

"It is an interesting fact, that Fais, which, from its composition, form, height, and situation at the western end of the Caroline Archipelago, one is strongly induced to believe existed before its upheaval as an atoll, lies exactly in the prolongation of the curved line of the Mariana group, which we know to be a line of recent elevation. I may add, that Elizabeth Island, in the southern part of the Low Archipelago, which seems to have had the same kind of origin as Fais, lies near Pitcairn Island, the only one in this part of the ocean which is high, and at the same time not surrounded by an encircling barrier-reef.

"*On the Absence of active Volcanos in the Areas of Subsidence, and on their frequent Presence in the Areas of Elevation.*—Before making some concluding remarks on the relations of the spaces colored blue and red, it will be convenient to consider the position on our map of the volcanos historically known to have been in action. It is impossible not to be struck, first with the absence of volcanos in the great areas of subsidence tinted pale and dark blue,—namely, in the central parts of the Indian Ocean, in the China Sea, in the sea between the barriers of Australia and New Caledonia, in the Caroline, Marshall, Gilbert, and Low Archipelagos; and, secondly, with the coincidence of the principal volcanic chains

with the parts colored red, which indicates the presence of fringing-reefs; and, as we have just seen, the presence in most cases of upraised organic remains of a modern date. I may here remark that the reefs were all colored before the volcanos were added to the map, or indeed before I knew of the existence of several of them.

"The volcano in Torres Strait, at the northern point of Australia, is that which lies nearest to a large subsiding area, although situated 125 miles within the outer margin of the actual barrief-reef. The Great Comoro Island, which probably contains a volcano, is only twenty miles distant from the barrier-reef of Mohila; Ambil volcano, in the Philippines, is distant only a little more than sixty miles from the atoll-formed Appoo reef: and there are two other volcanos in the map within ninety miles of circles colored blue. These few cases, which thus offer partial exceptions to the rule, of volcanos being placed remote from the areas of subsidence, lie either near single and isolated atolls, or near small groups of encircled islands; and these by our theory can have, in few instances, subsided to the same amount in depth or area, as groups of atolls. There is not one active volcano within several hundred miles of an archipelago, or even a small group of atolls. It is, therefore, a striking fact, that in the Friendly Archipelago, which owes its origin to the elevation of a group of atolls, two volcanos, and perhaps others, are known to be in action: on the other hand, on several of the encircled islands in the Pacific, supposed by our theory to have subsided, there are old craters and streams of lava, which show the effects of past and ancient eruptions. In these cases, it would appear as if the volcanos had come into action, and had become extinguished on the same spots, according as the elevating or subsiding movements prevailed.

"There are some other coasts on the map, where volcanos in a state of action concur with proofs of recent elevation, besides those colored red from being fringed by coral-reefs. Thus I hope to show in a future volume, that nearly the whole line of the west coast of South America, which forms the greatest volcanic chain in the world, from near the equator for a space of between 2000 and 3000 miles southward, has undergone an upward movement during a late geological period. The islands on the north-western shores of the Pacific, which form the second greatest volcanic chain, are very imperfectly known; but Luzon, in the Philippines, and the Loo Choo Islands, have been recently elevated; and at Kamtschatka there are extensive tertiary beds of modern date. Evidence of the same nature, but not very satisfactory, may be detected in northern New Zealand, where there are two volcanos. The co-existence in other parts of the world of active volcanos, with upraised beds of a modern tertiary origin, will occur to every geologist.* Nevertheless, until it could be shown that volcanos were inactive, or did not exist in subsiding areas, the conclusion that their distribution depended on the nature of the subterranean movements in progress, would have been hazardous. But now, viewing the appended map, it may, I think, be considered as almost established, that volcanos are often (not necessarily always) present in those areas where the subterranean motive power has lately forced or is now forcing outwards the crust of the earth, but that they are invariably absent in those, where the surface has lately subsided or is still subsiding.

"*On the Relations of the Areas of Subsidence and Elevation.*—The immense surfaces on the map, which, both by our theory and by the plain evidence of upraised marine remains, have undergone a change of level either downwards or upwards during a late period, is a most remarkable fact. The existence of continents shows that the areas have been immense, which at some period have been upraised. In South America we may feel sure, and on the north-western shores of the Indian Ocean we may suspect, that this rising is either now actually in progress, or has taken place quite recently. By our theory, we may conclude that the areas are likewise immense which have lately subsided, or, judging from the earthquakes occasionally felt, and from other appearances, are now subsiding. The smallness of the scale of our map should not be overlooked: each of the squares on it contains (not allowing for the curvature of the earth) 810,000 square miles. Look at the space of ocean from near the southern end of the Low Archipelago to the northern end of the Marshall Archipelago,—a length of 4500 miles, in which, as far as is known, every island, excepting Aurora, which lies just without the Low Archipelago, is atoll-formed. The eastern and western boundaries of our map are continents, and they are rising areas: the central spaces of the great Indian and Pacific Oceans are mostly subsiding; between them, north of Aus-

"* During the subterranean disturbances which took place in Chile, in 1835, I have shown (*Geolog. Trans.* 2nd Ser. vol. v. p. 606,) that at the same moment that a large district was upraised, volcanic matter burst forth at widely-separated points, through both new and old vents.

tralia, lies the most broken land on the globe, and there the rising parts are surrounded and penetrated by areas of subsidence; so that the prevailing movements now in progress, seem to accord with the actual states of surface of the great divisions of the world.

"The blue spaces on the map are nearly all elongated; but it does not necessarily follow from this, (a caution, for which I am indebted to Mr. Lyell,) that the areas of subsidence were likewise elongated; for the subsidence of a long, narrow space of the bed of the ocean, including in it a transverse chain of mountains, surmounted by atolls, would only be marked on the map by a transverse blue band. But where a chain of atolls and barrier-reefs lies in an elongated area, between spaces colored red, which therefore have remained stationary or have been upraised, this must have resulted either from the area of subsidence having originally been elongated (owing to some tendency in the earth's crust thus to subside), or from the subsiding area having originally been of an irregular figure, or as broad as long, and having since been narrowed by the elevation of neighbouring districts. Thus the areas, which subsided during the formation of the great north and south line of atolls in the Indian Ocean,—of the east and west line of the Caroline atolls,—and of the north-west and south-east line of the barrier-reefs of New Caledonia and the Louisiade, must have originally been elongated, or if not so, they must have since been made elongated by elevations, which we know to belong to a recent period.

"I infer from Mr. Hopkins's researches, that for the formation of a long chain of mountains, with few lateral spurs, an area elongated in the same direction with the chain, must have been subjected to an elevatory movement. Mountain-chains, however, when already formed, although running in very different directions, it seems,* may be raised together by a widely acting force: so, perhaps, mountain-chains may subside together. Hence, we cannot tell whether the Caroline and Marshall Archipelagos, two groups of atolls running in different directions and meeting each other, have been formed by the subsidence of two areas, or of one large area, including two distinct lines of mountains. We have, however, in the southern prolongation of the Mariana Islands, probable evidence of a line of recent elevation having intersected one of recent subsidence. A view of the map will show that, generally, there is a tendency to alternation in the parallel areas undergoing opposite kinds of movement; as if the sinking of one area balanced the rising of another.

"The existence in many parts of the world of high table-land, proves that large surfaces have been upraised in mass to considerable heights above the level of the ocean; although the highest points in almost every country consist of upturned strata, or erupted matter: and from the immense spaces scattered with atolls, which indicate that land originally existed there, although not one pinnacle now remains above the level of the sea, we may conclude that wide areas have subsided to an amount, sufficient to bury not only any formerly existing table-land, but even the heights formed by fractured strata, and erupted matter. The effects produced on the land by the later elevatory movements, namely, successively rising cliffs, lines of erosion, and beds of littoral shells and pebbles, all requiring time for their production, prove that these movements have been very slow; we can, however, infer this with safety, only with respect to the few last hundred feet of rise. But with reference to the whole vast amount of subsidence, necessary to have produced the many atolls widely scattered over immense spaces, it has already been shown, (and it is, perhaps, the most interesting conclusion in this volume) that the movements must either have been uniform and exceedingly slow, or have been effected by small steps, separated from each other by long intervals of time, during which the reef-constructing polypifers were able to bring up their solid frame-works to the surface. We have little means of judging whether many considerable oscillations of level have generally occurred during the elevation of large tracts; but we know, from clear geological evidence, that this has frequently taken place; and we have seen

"* For instance, in S. America from lat. 34° for many degrees southward, there are upraised beds containing recent species of shells, on both the Atlantic and Pacific side of the continent, and from the gradual ascent of the land, although with very unequal slopes, on both sides towards the Cordillera, I think it can hardly be doubted that the entire width has been upraised in mass within the recent period. In this case the two W.N.W. and E.S.E. mountain-lines, namely, the Sierra Ventana and the S. Tapalguen, and the great north and south line of the Cordillera, have been together raised. In the West Indies the N. and S. line of the eastern Antilles, and the E. and W. line of Jamaica, appear both to have been upraised within the latest geological period.

on our map, that some of the same islands have both subsided and been upraised. I conclude, however, that most of the large blue spaces have subsided without many and great elevatory oscillations, because only a few upraised atolls have been observed: the supposition that such elevations have taken place, but that the upraised parts have been worn down by the surf, and thus have escaped observation, is overruled by the very considerable depth of the lagoons of all the larger atolls; for this could not have been the case, if they had suffered repeated elevations and abrasion. From the comparative observations made in these latter pages, we may finally conclude, that the subterranean changes which have caused some large areas to rise, and others to subside, have acted in a very similar manner.

"*Recapitulation.*—In the first three chapters, the principal kinds of coral-reefs were described in detail, and they were found to differ little, as far as relates to the actual surface of the reef. An atoll differs from an encircling barrier-reef only in the absence of land within its central expanse; and a barrier-reef differs from a fringing-reef, in being placed at a much greater distance from the land with reference to the probable inclination of its submarine foundation, and in the presence of a deep-water lagoon-like space or moat within the reef. In the fourth chapter the growing powers of the reef-constructing polypifers were discussed; and it was shown, that they cannot flourish beneath a very limited depth. In accordance with this limit, there is no difficulty respecting the foundations, on which fringing-reefs are based; whereas, with barrier-reefs and atolls, there is a great apparent difficulty on this head;—in barrier-reefs, from the improbability of the rock of the coast or of banks of sediment extending, in every instance, so far seaward within the required depth;—and in atolls, from the immensity of the spaces over which they are interspersed, and the apparent necessity for believing that they are all supported on mountain-summits, which, although rising very near to the surface-level of the sea, in no one instance emerge above it. To escape this latter most improbable admission, which implies the existence of submarine chains of mountains of almost the same height, extending over areas of many thousand square miles, there is but one alternative; namely, the prolonged subsidence of the foundations, on which the atolls were primarily based, together with the upward growth of the reef-constructing corals. On this view every difficulty vanishes: fringing-reefs are thus converted into barrier-reefs; and barrier-reefs, when encircling islands, are thus converted into atolls, the instant the last pinnacle of land sinks beneath the surface of the ocean.

"Thus the ordinary forms and certain peculiarities in the structure of atolls and barrier-reefs can be explained;—namely, the wall-like structure on their inner sides—the basin or ring-like shape both of the marginal and central reefs in the Maldiva atolls—the union of some atolls as if by a ribbon—the apparent disseverment of others—and the occurrence, in atolls as well as in barrier-reefs, of portions of reef, and of the whole of some reefs, in a dead and submerged state, but retaining the outline of living reefs. Thus can be explained the existence of breaches through barrier-reefs in front of valleys, though separated from them by a wide space of deep water; thus, also, the ordinary outlines of groups of atolls, and the relative forms of the separate atolls one to another; thus can be explained the proximity of the two kinds of reefs formed during subsidence, and their separation from the spaces where fringing-reefs abound. On searching for other evidence of the movements supposed by our theory, we find marks of change in atolls and in barrier-reefs, and of subterranean disturbances under them; but, from the nature of things, it is scarcely possible to detect any direct proofs of subsidence, although some appearances are strongly in favour of it. On the fringed coasts, however, the presence of upraised marine bodies of a recent epoch, plainly show, that these coasts, instead of having remained stationary, which is all that can be directly inferred from our theory, have generally been elevated.

"Finally, when the two great types of structure, namely, barrier-reefs and atolls on the one hand, and fringing-reefs on the other, were laid down in colors on our map, a magnificent and harmonious picture of the movements, which the crust of the earth has within a late period undergone, is presented to us. We there see vast areas rising, with volcanic matter every now and then bursting forth through the vents or fissures with which they are traversed. We see other wide spaces slowly sinking without any volcanic outbursts; and we may feel sure, that this sinking must have been immense in amount as well as in area, thus to have buried over the broad face of the ocean every one of those mountains, above which atolls now stand like monuments, marking the place of their former existence. Reflecting how powerful an agent with respect to denudation, and consequently to the nature and thickness of the deposits in accumulation, the sea must ever be, when acting for prolonged periods on the land,

during either its slow emergence or subsidence; reflecting, also, on the final effects of these movements in the interchange of land and ocean-water, on the climate of the earth, and on the distribution of organic beings,—I may be permitted to hope, that the conclusions derived from the study of coral formations, originally attempted merely to explain their peculiar forms, may be thought worthy of the attention of geologists."

The gratification which we have derived from the reading of this treatise, impels us earnestly to hope that it may meet with that cordial reception from the author's fellow labourers in science, which is due to his devoted zeal, his patience and industry, not less than to his modest pretensions. Such a reception might not a little tend to encourage him in the production of the Second and Third Parts of his Geological Observations connected with the Voyage of the Beagle—one on the Volcanic Islands visited—and the other on the Geology of South America.

The work is written in a very lucid style, and cannot fail to please all classes of readers, whether it be the finished chemist or geologist, or he who is zealous in the pursuit of general knowledge.

CHEMISTRY OF THE FOUR ANCIENT ELEMENTS, *Fire, Air, Earth, and Water. An Essay, founded upon Lectures delivered before Her Most Gracious Majesty*, THE QUEEN, *and Dedicated by Special Permission to Her Majesty.* By THOMAS GRIFFITHS, Lecturer on Chemistry and Medical Physics at St. Bartholomew's Hospital. London: Samuel Highley, 32, Fleet Street. 1842. pp. 226.

A VERY interesting little work, treating of the four ancient elements after the approved fashion of modern times. The author commences by giving some of the alchemical notions regarding the elements, and then proceeds to furnish the general reader with information on the subject to which the work is devoted; and the clear and succinct manner in which that information is conveyed, and the simplicity of the language, with the sparing use made of technicalities, must insure this little work a very extensive perusal.

It is expressly "intended for the use of those who have not studied the science; abstruse details and theories are, therefore, avoided, useful and interesting information supplied, and instruction united with entertainment. Explicit directions are given respecting the performance of the experiments, and cautions concerning the nature of several agents employed during the inquiry will be found in the Appendix."

The experiments are very interesting, and may be performed by any careful person.

THE EFFECTS OF A LIGHTNING FLASH *on the Steeple of Brixton Church; and Observations on Lightning Conductors generally.* By CHARLES V. WALKER, Esq., Hon Sec. London Electrical Society. London: Stewart and Murray, Old Bailey. pp. 18.

A CLEVER pamphlet by the talented Secretary of the London Electrical Society, and one which our readers will do well to procure and read with attention. Want of space precludes us from saying more. It is embellished with an explanatory plate. We have given in another column a "Note on Ozone," extracted from this work.

II. CHEMICAL MANUFACTURES.

OBSERVATIONS ON THE SELECTION OF STONE FOR THE PURPOSES OF BUILDING.

BY CHARLES WATT, JUN.

AT a time when so many public buildings are in the course of erection, a few remarks on the selection of stones for the purposes of building may not be considered out of place; especially when it is stated that these observations arise from a mature and careful consideration of the subject, as well as from an examination of those stones of which the greater number of our public buildings are constructed.

Before going into this subject in detail, I would remark that no private interest should be allowed to weigh in the selection of stones for the construction of public buildings: that such is very frequently the case is well known: it is not, however, my intention on the present occasion to enter upon this question; I shall content myself with this passing remark, hoping that it will have the effect of rousing public attention. Stones should be selected and approved of by several persons

whose knowledge of the subject is publicly known. I shall, therefore, in the course of this paper, make some observations for the consideration of those who may be called upon for their opinion as to the quality of any stone intended for building purposes.

The two great desiderata to be sought for in a stone are durability and beauty; and if these are not to be found united in the same stone, the preference should be given without hesitation to the stone that possesses the first quality. I think that in this remark I shall be borne out by the public, especially when the vast expense of these buildings to the country is taken into consideration. Stone which is to be used for building should be as compact as possible. The method I pursue for ascertaining the "porosity" of stone is the following:*—I take pieces of the same weight, and as nearly as possible of the same shape, and put them in distilled water, allowing each of them to remain steeped in it for at least forty-eight hours; they are then taken out; their surfaces are wiped, in order to remove all adherent moisture, and their weights are again taken. The more compact the stone, the less will be its increase of weight when treated in the manner described. In some stones I have found the increase of weight double as much as in others. Looseness of structure is attended with many mechanical, not to say chemical disadvantages, (the latter of which I shall afterwards point out,) in consequence of the large quantity of water which it is capable of absorbing keeping the walls continually damp; and this, it is generally believed, and I am inclined to think that the opinion is correct, causes the stone to "scale;" the water expanding when the sun shines upon the building directly after a shower of rain. Of course this effect is not produced by one or two showers; it is the work of time. Again, the more porous a stone is, the more readily will it collect the "blacks" which are always floating in the air of a large town; and the beautiful carvings with which the stone may be adorned, are consequently hidden in a mass of blackness. That this must be the case to a certain extent in England, where so much coal is burnt, I readily admit; but the finer the texture of the stones of which the building is constructed, the less dirt will it collect, and the more easily will that dirt be removed by the rain.

As regards the composition of stones for the purposes of building, especially those selected for the exterior, I do most certainly disapprove of those which are composed of the carbonates of lime and magnesia, but especially the latter. But in this opinion we should, to a certain extent, be guided by the structure of the stone, as will be evident from the subsequent remarks.

The reason that I object to stones containing the above named carbonates is because those carbonates are to a certain extent soluble in water containing carbonic acid. Rain, falling, as it does, from a great height, absorbs in falling a quantity of the carbonic acid contained in the atmosphere, and the water containing carbonic acid passes into the stone, dissolving small portions of the before-mentioned carbonates in its passage. The carbonate of magnesia being more readily soluble in carbonic acid than the carbonate of lime, it will be dissolved in preference, and is therefore the most objectionable.

Being desirous of practically testing the accuracy of these observations, I tried some experiments on the subject. The following was the plan which I pursued:—Some pieces of stone being carefully and completely dried and weighed, were put into distilled water containing carbonic acid, in which they were allowed to remain some days; they were then taken out, thoroughly dried by a very gradual heat, and again weighed. It was found that those stones which contained the carbonates of lime and magnesia lost weight according to the length of time they were kept in the liquid containing a preponderance of carbonic acid, and until the water was saturated with those carbonates. To ascertain beyond a doubt that the water contained the carbonates before mentioned, it was analysed, which proved the affirmative.

It may be argued against this, that rain contains but a small portion of carbonic acid, whereas the water prepared by me contained a great deal: this I admit; but then it is to be recollected that the smallness of the quantity is made up by the frequent repetition of the dose.

That these dissolved carbonates are again liberated in the stone by the evaporation of the carbonic acid and water, is most probable; but they are deposited in a pulverulent form in the interstices of the stone, and consequently do not give any more stability to it than if they had been removed by the action I have described. These stones, therefore, in the course of time become so soft that they will crumble to pieces by mere rubbing between the fingers.

In the erection of buildings which are much exposed to the weather, the state of the surface so exposed is of much importance: the smoother and more polished it is, the less readily will it admit the passage of water; and this accounts for the fact that

* See also "The Chemist," No. XI.

marble, although carbonate of lime, is so very durable; the closeness of its texture, and the beautiful polish of which its surface admits, enabling the water to run off without passing into its interior.

Having thus remarked upon those stones which I have generally found to have most suffered from time, "the consumer of all things," I shall now pass on to the notice of those which appear to me the most durable.

These stones are for the most part siliceous stones, upon which carbonic acid and moisture produce no effect.

A beautiful example of the unalterableness of siliceous stones is exhibited in the stone of which the shrine of St. Winifred's Well, at Holywell, in Flintshire, is built, which has stood for centuries without being apparently the worse for wear. This stone is called Talacre stone.

In advocating the use of siliceous stones for buildings, I wish particularly to be understood not to mean those stones which are termed "sandstones;" these, although containing much silica, possess no durable qualities, as may be observed in many of even the newest buildings which have been erected with it. I have observed some buildings in which it has been used, although erected only some twenty or thirty years, the external surfaces of which are crumbling to pieces, and they are sad examples of the necessity of thoroughly investigating the qualities of a stone before using it in building.

In this stone, the silex is intermingled with carbonate of lime, and sometimes also with carbonate of magnesia; the silex is particularly granular, its particles adhere but loosely, and have very much the appearance of sand conglomerated into a mass, from which, I suppose, it derives its name of "sandstone." I think enough has now been said to show the necessity of using great care in the selection of stones; but should any more be required on this point, I would tell the inquirers to look around them, and they cannot fail to observe the deplorable state of dilapidation of some of even our most recent buildings.

In conclusion, I will merely add, that I hope these few observations may prove serviceable in the selection of stones for the important purposes of building.

A NEW METHOD OF FIXING PHOTOGRAPHIC IMAGES.

BY REUBEN PHILLIPS.

My method consists in coating the paper with a soluble compound of silver, which by the action of light becomes insoluble; then placing the picture in the menstruum in which the colorless compound dissolves, and transmitting through it a current of electricity. It is advisable to employ electrodes nearly as large as the picture, and to place them parallel with it, a short distance apart; the soluble compound is decomposed, and the silver is removed from the paper.

Some time since I made some experiments to ascertain how far the plan was applicable. I employed the common superfine writing paper, the ammonio-nitrate of silver, and a pile of five 5-inch plates; four were weakly charged with acid. I placed the picture between two pieces of flannel moistened with a weak solution of ammonia, placing the three between the two last plates; by this means I perfectly fixed the picture in about twenty minutes. There seems to be no reason why, with an efficient apparatus, the operation should not be concluded in one minute: perhaps a battery of ten plates in vigorous action would effect this object. Other compounds, more susceptible of light, may be employed instead of the nitrate of silver, as the fluoride; or an unalterable compound, as the iodide, may be moistened with a solution of silver.

REVIEW:—

MEMOIR ON THE ABSORPTION OF METALLIC POISONS BY PLANTS.

By P. Louyet.* Brussels: Société Encyclographique des Sciences Médicales, Rue de Flandre, 155. 1841. pp. 73, 8vo.

In this important little work, M. Louyet gives the public the advantage of an immense number of experiments which he has tried on this subject. The author's attention, as well as that of many others, was called to this useful point of scientific inquiry by the following question, put by the *Académie Royale des Sciences et Belles Lettres de Bruxelles*:—

To determine, by experiments, whether metallic poisons, such as white arsenic, (arsenious acid,) buried in cultivated soil, penetrate equally into all parts of the vegetables which grow in it, and among others into the seeds of cereals, and whether there is, on that account, any danger to the public health in spreading arsenious acid and other analogous poisons in fields, in order to destroy injurious animals.

The Memoir before us gained the silver medal, and it certainly well merited it: the gold medal was not awarded at that *concours*.

* *Mémoire sur l'Absorption des Poisons Métalliques par les Plantes.* Par P. Louyet.

Want of space prevents us from giving so lengthy an extract as we could wish: however, we will give a translation of the author's

" GENERAL CONCLUSIONS.

" From all the experiments which I have just detailed, I think that the following general conclusions may be drawn:

" 1st. When an insoluble metallic compound is introduced into the earth, it does not appear to prevent germination, and plants grown in the soils containing these compounds do not contain any quantities of them appreciable by reagents, except when they have in some measure a kind of affinity for those reagents; as we see in the case of the hortensia and oxide of iron.

" 2nd. If solid and soluble metallic compounds be introduced into an ordinary soil, and if different seeds be sown in it, germination may be entirely prevented, if the quantity of the compound in the soil be very great; and partially, if the quantity be small as compared with the extent of the soil. In the latter case, no part of the plant appears to contain any of the poisonous matter: besides, certain soluble compounds (metallic) introduced into the earth, are there converted into insoluble compounds: they then come within the observations in the preceding paragraph. The same results seem to be obtained with the compound in the solid form, or dissolved in water; the mixture of arsenious acid and farina appears to act as an insoluble compound, that is to say, it does not prevent the development of the vegetable.

" 3rd. When an insoluble poisonous metallic compound is introduced into the earth, at the root of growing vegetables, it does not appear in any way to injure them, and does not penetrate into their parts.*

" 4th. If we pour at the root of a developed vegetable a metallic solution not susceptible of being converted into any insoluble compound by its contact with the elements of the soil, the metallic substance is absorbed by all parts of the vegetable, even by the seeds, in great abundance.

" 5th. On immerging an entire plant, by the root or the extremity of the stem, in any metallic solution whatever, the inorganic substance is absorbed by all parts of the vegetable, and is thus found in the seeds.

" 6th. When different seeds are immerged for some time in certain metallic solutions, some of the latter, as the arsenical solutions, seem to destroy the power of germinating; others, on the contrary, do not appear to produce any sensible effect; and, in the latter case, the plants which arise from these seeds do not contain the inorganic compound in sufficient quantity to be appreciated by the most delicate tests."

We may occasionally give further extracts from this useful little work, and we recommend the subject to the attention of agriculturists.

III. PHARMACY, &c.

TREATMENT OF THE ASSISTANTS OF MEDICAL MEN AND CHEMISTS: PROPOSED REMEDY.

To the Editors of The Chemist.

London, May, 1842.

GENTLEMEN,—

The crisis has now arrived at which all communities and individuals must respectively stand up for their own rights and be fully alive to their own interests, otherwise they will be swept down by the overwhelming tide of prejudice, oppression, and tyranny, which is fast encroaching upon us, bringing with it poverty, degradation, and crime. Such is the case with some members of the medical and chemical bodies, *viz.* those already settled in life, either as physicians, surgeons, apothecaries, or chemists and druggists; but still, gentlemen, there remains a most numerous class, who may really be termed the *white slaves* of England, having been hitherto very hardly dealt with, and being still much aggrieved; it is the medical, surgical, and chemical assistants, in whose behalf, as well as my own, this statement is laid before you, with a sincere hope, that you will befriend us, by giving publicity to this letter in the columns of your valuable Journal, THE CHEMIST, in order that it may reach the notice of every master, as also meet with their and your serious consideration, and, more especially, that all assistants may see the great necessity of exerting themselves, to gain proper redress, and such alterations, as will tend greatly, not only to benefit themselves, but also their employers and the public at large.

With these views do I urge them to meet

* It must be observed that I acted on terrestrial plants, (*plantes terrestres,*) in a dry soil, and at a season when it rarely rains, —the month of July.

together and set an example in endeavoring to do that which others have failed in, by becoming united and causing an unanimous feeling, in principles, sentiments, and actions, to exist throughout, for the purpose of protecting themselves, and doing unto others as they would should be done unto them. In all our plans and operations, let us not forget to pay due respect to all, expecting the same in return, condemning none for thinking differently to ourselves, even though they be in error, but striving, by conciliating means, and reasonable and persuasive arguments, to convince them otherwise.

With regard to remuneration, will it be credited, that 20*l.* per annum is considered ample by our employers, (who are probably in the actual receipt of many hundreds per annum,) for a man, who sacrifices his very life, health, and happiness, to obtain him this money; one, whose work is never done, whose nocturnal rest is continually broken, and whose mental anxiety and bodily fatigue, in the execution of his highly responsible duties, are very great; his board and lodging is allowed, certainly, such being the most plain, in every respect; and, in addition to all this he is obliged silently to bear with his employer's ways and temper, however harsh they may be, if he wishes to retain his place and insure a fair character. The profession requires him to make a gentlemanly appearance, and such must be done out of his little money, besides paying his way, and putting by a something to support himself with, when ill, out of place, or in after years!

These, gentlemen, are facts; and as we do not carry our brains in our pockets, and I dare say there are but few of us who have been born with golden spoons in our mouths, I appeal to yourselves and the public, whether we are not justified in uniting together, to assist and protect one another, to prove that we have yet some little respect for ourselves, inasmuch as we intend to accept no less than 30*l.* per annum, so being raised a little above the pay of a common laborer, and thereby being enabled to do justice, honestly and willingly, to our employers?

Were our masters to reflect upon these truths; to recollect that we are respectable men, educated, and endowed with feeling like themselves; that we are now, what they formerly were, and compelled, by circumstances alone, to be thus differently situated, and forced to submit to such unchristian-like, ungenerous, and erroneous usage,—I feel confident they would perceive the justness of my remarks, and would be convinced how much more heartfelt interest would be taken by us in their employ, did they act towards us more like companions, than servants, with greater humanity and kindness; and were we paid like men, possessed, not only of hands, but heads, with a full share and use of our mental, moral, and intellectual faculties.

I would propose that a Committee, in London, be formed, who shall have no selfish or individual interest, but the general good of all, for their only object; that such Committee represent the whole body of assistants, and that a general meeting take place as soon as possible, to take into consideration the following, *viz.*—

I. The drawing up, signing, and getting of a petition presented, praying that a clause be inserted in the forthcoming New Medical Reform Bill, providing,

1st. That every assistant, either to a surgeon, apothecary, or chemist, receive not less than the fixed salary of 30*l.* per annum.

2nd. That a portion of each day be allowed every assistant, during which time he may either walk out, read, experimentalise, or otherwise recreate himself.

3rd. That no work be done, except upon any urgent necessity, on Sunday, and that that day be at the disposal of every assistant; and,

4th. That public rewards be granted, in order to induce men to study, search, and dive into those profound and beautiful sciences of Chemistry and Botany, as also the arts of Surgery and Medicine, and the knowledge of Pharmacy, that further discoveries and improvements may be made in each.

II. The forming of a fund, to be raised by a monthly or quarterly subscription, for assisting such as may require it under any peculiar circumstances, such as, being out of place, or in case of illness, &c., as may be hereafter determined upon, &c. &c.

Hoping you will honor us by making your own remarks upon this, and giving it a place in your Journal,

With great respect and gratitude, allow me to remain,

Gentlemen, in haste,
Yours faithfully and sincerely obliged,
A London Assistant and a former Correspondent.

Our correspondent is justified in his complaints: the manner in which chemists' and medical men's assistants are treated demands legislative interference as much as ever did the Factory question. Men, nay, not unfrequently, mere boys, the assistants and apprentices of chemists, have to labor incessantly, for, perhaps, seventeen or eighteen hours *per diem*, deducting the short time allowed them for their oft interrupted meals.

The advice given to the body of the assistants to meet and petition for legislative aid meets with our concurrence; but it is of no use to give advice, unless we give the means of following it. The master chemists and druggists will not allow their assistants and apprentices to attend any such meeting, should one be convened: not that they would be injured by the success of their servants in obtaining an enactment restricting their hours of labour; the compulsion to close early would be as beneficial to one as to the other: but then not to be allowed to pay them less than £30 per annum; this must and will meet with opposition from the niggardly; others, we think, would, for many reasons, allow of it.

ON THE STATE OF THE MERCURY IN HYDRARGYRUM CUM CRETA.

BY CHARLES WATT, JUN.

A DIFFERENCE of opinion having so long existed as to the state in which the mercury exists in the Hydrargyrum cum creta, I some months ago commenced a course of experiments, the object of which was to elucidate the subject; but pressing engagements then compelled me for a time to defer the prosecution of the investigation; now, having sufficiently ascertained the nature of the compound under consideration, I hasten to lay the result before the readers of THE CHEMIST.

After the description of the mode of preparing the hydrargyrum cum creta, in Thomson's *London Dispensatory*, the following remarks are made:—

"In these processes the mercury is generally supposed to be oxidised during the trituration, and in the state of the protoxide, 100 parts of which, according to Fourcroy, contain, when well prepared, almost 4 of oxygen; but Mr. Phillips says that the mercury in this preparation is not a protoxide, being insoluble in acetic acid: but he adds, that when the chalk is taken up by acetic acid, the mercury does not run together, but remains in minute globules,—a circumstance which induces the belief that it is not merely mechanically divided."

If the chalk contained in this compound be removed by acetic acid, and if the remaining undissolved portion be examined, it will be clearly seen not to be metallic mercury. That the compound does contain small portions of mercury, in the metallic state, I readily admit; the quantity in it generally amounts to 4 or 5 per cent. of the whole quantity of mercury.

The particles of the mass, instead of being round, like the globules of mercury, however finely divided, have an appearance similar to that of a precipitate.

Being convinced, from the above observations, that it was not metallic mercury, I analysed the compound, and the result entirely coincides with the analysis of Fourcroy, namely, that it is the protoxide of mercury. This may very readily be proved in the following manner:—

Saturate the carbonate of lime by means of acetic acid; wash the portion which remains undissolved once or twice, then add to it hydriodic acid. This immediately converts it into the green protiodide of mercury. Hydriodic acid does not act upon metallic mercury: this, therefore, most decidedly proves that it is the protoxide.

There is one thing remarkable about protoxide of mercury thus obtained; *i. e.* its difficult solubility in acetic acid; and this it is that has caused the above-quoted erroneous opinion given by Mr. Phillips. I cannot exactly account for this difficult solubility; but I think that it must have been generally observed by chemists that metallic oxides prepared by different methods, and with solutions of the metallic salts of different strengths, differ considerably in the readiness of their solubility in the acids.

Some time since it was proposed to prepare hydrargyrum cum creta by mixing the protoxide prepared by means of an acid, with carbonate of lime, in order to save the immense labour of trituration,—a process which occupies many days. Some was prepared by this means, but the compound was said to fall far short in power on the human frame to that prepared by the usual method. This may probably arise from the fact that the compound prepared in the ordinary way always contains a small portion of metallic mercury.

Sufficient has, I hope, been advanced to prove that the mercury exists in the hydrargyrum cum creta, in the state of the protoxide; but should it be wished to analyse it, the object may be accomplished in the following way:—

Remove the carbonate of lime by means of acetic acid, and well wash the insoluble portion; lay it on blotting paper, in a dark place, and dry it as much as possible; then place it in a vacuum containing sulphuric acid, until it is completely dry.*

It is then to be placed in a glass tube, heated by means of a spirit lamp, and the evolved oxygen collected in the usual manner.

* Great care is requisite during this part of the operation, which should be conducted in as dark a place as possible.

It will be as well to remove the protoxide from the small portion of metallic mercury before mentioned, previous to analysing it; this may be done by keeping the oxide suspended in water, by gently shaking and pouring off the supernatant liquor, which contains the oxide in suspension. The metallic mercury, being heaviest, falls first. By repeating this operation two or three times, all the oxide may be removed from the metallic mercury.

In conclusion, I may remark, that because the mercury remaining after the removal of the carbonate of lime by acetic acid, did not readily dissolve in acetic acid, this was not sufficient to warrant the conclusion that it was not protoxide of mercury, but metallic mercury; for it should have been borne in mind that protoxide of mercury, however prepared, is by no means easily soluble in that acid.

ON CERTAIN FERRUGINOUS PREPARATIONS.

BY MR. A. J. COOLEY.

In an examination of several samples of vinum ferri, recently entered into with the view of eliciting a method of rendering this article of more regular strength and of increased medicinal power, I was induced to extend my researches to several other ferruginous preparations. The result has proved quite satisfactory; and in the present paper several formulæ will be given, which I believe will be found *entirely new*, as well as some others, which are but little known, or but lately introduced into England. The latter will be noticed for the purpose of affording information to those who have not an opportunity of perusing foreign works on Pharmacy.

CITRATE OF IRON.

There are three salts *generally known* under this name—*two* having the peroxide for their base, and *one*, the protoxide. There is also a fourth, formed from the magnetic oxide of iron, which has scarcely been introduced into this country, though commonly employed in France, and highly recommended by Béral. The salt at present so much puffed by advertisement as citrate of iron, is a double citrate of iron and ammonia—an ammonio-citrate,—and as such I shall describe it. I find that several other double citrates of iron may be prepared, but they are possessed of inferior qualities to those just mentioned. They therefore offer no inducement for their manufacture.

CITRATE OF PROTOXIDE OF IRON.

This salt is easily formed by digesting iron filings or wire in liquid citric acid. It presents the appearance of a white powder, nearly insoluble in water, and rapidly passing to a higher state of oxidation under the influence of light, damp, or warmth, or mere exposure to the air under most ordinary circumstances. Its taste is very metallic, and it is best exhibited under the form of pills, mixed with gum and syrup, or syrup, to prevent it from being prematurely decomposed.

CITRATE OF MAGNETIC OXIDE OF IRON.

Prepared from the magnetic oxide of iron, in the same way as the last. It may be formed into beautiful transparent scales, or lamellæ, in a similar manner to the ammonio-citrate. Its solution is of a lively green color, permanent in the air, but possessing an intensely ferruginous taste. For this reason, this citrate can be exhibited only in pills, or in syrup. A form for the preparation of the magnetic oxide of iron may be found in The Chemist, No. XXVII. p. 94.* The plan I followed was nearly similar.

CITRATE OF PEROXIDE OF IRON.

This beautiful salt is of a rich ruby color, and may be obtained under the form of glistening transparent scales, sparingly soluble in water. It is very suitable for pills. Its preparation is similar to that of the ammonio-citrate, with the omission of the ammonia.

AMMONIO-CITRATE OF PEROXIDE OF IRON.

This preparation is similar in appearance to that last mentioned, and possesses the valuable properties of being very soluble in aqueous menstrua, while its solution is not so easily decomposed as that of many other salts of iron. It is nearly tasteless, and highly deliquescent. The absurd statements put forth in the advertisements respecting this preparation being compatible with the volatile and fixed alkalis and their carbonates; &c., I find to be incorrect; for on adding some liquor potassæ to a solution of this salt, it immediately became turbid, exhaled ammonia in large quantities, and deposited oxide of iron. This was the case both with the citrate prepared by the advertising gentleman above alluded to, as well as my own. I found the same take place with the carbonate; and no doubt, had I extended the experiments to the other articles mentioned as compatible, I should have met with another similar result.

This salt is most conveniently formed by dissolving moist hydrated peroxide of iron in liquid citric acid (pure), assisting the so-

* Taken from *The Pocket Formulary*, 2nd edition.

lution by heat, and then bringing it to a perfectly neutral state by the addition of a little sesquicarbonate of ammonia. It must then be filtered, cooled, and spread very thinly on warm sheets of glass to dry, which it will rapidly do, and may then be easily detached from the glass, in thin scales, or lamellæ, of great brilliancy and beauty. Only a gentle heat must be employed, not exceeding that of a water bath. This is the mystery of producing those beautiful transparent ruby-colored scales which are so much admired. It is doubtful whether this article has not obtained a larger sale from its pleasing appearance, than from its medicinal virtues. I know several parties who have prepared this salt in lumps or powder, by simple evaporation of the solution to dryness, who have been unable to sell it under that form, even at a lower price.

M. Béral, in his directions for the preparation of this salt, directs a platina capsule to be used, as well as attention to other minutiæ, which I find quite unessential to the success of the operation. Glass, Wedgewood ware, or even metallic vessels, may be employed; the former, however, are preferable. I find that boiling water will dissolve about twice its weight of citric acid, and retain $\frac{1}{2}\frac{3}{0}$ths of this quantity in solution when cold, and that it takes rather more than half the weight of the citric acid, in most hydrated peroxide of iron, to produce saturation. We may, therefore, with great advantage, employ the following formula, which contains nearly the proportions recommended by Béral:—

Crystals of citric acid . . .	1 part
Boiling distilled water . . .	1 do.
Dissolve, *add* . .	$2\frac{1}{8}$ do. to $1\frac{1}{4}$ do.
Moist hydrated peroxide of iron	1 do.

Continue the heat until the acid is saturated, then add ammonia q. s. Filter, &c.

It is better to use more oxide than the acid will dissolve, as the remainder may be employed in a future operation. Less water may be used, or even a larger quantity than that mentioned; but in the first case the liquid will become difficult to filter—in the latter, it will require evaporation.

HYDRATED PEROXIDE OF IRON.

I prepare this oxide in sufficient purity for the above purpose, by adding nitric acid to a nearly saturated solution of pure sulphate of iron in distilled water, until red fumes cease to be produced on the addition of more acid, or a blue color when tested with the red ferro-cyanide of potassa. The red oxide must then be precipitated by adding the solution of an alkali; after which it must be well washed with distilled water, and have its redundant water drained off by enclosing it in a piece of linen and allowing it to hang in a cool place for a sufficient length of time. It must be used *moist*.

We are indebted to the celebrated French *Pharmacien*, whose name I have before mentioned, for the preceding preparations; they have been employed in France for the last twelve years, but only lately introduced into this country.—(*See* THE CHEMIST, Vol. I. page 92, and Vol. II. page 27.)

FERRO-CITRATE OF QUININE.

If fresh precipitated quinine be dissolved in an excess of citric acid, and if this solution be added to a solution of citrate of peroxide of iron, a salt may be obtained of similar appearance to the last preparation, and possessing the medicinal properties of these two powerful bodies combined. The proportions should be 9 of the citrate of iron to 2 of the citrate of quinine. This salt is soluble; but, from its intensely bitter taste, should be administered in the pilular form.

TARTRATE OF PEROXIDE OF IRON.

By saturating pure liquid tartaric acid with fresh precipitated peroxide of iron, we obtained a solution of a rich color, of little taste, and of great permanency. It resembles the citrate of iron in its general character, while it is considerably less expensive. The proportions should be 75 parts of crystallised tartaric acid dissolved in twice that weight of water to about 45 parts of the oxide. The latter must be always in excess. This may be concentrated by evaporation.

AMMONIO-TARTRATE OF PEROXIDE OF IRON.

If to the above solution we add a small quantity of sesquicarbonate of ammonia, we produce a perfectly neutral salt, possessing most of the properties of the ammonio-citrate, and many valuable ones peculiar to itself, and characteristic of the salts of the acids of which it is formed. In solution, its color and taste are very similar to those of the citrate, and certainly not more ferruginous; and it is only by great dilution with water that any difference of color can be perceived; it then displays a lively gold color. By evaporation, it may be obtained under the form of a hard gummous mass of a fine garnet color. It is soluble in all proportions in water, and is compatible with many preparations which decompose other salts of iron, including the citrate. I dissolved two ounces of this salt in one pint of water, and added GRADUALLY one pound of potassa, without rendering the solution turbid, or freeing its ammonia. I afterwards added

potassa to a small quantity of this solution in such excess, that it fell undissolved to the bottom of the glass; the result was similar. I tried ammonia in the same manner, with like success. I believe there is no other reason why this salt should not supersede the more costly ammonio-citrate, than that the latter is obtained under a form more inviting to the eye. I shall presently show in what way this article may be employed to produce several other very elegant preparations.

AMMONIO-TARTRATE OF PROTOXIDE OF IRON (AIKIN'S).

Digest iron filings, or fresh precipitated protoxide of iron, (prepared from the sulphate by adding an alkali,) in liquid tartaric acid, to saturation; then add a sufficient quantity of ammonia, filter, and evaporate to dryness. This salt may also be obtained in glossy black scales, by spreading it on glass, and drying it after the plan employed for the ammonio-citrate. This preparation is soluble, and tastes less ferruginous than any other soluble salt of the protoxide of iron.

LACTATE OF PROTOXIDE OF IRON.

I find that the best method of procuring this article is to add a saturated solution of protochloride of iron in alcohol, to a saturated solution of lactate of lime, also in alcohol. The alcohol may easily be recovered by distillation.

Iron filings or fresh precipitated protoxide of iron may also be dissolved in lactic acid, by digestion at a moderate heat, and the solution filtered and concentrated until it crystallises upon cooling. This salt must then be washed in alcohol and dried quickly.

LACTATE OF PEROXIDE OF IRON.

To a solution of lactate of lime add a solution of persulphate of iron, when a soluble lactate will be obtained. This salt may also be made, by direct solution of the moist peroxide of iron in lactic acid, by means of heat. These preparations have been highly spoken of, but are little employed in England. To procure the lactic acid, add one part of sugar of milk in solution, to twenty parts of milk, and expose the mixture at a summer heat for several days, when the free acid formed must be saturated by the addition of bicarbonate of soda or lime. Expose again for a like period, and again neutralise; repeat the operation until it ceases to become acid on further exposure; boil, filter, and evaporate to a syrup; add alcohol, and digest; filter; add sulphuric acid, avoiding excess, to throw down the alkali,—filter again. The liquid is lactic acid dissolved in alcohol, which may be evaporated.

ARSENIATE OF IRON.

Obtained by decomposing the arseniate of soda in solution, by adding a solution of green sulphate of iron. This article is in the form of a heavy powder;—by a more troublesome process it may be produced in crystals.

TANNATE OF PEROXIDE OF IRON.

Add tincture of galls to a solution of a peroxidised salt of iron. Expose to the air for a short time; filter; wash with distilled water, and dry on bibulous paper. We thus obtain a dark blue powder, possessing neither taste, smell, nor solubility.

TANNATE OF PROTOXIDE OF IRON.

This preparation suffers rapid decomposition, even during the process of its production, and must therefore be made in combination with syrup, as in the following article.

SYRUP OF TANNATE OF IRON.

Simple syrup	500 parts
Citrate of magnetic oxide of iron	10 ditto
Ext. of galls	5 ditto

Mix, and keep it in a cool, dark place.

WINE OF IRON—VINUM FERRI.

The variable strength of this article, as prepared by different parties, has long been complained of. In the Pharmacopœia of 1788, iron filings or wire were directed to be digested in white wine for a month;—in that of 1836, by dissolving a soluble potassio-tartrate of iron in a mixture of three parts of water and two of spirits of wine; while the present Pharmacopœia has discarded it altogether. The consequence has been, that almost every druggist prepares this article according to his own form, and generally by mere digestion of metallic iron in white wine, commonly raisin sherry. The variable nature of such a preparation may readily be surmised, and the amount of its virtues as a remedial agent will appear from the following experiments:—Six samples of vinum ferri were taken, three of which were procured from as many different houses, and three prepared by myself;—two of the latter being made by the old process, with sherry wine, and one by the formula of the last Pharmacopœia. I found, by very careful examination, that they contained the following amounts of metal, which I obtained from them in the state of oxide:—

Sample 1.	.	14 grs. in the pint.
2.	.	109 ditto.
3.	.	29 ditto.
4.	.	8 ditto.
5.	.	47 ditto.
6.	.	83 ditto.

These experiments, repeated several times,

produced nearly similar results, and from this we may easily perceive why the College rejected this article from their last Edition.

I find that sherry wine or raisin wine may stand over iron filings, (especially in close vessels,) for months, without more metal being dissolved than will give it a disagreeable ferruginous taste, and a *very minute quantity* will produce this effect. The little acidity in the wine will readily account for this. When the potassio-tartrate is dissolved in a weak spirit, a large quantity is again deposited by age, and the preparation is wholly deficient in those vinous qualities, which should be present, to render this article agreeable.

Mr. Donovan has proposed the digestion of two ounces of well levigated rust of iron in one pint of the best hock, at a temperature of 100°, for 12 hours, accompanying the digestion with frequent agitation. This will, no doubt, produce a very *agreeable* article, but I fear the process is not at all suited to render it of an invariable strength. The same plan was advised some years ago, with acid Teneriffe, but was not generally adopted, and rapidly fell into disuse. Wine has not sufficient solvent power to take up iron, or any of its preparations *insoluble* in water, in sufficient quantity to render this course advisable. A consideration of these facts, and many attempts to remedy them, has led me to adopt the following formula for wine of iron:—

I take one gallon of white wine, previously shaken with a small quantity of solution of isinglass and filtered, and add thereto nine ounces of the tartrate of peroxide of iron, or ten ounces of the ammonio-pertartrate dissolved in one pint of warm wine. The mixture must then be filtered, when a beautiful and nearly tasteless wine of iron will be procured, retaining its strength and transparency for years, and capable of being administered with many articles that decompose other solutions of this metal. Every ounce will contain 16 grains of the tartrate in solution, and the dose should be similar to that directed for the old vinum ferri, *viz.* 2 to 4 drachms. It will be seen that this wine will contain the same salt in solution, as that prepared by the more expensive and uncertain form of Mr. Donovan.

Several other wines of iron, besides that usually so called, are occasionally asked for, among which may be mentioned,

WINE OF ACETATE OF IRON,

prepared by direct solution of one part of acetate of iron in about two hundred and forty parts of white wine, as directed by Soubeiran, and

WINE OF FERRO-CITRATE OF QUININE,

directed by Béral. One part of citrate of iron, and three parts of quinine being dissolved in forty-six parts of wine.

LIQUOR OF PERTARTRATE OF IRON.

Tartrate of peroxide of iron	½ part
Simple syrup . .	2 do.
Distilled water . .	7 do.
Spirit of wine . .	1½ do.

Dissolve the salt in the water, then add the syrup and spirit, and lastly, six or seven drops of essential oil of bitter almonds. This solution is of a beautiful golden color, has a strong vinous smell and taste, is very agreeable and permanent, and may be used in the same way as the vinum ferri. The liquor of the ammonio-pertartrate and Aikin's ammonio-tartrate, may also be prepared as above.

COMPOUND SOLUTION OF BINIODIDE OF IRON.

Iodide of potassium .	2 parts
Green sulphate of iron .	1 do.
Distilled water . .	40 do.

Dissolve, then add

Nitric acid q. s. (*carefully*), until the liquor becomes quite transparent. It will then assume a rich deep red color.

I have not entered into any particulars respecting the administration of the preceding articles. It is the province of the Pharmaceutist to attend to their preparations,—that of the medical practitioner to their use and curative powers. It may not be amiss, however, to remark, that as a remedial agent, iron, when properly exhibited, acts as a genial stimulant and tonic, and it generally proves beneficial in cases of chronic debility, unaccompanied with organic congestion or inflammation.

Iron is one of the most valuable articles of the materia medica, and appears from the antiquity of its introduction into medicine, and the number of its preparations, to have been deservedly appreciated. It bears the recommendation of upwards of 3000 years upon its brow, and surely "a medicine that hath withstood such vicissitudes *cannot* be *destitute* of *virtue*."

(*To be continued.*)

REVIEW:—

THE ELEMENTS OF MATERIA MEDICA AND THERAPEUTICS. By JONATHAN PEREIRA, M.D., F.R.S. and L.S., &c. &c. &c. Second Edition, enlarged and improved. London: Longman, Brown, Green and Longmans. 2 vols. pp. 1926. 1842. 2*l.* 10*s.*

THIS is by far the best book on the subject in the English language. The author is well known as a lecturer of great eminence and ability, and no one can be better qualified for the production of such a work as

that before us. It is impossible to peruse this excellent treatise without being struck with the extent and variety of Dr. Pereira's reading, in both English and Continental works on Pharmacy; and ably, indeed, has he given to the profession the benefit of his industry. We congratulate him on the successful manner in which be has accomplished his immensely laborious task, and on the well merited reception which "The Elements of Materia Medica and Therapeutics" has met with from the public.

It is our intention to divide this notice into two parts: the first part relating to the general objects of the work, and the subjects of the First Volume, and the second, to those of Volume II.

The general objects of the work may be best explained in the author's own language (Preface to the first edition):—

"The object of the Author, in preparing the present work for the press, has been to supply the Medical Student with a class-book on Materia Medica, containing a faithful outline of this department of Medicine, and embracing a concise account of the most important modern discoveries in Natural History, Chemistry, Physiology and Therapeutics, in so far as they pertain to Pharmacology. These subjects he has treated of in the order of their natural-historical relations. This method he has followed for many years past in his lectures."

This second edition contains 500 pages more than the first; among which will be found "articles on Mental Impressions, Light, Heat, Cold, Electricity, Magnetism, Diet, Climate, and Exercise, considered as therapeutic agents. The processes of the British Pharmacopœias (including those of the new Edinburgh one) have been described in a more detailed manner. Upwards of a hundred wood-cuts have been added. They comprise figures of crystals, and of some insects used in medicine; microscopic views of the amylaceous grains of commerce; and illustrations of chemical manufactures, and of the modes of preparing some vegetable products. The text has been disencumbered of the references, which have been thrown into the form of foot-notes—and, in the typographical part, various improvements have also been effected." He informs us that the Index (an excellent one) was carefully prepared by Mrs. Pereira, to whose pencil, also, he is indebted for the figures of the magnified grains of fecula.

The first part of Vol. I. is occupied with the subject of general therapeutics, the second with the special pharmacology of the Inorganised Kingdom.

Under the head of "General Therapeutics," our author treats of "Psychical or Mental Remedies; of Somatical or Corporeal Remedies, including Light, Heat, Cold, Electricity, Magnetism; of Hygienic Agents,—Food, Dietetical Regimen, Exercise and Climate; and of Mechanical and Surgical Agents; of Pharmacological Agents, or Medicines." In this last subdivision he speaks of the means of ascertaining the effects of medicines; of their active forces; of their physiological effects; of their absorption; of their operation by nervous agency; of the parts affected by their remote action; of the general nature of their effects; of the circumstances which modify their effects; of the therapeutical effects of medicines; of the parts to which they are applied; of pharmacological classification; and of the physiological classes of medicines.

The second part includes the natural-historical arrangement, and proceeds to treat of the pharmacology of the Inorganised Kingdom, subdivided into two classes, comprising the non-metallic and metallic substances.

This volume contains much that has never before been published in this country: it is replete with the most valuable information; and so ample is the matter for extract, that we are fairly at a loss what to take; every thing is excellently treated of. We select, for the gratification of the reader, rather than as a specimen, the author's remarks on the therapeutical employment of

"VOLTAÏC ELECTRICITY."*

(Galvanism; Voltaism.)

"The apparatus usually employed for the medical application of voltaïc electricity consists of—

"1. Two wooden troughs, (devised by Mr. Cruikshanks,) each containing 50 pairs of copper and zinc plates, 2 or $2\frac{1}{2}$ inches square. These may be charged with a solution of common salt, or a weak acid liquor. Some electricians employ 1 part of common muriatic acid, and 16 or 20 parts of water. Singer thinks that $\frac{1}{200}$ of muriatic acid will be found the most useful. In some cases where the skin was very susceptible I have used water only."

"2. A pair of insulated directors, each consisting of a glass tube traversed by a copper wire. One extremity of the wire is in communication with one end of the trough; the other extremity is covered with sponge or flannel, moistened with a solution of common salt."

"3. Copper wire to connect the directors with the ends of the troughs."

"*Harrington's Electrizers* are plates of copper and zinc, or silver and zinc, made in

* Page 40.

various forms. Thus for the toothache a plate of copper is soldered edgeways to a piece of zinc, and worn in the mouth: the saliva serves to excite the apparatus. In another contrivance, an hexagonal plate of zinc is connected by its face to a plate of silver; and a series of these compound plates are connected together by wire, so as to move on each other like hinges. These are worn next the skin for the relief of rheumatism. The perspiration serves to excite the plates. Silver and zinc spangles also have been employed, instead of the plates just mentioned."

"PHYSIOLOGICAL EFFECTS.—The physiological effects of voltaïc electricity are three-fold, viz.—

"1. The production of certain sensations.

"2. The contraction of muscular fibre.

"3. An influence over secreting organs."

"1. *Production of certain Sensations.*—Although electricity acts on all the organs of sense, yet the nerve of each sense is affected in a manner peculiar to itself. Thus, by acting on *the nerves of touch*, we produce pain, the shock, and other disagreeable sensations: by affecting *the optic nerve*, we occasion a sensation of light: by influencing *the gustatory nerve*, a remarkable taste is excited: by affecting *the auditory nerve*, a peculiar sound is excited:* *the olfactory nerve* is influenced by electricity with more difficulty. Volta could not succeed in producing an effect on the sense of smell; which he ascribes to the circumstance of the electric effluvia not being expended in and conveyed by the air, which, it is thought, is the proper vehicle for exciting sensations in the olfactory nerves. Cavallo† and Ritter‡ each assert, however, that they have produced peculiar smells by electricity."§

"The sensations excited by the passage of the voltaïc current through the sensitive nerves may be owing to the mechanical or chemical influence of the current, and not any thing peculiar to the electricity. Thus the nerves of the touch, the optic nerve, and the auditory nerves, have each their special sensations excited by mechanical violence. The acid or the alkaline taste produced by electricity, may be referred to the electrolysis of the salts of the saliva, and the development of an acid and an alkali at the opposite electrodes; and the metallic taste may be owing to the chemical action of the constituents of the saliva on the electrode, by which a soluble metallic compound is produced."

"2. *Contraction of Muscular Fibre.*—Voltaïc electricity excites muscular contractions when applied to the motor nerves, or to the central organs of the nervous system. The effect is produced not only on the living, but on recently killed, animals; and is more powerful on the voluntary, than on the involuntary, muscles."

"MM. Prevost and Dumas* have proposed an electrical theory of muscular contraction, which appears to me to be disproved by anatomical and physiological, as well as by physical considerations. They assert that the nervous fibres run transversely across the muscular fasciculi; that when the muscular fibres become shorter by contraction, they do so by assuming a zig-zag inflection; that the nervous fibres are conductors of a voltaïc current; and, lastly, that zig-zag inflection is produced by the mutual attraction of the parallel rectilineal currents in the nerves—the muscular fibre itself being passive. But not one of these assumptions can be admitted. Schwann† has shown that Provost and Dumas mistook entire nervous fasciculi for primitive nervous fibres. Professor Owen and Dr. A. Thompson‡ doubt whether the zig-zag inflection exists during contraction. Prof. Owen says that the fibres become shorter and thicker, and only assume a wavy as zig-zag arrangement has ceased. Lastly, we have yet to learn how the voltaïc current is insulated in the nerves, and prevented passing off laterally; for the neurilema, and the other soft tissues, are excellent conductors of electricity."§

"3. *Influence over the Secreting Organs.*—Of the great influence exercised by the nerves in the process of secretion, no doubt can be entertained. Now it appears highly probable, that as the voltaïc current excites the functions of the sensitive and motor nerves, it may also exercise a similar in-

* "Volta's *Phil. Trans.* for 1800, p 403."

† "Wilkinson's *Elements of Galvanism*, vol. i. p. 223. London, 1804."

‡ "Müller's *Elements of Physiology*, translated by Bayly, vol. i. p. 623. London, 1838."

§ "The peculiar smell evolved by working the ordinary electrical machine in the atmosphere,—by electric sparks, and in some electro-chemical decompositions,—is ascribed by Schönbein to a new elementary substance, which he terms *ozone* (from ὄζω, I smell,) and which is evolved at the anode or positive surface. He supposes it to be a constituent of an electrolyte, small quantities of which exist in both air and water.—*Athenæum*, for 1840, p. 742."

* "Edwards, *De l'Influence des Agens Physiques*, p. 531. Paris, 1824."

† "Müller, *Op. supra cit.* p. 900."

‡ "*Ibid.* p. 887."

§ "*Ibid.* p. 635."

fluence over those nerves which are distributed to the organs of secretion. Dr. Wilson Philip* has endeavored to establish the truth of this opinion in the case of the secretion of the gastric juice. He divided the nervi vagi in a rabbit, and found, as he supposed, that the digestive process was stopped. In another experiment he restored, as he tells us, the functions of these nerves by the voltaïc influence. But subsequent experiment has shown that the division of the nervi vagi does not wholly stop the digestive process, and that electricity cannot restore it to its original state.†

" Uses.—The therapeutic uses of voltaïc electricity, like those of common electricity, are partly rational, partly empirical.

" 1. *To stimulate the Sensitive Nerves.*—In cases of nervous *deafness*, voltaïc electricity has been used to stimulate the auditory nerve. For this purpose, one wire, (pole or electrode,) is introduced into one ear, and the other wire, (pole or electrode,) into the opposite ear. The circuit is then to be rapidly broken and completed a number of times. In *amaurosis*, the same remedy has been used empirically, to stimulate the retina, when other remedies have failed. It must, however, be employed with great caution, as its mechanical effect is calculated in many cases to aggravate the malady.

" 2. *To excite the Motor Nerves.*—In *paralysis*, voltaïc electricity is occasionally resorted to, but for the most part empirically. It can, of course, be of no avail if the disease arise from organic changes in the nervous centres. But when the malady appears to be functional only, or when there is reason to suppose that the blood effused in the brain has been absorbed, and that the paralysis remains from desuetude only, stimulating the motor nerves by electricity may, perhaps, prove serviceable. In *asphyxia* from drowning, hanging, the inhalation of noxious gases, &c. voltaïc electricity is occasionally employed to excite the muscles of respiration. It appears to be a very plausible remedy; but in the cases in which it has hitherto been tried on the human subject, it has mostly failed to effect resuscitation.‡ In *sanguineous apoplexy*, Dr. W. Philip suggests that it might be used to enable the lungs ' to perform their functions for a longer time than without this aid,' and that by it the life of the patient may be prolonged. For the asphyxia produced by *concussion*, galvanism has been suggested by M. Goudret."

3. " *In Asthma and Dyspepsia.*—Dr. Wilson Philip, having observed that withdrawing a considerable part of the nervous influence from the stomach and lungs deranges the digestive powers, and produces great difficulty of breathing, was led to expect relief from galvanism in indigestion and habitual asthma. He describes the benefit obtained as greatly exceeding his expectations. The positive pole (*anelectrode*) is applied to the nape of the neck,—the negative pole (*cathelectrode*) to the pit of the stomach. A weak power should be commenced with, and the strength gradually increased until some uneasiness is experienced. In some instances perfect cures were obtained; in others, relief was gained."*

4. To electrolyse urinary calculi. * * *

5. To coagulate the blood within an aneurismal tumor. * * *

6. To cauterise. * * *

" 7. *To promote Absorption of Medicinal Substances.*—In 1832, Dr. Coster,† and in 1833, M. Fabré-Palaprat,‡ employed voltaïc electricity to assist the introduction of certain medicinal substances into the blood. They adopted Sir H. Davy's§ opinion (subsequently shown by Mr. Faraday‖ to be erroneous), that the poles (*electrodes*) of a voltaïc battery have attractive and repulsive powers for certain substances; the positive pole (*anelectrode*) for oxygen, chlorine and iodine,—the negative pole (*cathelectrode*) for hydrogen and the metals. M. Fabré-Palaprat asserts, that by the aid of galvanism, he has caused certain chemical agents

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* " *An Experimental Inquiry into the Laws of the Vital Functions*, pp. 111, 213, 256, &c., 3rd Edit. Lond. 1826."

† " Müller, *Op. ante cit.* p. 549."

‡ " The Professors of the Irish College of Surgeons, in 1829, failed to restore by it the respiratory movements in a person who had been hung, (Dr. Apjohn, in *Cyclopædia of Practical Medicine*, art. *Galvanism*).—Electricity, in conjunction with other means, was tried, but without success, in the case of Scott, the American diver, who had accidentally hung for five or six minutes, (see *Times*, Jan. 13, 1841)."

* " See *Phil. Trans.* 1817, p. 22; and Dr. Wilson Philip's *Treatise on Indigestion.*—Also, Le Beaume, *On the Medical Effects of Electricity and Galvanism in Nervous and Chronic Disorders.* 1820."

† " *Archives Générale de Médecine*, p. 57. Paris, 1828."

‡ ' *Ibid.* IIme. Série, t. ii.—Also, Becquerel, *Traité de l'Electricité*, t. iv. p. 321."

§ " *Phil. Trans.* 1807, p. 1."

‖ " *Ibid.* 1838 and 1834."

to traverse the body, and appear at some distant part. He bound on one arm a compress, moistened with a solution of iodide of potassium, and covered by a platinum disk, connected with the negative pole (*cathelectrode*) of a voltaïc battery of thirty pairs of plates. On the other arm was placed a compress, moistened with a solution of starch, and covered by a platinum disk, connected with the positive pole (*anelectrode*) of the battery. In a few minutes the starch acquired a blue tinge, showing that the iodine had been transported from one arm to the other. But Davy's idea that the poles (*electrodes*) possess attractive and repulsive properties is not correct, as I have before remarked. That electricity may promote absorption, either by increasing endomosis, or by acting as a stimulus to the blood-vessels and lymphatics, is not improbable; but that the poles (*electrodes*) can draw medicinal substances either into or out of the body, is not true. I have twice repeated Fabré-Palaprat's experiment; but, though I employed fifty pairs of plates for fifteen minutes, I was unable to obtain the slightest evidence of the passage of iodine through the body."

"ELECTRO-PUNCTURE."

(Galvano-Puncture.)

"The operation of electro-puncture was proposed by Sarlandière,* in 1825. It consists in introducing two acupuncture needles in the usual way, and connecting them with the poles of a weak voltaïc battery; the contact being occasionally suspended and renewed, in order to produce a succession of shocks. This practice has been successfully adopted for the relief of rheumatism, neuralgia, local paralysis, sciatica, spasmodic affections, and other maladies in which the operation of simple acupuncture has been used,—than which it has been thought, by some, to be more efficacious. In neuralgia and rheumatism it should be employed only in the interval of the paroxysms.† M. Bourgeois‡ proposed to employ the operation of electro-puncture of the heart, to promote resuscitation, in cases of asphyxia."

"MAGNETIC ELECTRICITY."

"The apparatus required for the medicinal application of magnetic electricity consists of—

"1. A magneto-electric machine."

"2. A pair of directors."

"The most convenient, simple, and powerful magneto-electric machine is that devised by Mr. E. M. Clarke, of the Strand. It consists of a battery of six curved permanent magnets, and an intensity of armature, around whose cylinders 1500 yards of fine insulated copper wire are coiled. The ends of this wire communicate respectively with a pair of directors, each holding a piece of sponge (dipped in vinegar or a solution of common salt). When the armature is rotated, and a portion of the living body interposed between the directors, a succession of shocks is received."

"A magneto-electric machine is not affected by the moist state of the atmosphere: this gives it an advantage over the common electric machine; and as acids are not required to excite it, one inconvenience of the voltaïc battery is obviated."

"It is employed in medicine as a convenient substitute for the ordinary voltaïc battery."

We may add the following:—

"MAGNETISMUS.—MAGNETISM."

(Mineral Magnetism.)

"Ætius,* who lived about A. D. 550, is the oldest author who mentions the application of magnetism to the cure of diseases; for, although Hippocrates† speaks of the magnet as a remedial agent, he refers to its internal use only. About the end of the seventeenth century, magnetic tooth-picks and ear-picks were made as secret preventives against pains in the teeth, eyes, and ears."‡

"The power of a magnet to affect the vital function is not generally admitted in this country; but it must be remembered that the experience which British practitioners have had of its use is exceedingly limited. Beckers says that the sensations which his patients experienced from the use of the magnet were, (probably from the coldness of the steel,) heat; this is the most frequent effect, especially in the ears, and it often amounts to unpleasant burning; *traction*, (from the slightest degree, when it is almost painful, like that of a cupping-glass); *throbbing; pain;* and *numbness* or *loss of feeling in the magnetised part*. Some years ago Mr. Faraday allowed Dr. Keid to try his magnets in every way he thought proper,

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* "*Mémoires sur l'Electro-puncture.* Paris, 1825."

† "Trousseau and Pidoux, *Traité de Thérapeutique*, t. i. p. 579. Paris, 1836."

‡ "Quoted by Merat and De Leons, in the *Dict. Univ. de Mat. Méd.* art. *Electro-puncture.*"

* "Sermo ii. n. cap. 25."

† "*Opera; De Intern. Affect.* p. 543; and *De his quæ Uterum non gerant*, p. 686, ed. Fœsii."

‡ "Beckmann, *History of Inventions and Discoveries*, vol. i. p. 74."

on himself (Mr. F.), but without any effect resulting.* In some instances it has appeared to exercise a most remarkable influence over neuralgic pains and spasmodic affections; at one time apparently curing, at another palliating, and occasionally augmenting all the patient's sufferings. But, in a large proportion of cases, it fails to produce an obvious effect. The employment of magnetic plates is sometimes attended with itching, and an eruption of pimples. Toothache, neuralgia, painful affections of the stomach, rheumatic pains, spasmodic asthma, angina pectoris, and palpitation of the heart, are the maladies which have occasionally appeared to be relieved by the magnet. It is said that, in some cases, neuralgic pain is alleviated by the application of the North pole of the magnet, but is augmented by the South pole.† Laennec‡ speaks highly of the efficacy of magnetised plates in neuralgia of the lungs, and angina pectoris. He applied two strongly magnetised oval steel plates—one to the left precordial region, the other exactly opposite on the back, so that their poles were opposed. He says the relief is increased, if a blister be applied under the anterior plate. The late Dr. Thomas Davies§ tried this plan, and with good effect."

"There are several modes of using magnets. For tooth-ache, a *simple straight* or *bar magnet*, sometimes called *a magnetic staff*, is used. It is first made warm, and its North pole applied to the tooth: if the pain be not relieved, the South pole should then be substituted. Or the poles are applied to, or passed over, the gums or cheeks. In neuralgic pains, *a compound magnet*, called *a magnetic battery*, is commonly employed. This consists of several curved (horse-shoe, lyre-shaped, or U-shaped,) magnets, placed one over the other, with all their poles similarly disposed, and fastened firmly together. Dr. Schmidt|| employed a battery of five magnets of unequal length, the centre one being the longest and thickest. This kind of battery is usually called by workmen *a magnetic magazine*. *Magnetic collars*, *girdles*, *bracelets*, &c. are made of several artificial magnets, with their opposite poles in contact, inclosed in linen or silk. *Magnetised steel plates* (magnetic *plates*), of various forms, are fitted to any part of the body. They are applied to the naked skin, and worn by the aid of a bandage.* To attempt to explain the *methodus medendi* of an agent whose therapeutical influence is not generally admitted, appears to be somewhat premature. I may remark, however, that should the existence of *electro-vital* or *neuro-electric* currents in the animal body, as announced by Prof. Zantedeschi and Dr. Favio,† be hereafter fully established, we shall have a ready explanation of the medicinal power of magnetism in the well known influence of a magnet over a voltaïc current.‡"

We recommend this work to the chemist and druggist who has a laudable desire to obtain a proper knowledge of the nature and therapeutical action of the drugs in which he deals, feeling confident that by a careful study of it, he will be rendered far more capable of exercising that privilege of which he is so tenacious, and which the whole trade so lately exerted themselves most vigorously to defend and maintain—the right of prescribing under certain circumstances. Of course that right must be based on COMPETENCY; and if it be not so in all cases, those who constitute exceptions to the rule should assiduously apply themselves to acquiring a sufficient knowledge of those branches of science, an acquaintance with which alone can entitle them to public

* "*Lancet*, for 1835-36, vol. i. p. 716."

† "*Ibid.* 1832-33, vol. ii. p. 312."

‡ "*A Treatise on Diseases of the Chest*, translated by Dr. Forbes, pp. 402 and 693. Lond. 1827."

§ "*Lectures on the Diseases of the Lungs and Heart*, p. 497. Lond. 1835."

|| "*Lancet*, for 1835-36, vol. i. p. 338."

* "Figures of the different forms of magnetic instruments here referred to are given by MM. Andry and Thouret, in their very elaborate and able article on *Medical Magnetism*, in the *Mémoires de la Société Royale de Médecine*, Année 1749, p. 531."

† "*Report on the Memoir on Electric Currents in Warm-blooded Animals*, by Prof. Zantedeschi and Dr. Favio, presented to the Royal Academy of Sciences of Brussels on the 4th of April, 1840. By M. Cantraine. In *Lond. Edinb. and Dub. Phil. Mag.* for April, 1841."

‡ "For further information respecting magnetism, as a therapeutical agent, I must refer to Andry and Thouret's Memoir before quoted; as also to Dr. Recker's *Der mineralische Magnetismus und seine Anwendung in der Heilkunst*, Mühlhausen, 1829; Dr. Bulmerincq's *Beiträge zur ärztlichen Behandlung mittelst des mineralischen Magnetismus*, Berlin, 1835; and Dr. Schnitzer's *Ueber die rationelle Anwendung des mineralischen Magnetismus*, Berlin, 1837. Also, Most's *Encyklopädie der gesammten medicinischen und chirurgischen Praxis:* art. *Magnetismus mineralis*, 2er Band, S. 394. Leipsig, 1837."

confidence, or raise them to eminence among their brethren. They should fly to the work before us; they should carefully and diligently peruse it. They will find it a most valuable assistant in all cases of difficulty; no compound can be prescribed which is not fully treated of in it. The physician, the surgeon, the apothecary, the scientific chemist, and the man of science generally, will derive the greatest advantage from it: as a work of reference it is unsurpassed, nay, unequalled, by any on the subject in our language.

The subjects of the Second Volume will be noticed in our next.

ANTI-EPILEPTIC MEDICINE.*

BY DR. SEIDEL.

Dr. Seidel, of Breslau, states that the following was communicated to him by a pastor of Lower Silesia, who had long gratuitously supplied the remedy to the epileptic inhabitants of that country. This means consists in the *snuff of a tallow candle*, which is triturated with a small quantity of powdered *calamus aromaticus* and cinnamon, and which compound he (the pastor) administered for *three* consecutive *new moons*, each time for *three* days, and *three* times in the twenty-four hours, in the dose of from half a teaspoonful to about a teaspoonful.

Dr. Seidel thought that the empyreumatic parts and the small portion of hydrocyanate of ammonia which are found to be contained in vegetable and animal charcoal, might be considered as the active principles of this agent, which he tried, with or without regard to the phases of the moon, according to the feeling of the patients with regard to the importance of this particular, sometimes with advantage, sometimes, on the contrary, without the slightest success.

[What, in the name of all that is extraordinary and ridiculous, shall we have next? Our forefathers are outdone: their powders of crab's eyes and dead men's skulls must sink into nothing before the SNUFF OF A TALLOW CANDLE! "That it should come to this!" Who can read this article without being struck with the systematic precision of the worthy pastor in the application of this "medicine?" We very much regret that he has not favored the world with his opinion as to the additional efficacy imparted to it by the method in which he administered it. We admire the intelligence which prompted him to pay such profound respect to the lunar influence and to the magic powers of the cabalistic number THREE! What a pity that such talent should be doomed to the gown and cassock, instead of the pestle and mortar. He should have been at least an Examiner at Apothecaries' Hall. We fear that the learned Dr. Seidel did not pay sufficient attention to the mode of exhibition directed by the pastor: we cannot but feel that the universal success of the latter was the natural result of his strict regard to the phases of the moon and the number THREE (how we respect that number!). What a splendid addition will this make to the LONDON PHARMACOPŒIA!!! *Ellychnium officinale* will, we suppose, be the name given to it by the College.]

* *Berliner Medicinische Zeitung*, No. 41, 1841.

NEW METHOD OF ADMINISTERING SULPHATE OF QUININE.*

BY DR. GIOVANNI GUASTAMACCHIA.

Dr. Pointe proposed to apply sulphate of quinine by friction on the gums; Dr. Schweinsberg, to give it in combination with an aromatic powder to correct its extreme bitterness; Dr. Gerhardt employed it by the endermic method; M. Schuster dissolved it in alcoholised sulpburic ether, in order afterwards to be used in friction on the epigastrium; M. Dassit prescribed it in frictions under the armpits; Dr. Confani associated it with sulphuric acid to facilitate its administration. Finally, Dr. Guastamacchia now proposes a new method of employing this salt in the curative treatment of periodical pyrexiæ.

This method consists in dissolving sulphate of quinine in rectified spirit, in the proportion of 40 centigrammes (8 grammes) of salt to 15 grammes (4 drachms) of menstruum; and in employing the solution for making two frictions, a quarter of an hour apart, throughout the whole length of the vertebral column. The best time for practising this double friction is just when the shivering commences.

The author affirms,—without giving, however, any scientific reason for this preference,—that he has obtained the best results from this mode of exhibition.

MEANS OF ACCURATELY DETERMINING THE DOSE OF QUININE REQUIRED FOR CURING INTERMITTENT FEVER.†

Dr. Hille, physician to the Netherlands army, at Surinam, has ascertained, from

* *Il Filiatre Sebezio*, August, 1841.

† *Wochenschrift für die Gesammte Heilkunde*, No. 6, 1842.

long practice, that the preparations of quinquina act effectually in the intermittent affections which predominate in that country, only when prescribed in large doses, for example, from 60 centigrammes to 2 grammes *per diem*. A certain sign, according to him, that the dose of these preparations is sufficient, is a tingling in the ears, of which the patients never fail to complain. When this sign presents itself, the use of the medicine may be boldly discontinued, without fear of a relapse; at any rate, this is quite an exception. If, however, the fever is subdued without this tingling having been produced, relapse is almost certain.

EMPLOYMENT OF THE MAGNET.*

There has lately been established in the workshops of Fairbairn (Belgium), an artificial magnet of great power, placed at the height of the eye. Every moment there may be seen running to this magnet either a turner or an "adjuster" who has received a small piece of iron in the eye: the magnet draws it out as soon as the eyelid is opened.

It is conceived that a magnet capable of raising 1000 kilogrammes might attract even a piece of iron implanted in the flesh and incrusted in the bone.

All places where they work in iron should be provided with so useful an apparatus.

PLASTER OF CROTON OIL.†

BY M. BOUCHARDAT.

Croton oil acts as a valuable repulsive in many cases. Applied to the skin, it produces a very considerable vascular eruption, but much less painful than that of the pustules caused by the employment of stibial tartar under the form of frictions. It is usually prescribed pure, or mixed with oil of sweet almonds. This means of administration is inconvenient; for crotonic acid, the active principle of the oil, is volatile, and is often dissipated without producing any effect, or else causes inflammation of the skin of the fingers used in rubbing. To avoid these inconveniences, M. Bouchardat prepares croton oil in the following manner:—

He melts, on a gentle fire, 80 grammes of diachylon plaster, and mixes with this semifluid plaster, 20 grammes of croton oil; he then spreads the plastic mass on calico, so as to obtain a very adhesive croton oil plaster, which causes a powerful irritation of the skin.

M. Caventou, in a note in reply to M. Bouchardat's Memoir, wished to establish his right to the propagation of croton oil as a therapeutical agent. He thinks that the heat to which the oil is submitted at the time of mixing must injure its properties, which would be true if M. Bouchardat did not add the croton oil to the liquefied and partially cooled plaster. M. Caventou prefers the following formula:—

R Hog's lard .	2½ parts
Wax . .	½ part
Croton oil .	1 part

The wax and lard are melted together, and the croton oil is incorporated when they are cold.

* *Journal de Chimie Médicale*, June, 1842.

† *Bulletin de Thérapeutique.*

BOOKS RECEIVED.

Elements of Electro-Metallurgy; or the Art of Working in Metals by the Galvanic Fluid. By Alfred Smee, F.R.S., M.R.C.S., &c. 2nd Edition. Part III. June 1, 1842. London: Palmer, Newgate Street; Longman and Co., Paternoster Row.

The London and Edinburgh Monthly Journal of Medical Science. Edited by John Rose Cormack, M.D. (In Exchange.)

TO CORRESPONDENTS.

Phthysis Pulmonalis.—The correspondent who has adopted this forbidding subscription is recommended to use the nitrate of nickel. We cannot give all that he requires; it would occupy too much space. We cannot call to mind the former letters of which he speaks.

A Constant Reader is referred to the forthcoming number of the Proceedings of the London Electrical Society, which will doubtless contain the paper in full.

Ἄλφα.—We are fearful that if we were to insert such articles as our correspondent has sent, we should render ourselves has sent to the law of copyright.

Erratum.—No. XXX. page 178, col. 1, line 17, *for* "free oxide," *read* peroxide.

Nota Bene.—*All Communications and Books for Review must be addressed, "To the Editors of the Chemist, Care of Mr. Hastings, Publisher, 13, Carey Street, Lincoln's-inn Fields, London." Communications must be prepaid, and sent before the 15th of each Month; Books for Review before the 10th.*

THE CHEMIST.

I. CHEMISTRY.

ON THE PREPARATION AND EMPLOYMENT OF CYANURET OF POTASSIUM.*

BY JUSTUS LIEBIG.

One of the best methods of preparing cyanuret of potassium consists, as is known, in the decomposition of ferrocyanuret of potassium at a red heat; but it is attended with several inconveniences, and one third of the cyanogen which this salt contains is lost. Formed of two atoms of cyanuret of potassium and one atom of cyanuret of iron, it does not undergo any change by red heat in its first constituent; but the last is decomposed into carburet of iron with a disengagement of nitrogen. The carburet of iron absorbs the cyanuret of potassium in fusion like a sponge, and we are compelled to resort to the use of solvents, especially alcohol, to obtain the cyanuret of potassium produced, without iron and without loss.

Now, as cyanuret of potassium possesses properties which make it an exceedingly valuable means of reduction and separation in chemical analysis, I have endeavored to simplify its preparation.

If we dry (by slight calcination on an iron plate) eight parts of ferrocyanuret of potassium, and afterwards mix them intimately in fine powder with three parts of dry carbonate of potassa, and throw them at once into a Hessian crucible previously heated to a dull red, and keep them at that temperature, the mixture at first melts into a brown flux, with a rapid disengagement of gas; at the end of about ten minutes, when the fluid mass has been brought to a red heat, the deep color is seen to become lighter, and by continuation of the fusion, it becomes of a light amber color; if a hot glass rod be from time to time introduced into it, the portion which adheres to it when it is drawn out, at first remains brown after solidification; it afterwards becomes yellow; and in the last place, at the end of the operation, the liquid, which adheres to the glass rod, is clear and colorless as water, and forms itself into a crystalline mass of a brilliant white.

During the fusion, brown flocks may be seen to float in the mixture, which ultimately combine under the form of a sponge, and assume a light grey color. If the crucible be then removed from the fire and allowed to cool gently, the grey powder is generally completely deposited at the bottom; this deposition is facilitated by stirring once or twice with a glass rod. The fused and hot mass which floats at the top, may then very easily be decanted into a hot porcelain capsule, without removing a grain of the powder deposited.

In the mass separated from the iron by decantation, there is a mixture of two compounds, formed chiefly of cyanuret of potassium; the other combination is cyanate of potassa. They exist in the proportion of five atoms of cyanuret of potassium to one atom of cyanate of potassa.

The following is what occurs in the fusion of the ferrocyanuret of potassium with carbonate of potassa:—

At the commencement of the fusion, the cyanuret of iron of the ferrocyanuret of potassium is decomposed with the potassa of the carbonate of potassa into cyanuret of potassium and carbonate of protoxide of iron, from which the cyanuret of potassium removes all the oxygen at a more elevated temperature; in consequence of this reduction, cyanate of potassa and pure metallic iron are obtained.

If we suppose in the mixture two atoms of ferrocyanuret of potassium and two atoms of carbonate of potassa, we have:—

* *Annalen der Chemie und Pharmacie*, June, 1842.

Ferrocyanuret of potassium. Carbonate of potassa.

$$Cy^{12}Fe^2K^4 + K^2O^2 2CO^2 = Cy^{12}Fe^2O^2 2CO^2;$$

and we have after fusion:—

Cyanuret of potassium. Cyanate of potassa. Iron. Carbonic acid.

$$Cy^{10}K^5 + Cy^2O, KO, \quad Fe^2, \quad 2CO^2.$$

We obtained from two atoms of ferrocyanuret of potassium, five atoms of cyanuret of potassium; consequently, one fourth part more than by fusion of this salt at a red heat. The cyanate of potassa, with which it is mixed, does not injure any of its uses; its presence is easily detected by the supersaturation of this cyanuret of potassium with an acid; indeed, an effervescence ensues, owing to a disengagement of carbonic acid, and there is then found in the liquor an ammoniacal salt.

The explanation of the formation of cyanuret of potassium in the conditions indicated, is not perfectly accurate, because the carbonate of protoxide of iron, which is formed, is decomposed before its reduction into carbonic acid, carbonic oxide, and oxide of iron; and it is at the expense of the latter that an undeterminable quantity of cyanate of potassa is formed, even more than that indicated by the foregoing formula.

The metallic iron remaining, as well as the sides of the crucible, are covered with cyanuret of potassium; the most advantageous process of extracting it consists in removing from the crucible with cold water all that is soluble, and in heating the solution obtained from the cyanuret of potassium with a little sulphuret of iron, which dissolves in it with great facility.

The cyanuret of potassium is extracted from this solution by evaporation, in the state of ferrocyanuret; sulphuret of potassium remains in the supernatant liquors.

PREPARATION OF HYDROCYANIC ACID.

This cyanuret of potassium is much better adapted for the preparation of hydrocyanic acid, and a much more abundant product is obtained from it, with much greater facility in distillation.

There is deposited, as is known, in the distillation of the ferrocyanuret of potassium with dilute sulphuric acid, a blueish white powder—a compound of cyanogen, potassium and iron, whose composition is analogous to that of the ferrocyanuret of zinc, and is expressed by the formula:—

$$2\ Cf\ 7 + 3\ Fe \left.\begin{matrix} K \end{matrix}\right\} (Cfy = Cy^8\ Fe).$$

From the formation and composition of this body, it results that from 5 atoms of ferrocyanuret of potassium, which contain 30 atoms of cyanogen, we cannot obtain more hydrocyanic acid than from 9 atoms of cyanuret of potassium, namely, only 18 atoms of hydrocyanic acid; the other 12 atoms remain in the blueish white ferruginous precipitate.

If the ferrocyanuret of potassium be converted into the cyanuret by the method indicated, from 5 atoms of ferrocyanuret, 25 atoms of hydrocyanic acid are obtained, that is to say, 7 atoms more.

It is the custom to prescribe for 1 atom of ferrocyanuret of potassium, for its decomposition by sulphuric acid, a proportion of the latter which is sufficient for forming with the alkali sulphate of potassa; in the employment of cyanuret of potassium, only one atom of hydrated sulphuric acid is required.

Equal parts of cyanuret of potassium and hydrated sulphuric acid are the best proportions for the preparation of hydrocyanic acid. The sulphuric acid is sufficient for forming with all the potassa neutral sulphate of potassa, and acid sulphate of ammonia with the ammonia produced by the decomposition of the cyanate of potassa. The cyanuret of potassium is dissolved in twice its weight of water, and the sulphuric acid, diluted with three times its weight of water, is slowly added by small portions; each addition should be made after the effervescence which is produced has stopped.

PREPARATION OF CYANATE OF POTASSA.

The cyanuret of potassium (always prepared according to the method described) presents an excellent means of easily procuring, with very little loss, cyanate of potassa. In this process preference should be given to ordinary litharge, previously heated to dull redness. The cyanuret of potassium is fused in a Hessian crucible, and powdered litharge is added to it, little by little; the oxide of lead is immediately reduced, and the metal at first remains mixed, under the form of a fine powder, with the cyanate of potassa produced; but by greater heat it runs together. The fused matter, which is the cyanate of potassa, is reduced to a fine powder, and boiled with alcohol until no more crystals are obtained after the cooling of the solution. The crystallisation of the salt of potassa in alcohol is not necessary for the preparation of urea.

CYANURET OF POTASSIUM AS A MEANS OF REDUCTION.

It is not easy to form an idea of the extreme facility with which the cyanuret of potassium removes the oxygen or the sulphur from certain metallic oxides or sulphurets;

for it is in this property that it most resembles pure potassium.

The preparation of cyanuret of potassium, and of cyanate of potassa, furnishes two examples of this reductive power. The oxides of iron fused with cyanuret of potassium are reduced with great facility; the iron remains mixed in the state of powder with the cyanate of potassa in fusion, or else agglomerates in the form of a sponge.

On this reduction may be based a process for discovering, by the dry way, in a single operation, the proportion of metal in an ore of iron. If we expose to a strong red heat, in a porcelain crucible, a weighed quantity of ore with a mixture of cyanuret of potassium, and carbonate of potassa, alumina and silicic acid pass into the flux, and the reduced iron may be separated by washing with cold water and weighed. Protoxide of manganese is not reduced by the cyanuret of potassium; if it were mixed with an ore of iron, it would be necessary to determine it in a special operation.

If oxide of copper be thrown on to cyanuret of potassium in a state of fusion, it is very soon reduced with a developement of light and heat; after washing, a compact lump of pure copper is obtained.

The most beautiful reductions are those of the oxides of tin and antimony. By a weak red heat, the oxide of tin is changed into a shining regulus, which may be separated from the scoriæ under the form of a well fused ball; and it is in the same manner that deutoxide of antimony or antimonious acid may be reduced to the metallic state.

All these reductions are operated at a gentle red heat, not visible in the light; hence the peculiar advantage of not losing by volatilisation any of the reduced metals.

The sulphurets of tin and antimony are reduced by gentle fusion with cyanuret of potassium, before the blowpipe, as in the porcelain crucibles, with quite as much facility as the corresponding oxides. The flux contains sulpho-cyanuret of potassium. However, it is not only by the dry way that the cyanuret of potassium possesses reductive properties, but also in the state of solution. If, for example, we mix with its solution another of alloxan, in a few seconds, a heavy crystalline precipitate, of dialurate of potassa, sparingly soluble in water, is formed.

CYANURET OF POTASSIUM AS A MEANS OF SEPARATION.

Nickel, cobalt, and manganese, are so much alike, as is known, in their properties, that a strict quantitative separation of these metals presents great difficulty.

It is only under a single form of combination that nickel differs from cobalt, in such a manner that it may be used as an absolute means of separation. Heated with cyanuret of potassium and an excess of hydrocyanic acid, oxide of cobalt, or a salt of cobalt, chloride, &c., is converted into cobalto-cyanuret of potassium, the solution of which in water does not undergo the slightest decomposition by boiling with hydrochloric, sulphuric, and nitric acids, as Gmelin's observations have shown.

The oxide and the salts of nickel are precipitated by the cyanuret of potassium; this precipitate is dissolved in an excess of this body with a yellow color, and the double combination formed of cyauuret of nickel and cyanuret of potassium is not completely decomposed by acetic acid, but, indeed, by dilute sulphuric acid, and the cyanuret of nickel is precipitated from it.

If, to a mixture of salts of cobalt and nickel, acidulated with free acid, cyanuret of potassium be added in excess, in such a manner that the precipitate formed may redissolve, we have in solution free hydrocyauic acid, cyanuret of potassium, cyanuret of nickel, and cyanuret of cobalt; the latter is immediately converted by a gentle heat into cobalto-cyanuret of potassium; if then we add, when cold, dilute sulphuric acid, three cases are presented.

If the cobalt and nickel are found in solution in the proportion by weight of 2 of cobalt to 3 of nickel, (proportions which respond to their atomic weights in the cobalto-cyanuret of nickel,) the precipitate which is formed is cobalto-cyanuret of nickel of a blueish white color. The filtered liquor contains neither nickel nor cobalt.

If the solution contains a quantity of nickel less than that corresponding to this proportion (2 of cobalt to 3 of nickel), there remains in the liquor a certain quantity of cobalto-cyanuret of potassium in solution, and the precipitate is likewise cobalto-cyanuret of nickel.

If there is more nickel in the solution, the precipitate contains a mixture of cyanuret of nickel and cobalto-cyanuret of nickel.

In the first and second cases, the precipitate formed by the addition of dilute sulphuric acid is kept in ebullition with the acid liquor in a matrass, until no more disengagement of hydrocyanic acid is observed, (or else it is directly evaporated to dryness in a sand-bath,) and then it is gently heated with carbonate of potassa or caustic potassa in excess. The cobalto-cyanuret of nickel is then decomposed into pure or carbonated oxide of nickel, which may be washed on a

filter, dried and weighed, and in alkaline liquor, which contains all the cobalt. If the latter be evaporated to dryness, adding to it a little nitrate of potassa, and if the dried residue be heated to redness, and washed with water, all the cobalt remains in the state of oxide.

This process is applicable to all the analyses of ores of cobalt, where consequently the quantity of this metal predominates. With ores of nickel, in which the quantity of cobalt is weakest, it is necessary to take the precaution of using a very great excess of hydrochloric acid for the precipitation of the metallic cyanurets dissolved in the cyanuret of potassium, and the mixture should be kept boiling for at least an hour.

Indeed the precipitate formed contains in this case a mixture of cyanuret of nickel, which is decomposed with potassium and oxide of nickel; but this cyanuret of potassium retains another portion of nickel in solution.

By boiling the precipitate with hydrochloric acid, the cyanuret of nickel is decomposed into chloride of nickel and hydrocyanic acid, which the ebullition dissipates, and which is not opposed to complete precipitation. The cobalto-cyanuret of nickel is not attacked by boiling hydrochloric acid, and we ought not, with the presence of cobalt, to reckon on a complete solution. When the odor of hydrocyanic acid is no longer perceived, the boiling has been sufficiently long continued.

Experiments for separating by boiling with mercury, the solution of the two metallic cyanurets in the cyanuret of potassium, gave a less certain result.

In this process, attention must also be paid to the following facts:—

As the cyanuret of potassium contains a certaiu quantity of cyanate of potassa, there is formed in its decomposition by a mineral acid a certain quantity of ammoniacal salt, so that after boiling and the addition of caustic potassa, ammonia is liberated, which retains a certain portion of oxide of nickel in solution. A few minutes' boiling, or a greater addition of caustic potassa, causes this oxide of nickel to be completely deposited.

The same process is applicable to the separation of cobalt and manganese: only we cannot here reckon on a complete solution of the precipitate formed by the addition of cyanuret of potassium in the mixture of the salts of the two metals. The residue is separated by the filter, and the liquor is treated as if the nickel had been separated from the cobalt.

The cyanuret of potassium may be no less advantageously employed for separating the oxide of chromium from protoxide of iron.

If we precipitate by cyanuret of potassium a mixture of these two bodies, previously saturated with sulphuretted hydrogen (the addition of a few drops of sulphuret of ammonium), in order to have the iron in the state of protoxide in the liquor, and if an excess of this cyanuret be added, the iron is immediately dissolved in the state of ferrocyanuret of potassium, and all the oxide of chromium remains.

In certain cases the cyanuret of potassium is employed with advantage for separating iron from alumine, (a small quantity of iron and much alumine,) on account of the very great solubility of the protoxide of iron, as well as of the sulphuret of that metal, and of the insolubility of alumine in the cyanuret of potassium.

This latter body deserves to be studied as a perfectly general means of separation; unfortunately, of the great number of double combinations that it forms with other cyanurets, we know only their composition, but not the manner in which they act with the mineral and vegetable acids: this study requires to be resumed.

INVESTIGATIONS CONCERNING NITRIC ACID.*

BY M. E. MILLON.

The ordinary purification of nitric acid chiefly consists in separating from it the hydrochloric and sulphuric acids; but the processes employed do not remove the nitrous acid. This acid exists in it even when the nitric acid is very dilute and quite colorless. To demonstrate the action of this portion of nitrous acid, I may observe, that if the weakest acid contain traces of it, it will precipitate the iodine from the iodides, and the sulphur from the monosulphurets; it will give the protosalts of iron a brown color, and the ferrocyanide of potassium a green one: while the same acid, if perfectly pure, decomposes the monosulphurets without rendering them turbid, does not displace the iodine from its combination with the alkaline metals, and does not color the salts of iron, nor the ferrocyanide.

I may also add that indigo, which is deprived of color by nitrous nitric acid in a certain state of dilution, retains its color in contact with very considerable quantities of pure nitric acid, and that the very intense green color which is communicated by nitric acid to certain urines in which the presence of the coloring matter of the bile is suspected, is due solely to nitrous acid.

Nitric acid containing one equivalent of water cannot be distilled without decompo-

* *Comptes Rendus*, No. 24, June 13, 1842.

sition; it also appears to me difficult to obtain it by the processes hitherto pointed out. They have never yielded me any thing but very nitrous acids of variable degrees of hydratation. It is perfectly white, and does not become colored in the light, except at a temperature of $+30°$ or $+40°$ C.

I took the greatest care to establish all the hydrates which distillation could furnish; and the regret that I felt at finding very few details in the works of authors who have studied the subject, induced me to describe, in the Memoir of which this is an extract, with careful minuteness, all the circumstances under which I operated. Besides, on this accurate determination of the hydrates depend the principal results that I shall explain.

I have been able to obtain, besides acid with 1 equivalent of water, acids with 2, 4 and $4\frac{1}{2}$. I have found it impossible to fix others by means of distillation.

The constitution of these hydrates induced me to repeat the analysis of certain nitrates. Mr. Graham had already fixed the composition of the nitrates of zinc, magnesia, copper and hismuth; but he had heen contradicted with respect to the constitution of nitrate of zinc, and his conclusions seemed rather too generalised as regards all the oxides of magnesium.

I therefore repeated the analysis of the nitrates of zinc and magnesia; I analysed the nitrates of manganese and cobalt; I also revised that of nitrate of nickel already given in works. All these nitrates contain six equivalents of water, as Mr. Graham demonstrated in some and supposed in others. But the nitrate of lime differs from the magnesian series by its nitrate as well as by its sulphate; it contains only four equivalents of water, and also forms other hydrates in which the proportion of water is still less. Although it readily parts with all its water *in vacuo*, it is capable of forming several basic nitrates by boiling with nitrate of lime.

Thus nitric acid appears little adapted for forming series in the same manner as phosphoric acid, and visibly differs from the general formula which Mr. Graham seems disposed to adopt.

The action of nitric acid on chlorate of potassa furnishes a much more efficient deoxidising agent than those which have hitherto been employed as regards certain substances, as sulphur and selenium, for example, as well as organic substances. Spongy platinum resists this action very well, even at a temperature of $+125°$ C. in presence of nitric acid and nascent chlorine. It is a fact which does not seem to be without importance to the theory of oxidation to which the action of nitric acid on metals has led me.

In the course of the investigations which I have just very briefly mentioned, I observed that weak nitric acid, perfectly pure, did not attack copper, as well as several other metals, such as bismuth and mercury. This was a new fact. We were aware that nitric acid, in a very high degree of concentration, did not attack some metals, as, for example, iron and tin; but this same dilute acid was considered to be one of their most energetic oxidising agents; it is wanting, however; and if we might establish a general law with regard to the action of pure nitric acid on the metals, it would be that it does not attack any of them, except the alkaline metals.

It will be understood that this proposition requires further developement; I will commence by the exposition of the facts which I have been enabled to observe with respect to copper.

Acid of the density of 1·070 does not attack copper at + 20. When more concentrated, it powerfully attacks it; but if a current of deutoxide of nitrogen be passed over copper covered with acid which does not act on it, or, which is still better, if a few drops of a concentrated solution of nitrite of potassa be poured into it, the action commences, and lasts for some hours, provided that the quantities of metal and acid are sufficient. When the reaction has relaxed or completely stopped, which happens after a few hours, it may be revived by a fresh addition of nitrite of potassa, and, according as a more or less abundant disengagement is required, a greater or smaller quantity of this nitrite must be added.

I endeavored to discover whether a gaseous current traversing the acid would provoke the same reaction, but I employed, without effect, currents of carbonic acid, hydrogen, oxygen and protoxide of nitrogen gases. I decomposed in the acid, carbonate and chloride of calcium, and sulphuret of potassium; carbonic acid, chlorine and sulphuretted hydrogen were disengaged without developing any action.

If this oxidation were due solely to deutoxide of nitrogen, it struck me that I ought, by suddenly suppressing the supply of this gas, to stop the action, which, indeed, I did effect by the addition of protosulphate of iron. The disengagement, in full activity, was immediately interrupted.

As I had observed that a very slight elevation of temperature effected the oxidation of the copper, even with pure acid of 1·070, I thought that the heat developed by the decomposition of the nitrite might be the cause of the reaction. I then prepared a mixture of ice and salt, in which I placed the metal and acid while the oxidation was

being rapidly carried on; the gas stopped as soon as the acid began to congeal; but on afterwards removing from the frigorific mixture the glass tube in which the experiment was performed, and allowing it again to acquire the temperature of the surrounding atmosphere (+20° C.), the oxidation very soon recommenced, and I was thus able, with the same metal and acid, several times to interrupt and to re-establish, by successive congelations, the progress of this curious phenomenon.

This suspension of the gaseous disengagement by the application of cold, completely isolated the action of the deutoxide of nitrogen from any influence of caloric. I was nevertheless struck with the decomposition of pure and dilute nitric acid, by copper, at a temperature which was neither sufficiently elevated for decomposing the acid, nor sufficiently prolonged to concentrate it. I therefore sought to follow the influence of temperature, according to the different degrees of concentration of the acid.

I at first fixed upon the well known case in which copper is not attacked by fuming nitric acid. It is necessary that at +20° C. this acid should be very near its maximum of density, which is 1·552 at +20° C. It is this state that I have as completely as possible discussed and analysed in the course of my Memoir.

The pure acid with 1 equivalent of water attacks copper with extreme violence at +20° C.; it is the same, at this temperature, with the acids of 4 and $4\frac{1}{2}$ equivalents; and with all the acids intermediate to the density of 1·070.

But if all these acids of decreasing density, from that with one equivalent of water to that of 1·070, be placed, in glass tubes, in a refrigerant mixture, it is found that the copper is preserved in them all, with some modifications according to their state.

In the acid with one equivalent of water, the copper is covered with a blueish crust, and the liquor assumes a slight green tint. But the action does not go further; it is no longer carried on when the tube is removed from the mixture and allowed to return to the temperature of the atmosphere. I have thus preserved copper for several days at a temperature of +20° C. The blueish crust which is formed on the surface of the copper is insoluble in the concentrated acids, but very soluble in water.

Acids with 4 and $4\frac{1}{2}$ equivalents of water, and weaker acids, leave the copper in its metallic state, so long as they remain immersed in the mixture: but when they are removed from it, they cover the copper with a blueish crust and proceed no further, unless the tube is frequently agitated; whilst weaker acids commence their action as soon as the temperature is raised a little. The time of commencement always varies according to their concentration. Thus, acid of 1·217 begins at —10° C.; that of 1·108, at —2° C.

The gases which result from these oxidations differ according to the temperature and the degree of concentration. M. de Humboldt had already remarked in his Memoir on the analysis of the air, that dilute nitric acid gives, with copper, the purest deutoxide of nitrogen. If at the same time, the temperature be not raised, we thus have a gas completely absorbable by the protosulphate of iron. The acid whose reaction is accompanied by a great disengagement of heat is always mixed with nitrogen; indeed, acid of 1·217, whose action commences at —10° C., gives only very little binoxide, but much protoxide, of nitrogen, if it be maintained at that temperature.

From the above facts it must be concluded that the oxidation of the copper by nitric acid is subordinate to the concentration of the acid; to the temperature; to the presence of deutoxide of nitrogen, and to the solubility of the products which may be formed in the very acid which engenders them.

These principles extend to all the metals; but they present, according to the metal, modifications, the chief of which it is very important to be acquainted with.

Silver ranks beside copper. (I here suppress the details.) It is the same with mercury, which is completely attacked by acid with one equivalent of water, although the product which is formed is insoluble in this acid; but this complete action is due to the solubility of the molecules of mercury, which presents successively all its points.

Bismuth and tin present peculiarities. Bismuth retains its metallic lustre at +20° C. in acids, with 1 or 2 equivalents; but a less concentrated acid, with 4 or $4\frac{1}{2}$ equivalents, attacks it with great violence. If we continue to dilute the acid, so as to bring it to a density of 1·108, the metal is preserved. The assistance of heat and deutoxide of nitrogen is required for the action to commence: when once engaged, it undergoes by sulphate of iron and the refrigerant mixture the same influences as copper. Tin differs but little from bismuth.

Zinc has a progress peculiar to itself. Acids with 1 and 2 equivalents of water do not attack it in a refrigerant mixture, whose temperature should be exactly at —18° C., if it is not inferior. The metal is covered

with a slightly yellowish white crust, which protects it from ulterior action so long as it is retained in the mixture, but which doubtless becomes soluble by increase of temperature; for when the tube is removed from the mixture, the action is violently developed, and all the metal becomes dissolved.

In acid with 4 and 4½ equivalents, and even in weaker acid, zinc, which is very vividly attacked at a temperature of about 0° C., retains, on the contrary, its metallic lustre in the same acids when cooled; but when the temperature is gently restored to that of the surrounding atmosphere, the action appears in all its energy.

Finally, all the dilute acids attack zinc, notwithstanding abatement of temperature.

It is evident that this oxidation follows phases almost always the inverse of those of tin and bismuth.

It was only after having carefully studied all the re-actions which I have just noticed, that I dared to stop at that of iron: I found it described with so many minute and strange circumstances, that I repeated it very frequently before giving full credence to all the simplicity that I discovered in it. It is needless to say that I kept myself within the limits of the phenomena which I was examining, laying aside the effects of contact and the electrical phenomena, of which I was never able to obtain even the principal ones indicated for iron; which, doubtless, was owing to my great inexperience as regards this kind of investigation.

Small balls of iron, very well polished, placed in nitric acid containing 1 and 2 equivalents of water, were covered sometimes with a black, and sometimes with a blue crust, and sometimes with one of a blue color with a yellowish tint, giving rise to a few bubbles of gas which were dissolved. Iron, when thus covered, is not attacked by any weak or concentrated acid, at least, if the temperature be not raised. This crust sensibly exhibits the characteristics of protoxide of iron, which I have found to be unattackable by nitric acid of all degrees of strength, whether it be obtained by the combustion of iron in oxygen, by the passage of a powerful voltaïc current through harpsichord wires, or by the means which I have above pointed out.

There was in the first case an analogy with zinc, which is preserved in concentrated acid only by means of a yellowish crust which I have noticed, but which is found to be soluble in nitric acid, if its temperature be ever so little raised, even though diluted with a great quantity of water.

Acid of 4 or 4½ equivalents of water, and even rather weaker, preserves to iron all its metallic lustre; but it powerfully attacks it if heat be applied.

This second case causes a more complete analogy between iron and zinc.

Finally, I took a very dilute acid, or added water to acid in which the iron retained its metallic lustre, and I immediately saw the action commence, but without violence, and producing the green nitrate first noticed by Thénard.

It is therefore evident that the analogy between iron and zinc is continued in all cases, with the single difference of the temperature, which uniformly establishes the facility of the oxidation in favor of zinc.

Antimony and arsenic differ from all the metals which I have examined.

Arsenic is not attacked at the ordinary temperature (+ 20° C.), by any acid, whether pure or nitrous, however concentrated.

Antimony is acted on only by the more concentrated acids, but gently and with gaseous effervescence.

A mixture of nitric and hydrochloric acids does not attack these metals better, so long as the two acids do not react on each other, which causes a necessity for their concentration or the application of heat, when perfectly pure bodies are used. But if, on the contrary, we take a mixture of these two acids, in a state of extreme dilution, and after having put in it, either antimony or arsenic, add to it a few drops of nitrite, the action is immediately set up, as with copper and bismuth.

The mixture of nitric and hydrochloric acids, so long as they are neither sufficiently concentrated or heated to furnish *aqua regia*, remains without action. It is unavailing to pass into it a current of gaseous chlorine; it requires the addition of a nitrite, or else the formation of *aqua regia*, that is to say, still the presence of nitrous acid. Hydrochloric acid furnishes at the same time a solvent and an agent of decomposition, in order to arrive at a production of nitrous acid.

It is the same with platinum as with arsenic and antimony, but it requires a much higher temperature. However, even at the ordinary temperature of the atmosphere, in a mixture of hydrochloric and nitric acids too weak for furnishing *aqua regia*, platinum, under the influence of nitrite of potassa, is attacked sufficiently to yield a liquor charged with platinum and abundant crystals of double chloride of platinum and potassium which hang about the glass. But this reaction requires two or three days, and several successive additions of nitrite of potassa.

I also add two facts which, as it appears to me, elucidate this theory of *aqua regia*.

1. Spongy platinum left for twenty-four hours in contact with bichloride of hydrogen, which incessantly furnished chlorine and hydrochloric acid, did not lose one milligramme in weight.

2. Spongy platinum, in presence of nascent chlorine and nitric acid, at a temperature of + 125°,—conditions which the action of nitric acid on chlorate of potassa realises,—was neither dissolved nor oxidised, nor did it lose one milligramme of its weight.

In conclusion, nitric and hydrochloric acids mixed, nitric acid and chlorine combined, and hydrochloric acid and chlorine combined, do not attack platinum: hut if deutoxide of nitrogen intervene, in presence of a solvent, the platinum is attacked. The same principles and the same influences effect the solution of platinum, arsenic, antimony, and other metals. But what is the product which results from the action of deutoxide of nitrogen on nitric acid? A very simple experiment solves the problem.

If deutoxide of nitrogen be passed into nitric acid, and if the liquor be immersed in a freezing mixture, it may be saturated hy an alkali or its carhonate without disengaging a trace of deutoxide of nitrogen; a nitrite is produced. Deutoxide of nitrogen in presence of nitric acid constitutes therefore a combination, and not a solution. It is this nitrous acid, which, at a variable temperature, in conditions favorable to solubility, effects the oxidation of the metals. It will thus he understood how it is that platinum is attacked at the same time as silver when their alloy is treated hy nitric acid. As to the general progress of the oxidations which I have just described, they are easily explained: the nitrous acid forms nitrites of copper, mercury, and silver, which are decomposed by the nitric acid as soon as they are formed; this decomposition gives rise to deutoxide of nitrogen, which, with the nitric acid, again produces nitrous acid, from which results a new action and a new decomposition. Thus, these phenomena of propagation, so singular at first sight, present the simple case of an acid compound of nitrogen yielding its oxygen to the metals, and require for their explanation only the most ordinary principles of chemical affinity.

ON THE OXIDATION OF THE CONSTITUENTS OF THE OXIDE OF AMMONIUM BY THE ACIDS OF MANGANIUM.

BY REUBEN PHILLIPS.

The fact that ammonia under certain circumstances, is capable of oxidation and of slowly producing a nitrate, led me to consider it highly probable that powerfully oxidising agents would be competent to effect the instantaneous transformation of ammonia into combined nitric acid. With this view, I was led to try the effect of the manganate of potassa; I added a solution of ammonia to a solution of the manganate of potassa, and brought the whole to the boiling temperature: the solution almost instantly became colorless, while the peroxide of manganium was precipitated. The experiment was repeated in a bulh tube, the peroxide produced by discoloration being isolated hy the filter; after which the dissolved salts were hrought to dryness and subjected to a variety of reagents—it comported itself exactly like a mass containing nitrate of potassa.

But as the manganate was prepared by means of chlorate and hydrate of potassa and peroxide of manganium, and was not purified from the chloride of potassium, &c., the nitrate was necessarily mixed with much foreign matter, which obscured its properties, and prevented me from obtaining desirable proof of its presence: moreover, there passed through the filter a minute quantity of the oxide of manganium, apparently dissolved in excess of alkali. In order to obtain more convincing evidence I performed the following experiments:—

Camelion prepared hy Gregory's process was kept at a dull red heat for fifteen minutes, to insure the total decomposition of the chlorate; a piece nearly as large as a nutmeg was then added to half an ounce of water containing a quantity of ammonia; the whole was then kept at a boiling heat for ahout fifteen or twenty minutes. Owing apparently to the concentrated state of the solution, the action promised not to be very speedily terminated: I therefore added a small quantity of the common carbonate of ammonia, supposing that the carbonic acid would unite with the potassa, and the permanganic acid react on the ammonia. Accordingly there was a very strong reaction: the liquid became red, and copiously evolved gaseous matter, but whether oxygen, or not, I did not ascertain. In ahout half a minute the fluid became free from color; I then filtered, and dissipated the water, and digested the mass in spirit of wine; the carbonate of potassa, together with most of the chloride of potassium, was thus separated. The supernatant liquid was then poured into a capsule and set fire to, which speedily rendered the saline matter dry; after which it was heated nearly to redness; the salt was then placed in the hottom of a tuhe, and sulphuric acid, previously diluted with its bulk

of water, poured on it: plentiful fumes of nitrous gas were disengaged.

A solution of the permanganate of potassa at common temperatures appears by the change of color to act but slowly on the oxide of ammonium, while the manganate under similar conditions acts rather faster; but the relative intensity of their action seems to be influenced by the quantity of the oxide of ammonium, &c. present.

This ready conversion of the oxide of ammonium into nitric acid may perhaps prove a valuable test for the presence of that substance, as it is possible to detect extremely minute portions of nitric acid; however, I have not ascertained how far this method is capable of answering the purposes of a delicate test.

NEW METHOD OF PRODUCING FORMIC ACID.

BY CHARLES WATT, JUN.

The usual method of producing this acid is by the action of sulphuric acid and peroxide of manganium on starch; but the quantity obtained by this means is so small as to render the process of obtaining any quantity of it extremely troublesome: for instance, if we use the proportions recommended in Liebig's last edition of *Dr. Turner's Chemistry*, *viz.*, 10 parts of starch, 34 of peroxide of manganium, 30 of sulphuric acid, and 30 of water, we obtain only 3·35 parts of dilute formic acid, 100 parts of which will be required to saturate 15 parts of dry carbonate of soda; and, for my own part, I must say that by this process I have never been able to obtain even so much as is here stated.

My method consists in treating coal naphtha by sulphuric and chromic acids, (the latter prepared by the action of sulphuric acid on bichromate of potassa). The quantities which I have practically found to answer the best are—1 part of coal naphtha,* 3 of bichromate of potassa, and 3 of sulphuric acid.

The naphtha is to be placed in a flask fitted with a funnel tube; the bichromate of potassa is then to be added, and the sulphuric acid, diluted with an equal weight of water, gradually poured down the funuel; while the acid is being added, heat is to be applied, when the formic acid will distil over and may be condensed in a vessel kept cool.

A portion of naphtha will distil over with the formic acid, which may be again treated with bichromate of potassa and sulphuric acid, when a fresh portion of formic acid will be produced. If this acid is required perfectly pure, it must be saturated with pure carbonate of soda or potassa, and subjected to a gentle heat to volatilise any small portion of naphtha with which it may be contaminated. The formic acid is then to be liberated from it by means of dilute sulphuric acid, and subjected to distillation, when the acid will be obtained perfectly pure.

In this process peroxide of manganium may be substituted for the bichromate of potassa; but as this is never met with pure in commerce, I cannot recommend its employment. It may not be out of place here to remark, that I have detected considerable quantities of chloride of sodium in every sample of commercial peroxide of manganium which has fallen under my notice, and this is one of my principal objections to its employment in the production of pure formic acid.

By the process which I have described, I have been able to produce comparatively large quantities of formic acid, and at a very moderate expense.

* There are several other compounds which will answer as well as coal naphtha, such as pyroxilic spirit, &c.

ON THE ATOMIC WEIGHT OF ZINC.*

BY M. JACQUELAIN.

M. Jacquelain has submitted zinc to a new examination. He found that the sample of the zinc of commerce which he analysed, contained per cent.—

Carbon	0·003
Iron	0·142
Lead	0·685
Zinc	99·190

By converting the zinc into oxide, by means of either sulphuric or nitric acid, and afterwards decomposing the sulphate or nitrate by heat, M. Jacquelain determined the weight of the oxide, and fixed the atomic weight of zinc, which, according to his experiments, should be 414, instead of 403·226, as generally admitted.

IMPROVEMENT IN GROVE'S VOLTAIC BATTERY.

BY M. J. BRAZENDALE.

One great impediment to the general employment of the powerful battery of Mr. Grove, has been the inconvenient and suffocating evolution of nitrous gas which is evolved in large quantities during its action. By a very simple contrivance this objection is entirely obviated. The mahogany tray

* Abridged from *Comptes Rendus*.

holding the pots, or the trough, if the form be that of Cruikshank's, has a wooden ledge or gutter affixed to the top, and entirely surrounding it. A piece of wood-work is now to be constructed like the lid of a box; it must be rather wider and longer than the trough, so that the edges of it may fall exactly into the gutter; the top part of this lid is fitted with a piece of brass-work, in which an internal screw is cut, and adapted to fit the screw-joint and stop-cock of a caoutchouc gas-bag, or which, perhaps, is better, a conducting tube may be attached thereto, in order to convey the nitrous gas to a proper gasometer. When the battery is set in action, the gutter is filled with water,* and the lid and appendages placed on the top, and by this means the interior of the trough or pot battery is completely air-tight; the gas passes away into the reservoir provided for it, and might probably be applied to some useful purpose.

The pure hydrogen, which is abundantly given off from acid batteries such as those of Mr. Smee's construction, may be collected in a similar manner; and by using a piece of apparatus, previously described, for improving the light of coal gas,† might be effectively employed for illuminating purposes.

ON THE EXTRACTION OF ALL THE PERCHLORIC ACID CONTAINED IN THE PERCHLORATE OF POTASSA.‡

BY M. A. NATIVELLE.

Perchloric acid, so useful as a reagent, is frequently wanting in laboratories, owing, doubtless, more to the very small quantity of this acid that can be obtained by the ordinary proportions of sulphuric acid indicated in works, than to the difficulty of the operation, which, in itself, is very simple. It is sufficient, as is known, to introduce into a glass retort one part of perchlorate of potassa with half its weight of sulphuric acid diluted with one third of water, and to heat it, to obtain perchloric acid, very dilute, it is true, and in very small quantity compared with the proportion of perchlorate employed. If we examine that which passes over in this operation, it is observed that the water used for diluting the sulphuric acid first passes over without the slightest acidity; and, although at this stage of the operation the retort is in full ebullition, almost all the perchlorate remains undissolved; it is only afterwards, when the sulphuric acid has again acquired its original degree of concentration, that a portion of the perchlorate begins to be decomposed; but as the proportion of sulphuric acid is much too small for the quantity of perchlorate employed, it results that the distillation proceeds very slowly, and that a portion of the free perchlorate is decomposed by the heat, as it would be if alone, into oxygen and chlorate of potassa, which chlorate, aided by the action of caloric and the acid, is readily decomposed into oxygen and oxide of chlorine, and even into chlorine. These gases are disengaged as long as the operation lasts; if, at the end, the residue in the retort be examined, a great quantity of undecomposed perchlorate will be found. The product of the distillation, as already stated, is but very slightly acid, especially if the portions which first passed over, and which are nothing but pure water, have not been rejected. Notwithstanding the great dilution of the perchloric acid obtained by this means, it reacts very well on the salts of potassa; it is, perhaps, this which has given rise to the opinion hitherto held, that all the salt in the retort is decomposed, and that the perchloric acid which results represents nearly the quantity of salt employed, which is far from correct, on account of the partial decomposition into its elements, which the perchlorate undergoes by the sole influence of heat. If this operation has been well followed up, it is evident that the water added to the sulphuric acid is useless, or nearly so, and that the quantity of that acid is insufficient for decomposing all the perchlorate; therefore, contrary to what occurs in ordinary substitutions, it is the influence of the mass, —an union of forces, if I may so speak,— which conquers the affinity, since we know that in these reactions the perchloric acid displaces the sulphuric acid.

It now only remained for me to determine the proportion of sulphuric acid necessary for decomposing the whole of the perchlorate. This proportion is given below, and, although at first it may appear arbitrary, it is indispensable for obtaining all the perchloric acid.

500 parts of perchlorate of potassa, as free as possible from chlorate, and reduced to powder, are introduced into a glass retort; 1000 parts of sulphuric acid at 66°, free from nitric acid, are added, and then only 100 parts of distilled water: this small quantity of water is not indispensable, for it will be seen farther on, that without it, perchloric acid is immediately obtained in

* A thin layer of mercury might probably be preferred.

† Vide The Chemist, Vol. II. p. 240, for a description of Naylor's apparatus for increasing the illuminating power of coal gas.

‡ *Journal de Pharmacie et de Chimie*, Juin, 1842.

the solid form—that is to say, crystallised. To the retort is adapted an adopter fitted with a long tube entering into a tubulated receiver surrounded with cold water. The apparatus must not be luted with paper or any other organic matter, for, when heated and in contact with the vapor of perchloric acid, it is immediately decomposed, causing slight detonations: by using well adjusted vessels, it is easy to avoid the necessity for a lute; in the contrary case, filaments of asbestos may be employed. All the perchlorate is carefully heated; it dissolves without delay; and as it may be supposed that the perchloric acid has already become free, it is necessary to manage the fire carefully at the commencement, in order to prevent too much sulphuric acid from being carried over with it in the distillation. The best means of regulating the operation is to avoid ebullition, keeping always just below it: very little sulphuric acid passes over, as may be conceived, since perchloric acid evaporates at 140° C., a temperature very inferior to that at which sulphuric acid distils; it is known that the operation is terminated when the very transparent residue which the retort contains becomes colorless, or better still, when the last drops which distil over slowly follow one another, and when the temperature of the retort is almost sufficient for volatilising sulphuric acid. According to the manner in which the operation has been conducted, a quantity of distilled product is obtained, which may vary every time; these differences are owing to the greater or smaller quantity of sulphuric acid carried over in the distillation; on the average, a carefully managed operation gives, with the quantity of perchlorate directed, 300 parts of crude acid of a density of about 45°; when it has been too quickly carried on, the density of this crude acid approaches that of sulphuric acid, and its weight is then the same as that of the chlorate employed.

However, nothing is easier, to purify this acid, than to remove the sulphuric acid and the small quantity of chlorine which it contains: for that purpose, it is first agitated with a slight excess of a saturated solution of sulphate of silver; the chlorine is precipitated with the silver, the precipitate is separated with the filter, and the acid is poured into a large capsule; it is put in contact with hydrated carbonate of baryta obtained artificially and well washed; it is added until all the sulphuric acid is precipitated, and even until a small quantity of perchlorate of baryta is formed; it is filtered. The liquor contains only perchloric acid purposely mixed with a small portion of perchlorates of silver and baryta; it is put into a tubulated retort, and distilled at a gentle heat with the same apparatus as in the first operation, adopting the precautions already pointed out; as the liquor which first passes over is only water, it may be received in a capsule, and the receiver need not be added until after it has been ascertained by means of test paper that the liquor passing over is acid. In general, the more slowly the operation is conducted, the less water will remain in the retort, and the acid will be more dense. It is as well, in summer, to surround the receiver with water in which a few pieces of ice have been melted. Distillation is carried on almost to dryness, always avoiding, however, the decomposition of the perchlorates of baryta and silver which should remain as residue, for otherwise the distilled perchloric acid might contain traces of chlorine which have escaped the precipitating action of the salt of silver in the retort. The perchloric acid thus obtained is perfectly pure, colorless and transparent; its density varies between 60° and 65°; it is oily, like sulphuric acid. From 500 parts of pure perchlorate of potassa, I obtained 150 parts of this concentrated acid. I shall shortly describe the means of producing crystallised and pure perchloric acid.

SOLUBILITY OF FIBRIN AND COAGULATED ALBUMEN IN WATER.*

GMELIN observed, a long time ago, that coagulated albumen is susceptible of entirely dissolving in water, aided by a temperature of about 200° C. Wöhler and J. Vogel have since undertaken on this subject a series of experiments, which have proved to them that this solution may be effected at even 150° C. Indeed, the latter chemists having introduced pieces of white of egg, with a proportionately small quantity of water, into strong glass tubes, which were afterwards closed with a blowpipe, then heated for two or three hours in an oil-bath raised to that temperature, (taking all the precautions necessary for preventing explosions,) the solution was completely operated. It is true that the result is much more promptly obtained when it is heated to 200° C., but pyrogenous products are then also formed.

Coagulated albumen, thus dissolved in water, seemed, in the preliminary tests to which these experimentalists submitted it, to have preserved all its chemical characters, and to have undergone no modification but coagulation.

* *Annalen der Chemie und Pharmacie*, February, 1842.

By submitting to ebullition with water the fibrin of the blood and muscular fibres, the same phenomena were produced; these substances were almost entirely dissolved, and left but very little residue. The solution thus obtained furnished an abundant precipitate by the addition of acids, and even by very dilute nitric acid. The precipitate to which acetic acid gives rise is readily redissolved by an excess of that reagent.

PREPARATION OF CHLORIDE OF ZINC.*

BY M. RIGHIERI.

TAKE: Pure crystallised chloride of barium . 80 gram.
Pure sulphate of zinc 98·6 gram.
Pure distilled water . 1500 gram.

Divide the 1500 of water into two parts; dissolve the sulphate of zinc in one of them, and the chloride of barium in the other; mix the two solutions in a matrass; accelerate the decomposition by means of the heat of a sand-bath, for only a few minutes; filter, and evaporate on the sand-bath until only about 60 grammes of liquor remain, which must be received on a fresh filter on which is laid some animal charcoal mixed with a few centigrammes of powdered chloride of barium; the chloride being filtered, it is evaporated until the product, left to itself, is presented under the form of crystalline flocks of a brilliant white, which are preserved in a well-closed vessel when they are dry.

ON THE SUBSTANCES EMPLOYED AS DIAPHRAGMS OF CONSTANT BATTERIES.

BY J. N. ROWE.

BROWN PAPER.—Paper is about the most inconvenient substance hitherto employed for the purpose, as it never completely prevents the two liquids mixing, so that continuity of action is very speedily destroyed.

ANIMAL MEMBRANE.—This, although vastly superior to the last mentioned substance, is not free from defects, and is moreover not very pleasant to handle.

PLASTER OF PARIS.—Those formed of this material I have found to answer extremely well, and to be pretty durable. They were first, I believe, proposed by Mr. C. V. Walker, and will be found a great acquisition to those, who, from local circumstances, may not be able to obtain other kinds of porous ware.

BISCUIT WARE.—Diaphragms composed of biscuit ware or pipe-clay may be said to rank first in the list in regard to durability, porosity, capability of perfectly separating the two fluids, and offering no interruption to the electric current; consequently, they leave nothing to be desired.

Moulds have also been formed of wood cut into slices across the grain, leather, and pasteboard, some of which have been attended with very good success; but where duration of action is required, nothing can be superior to those formed of biscuit ware, and when such cannot easily be got, I should recommend next in preference those of plaster of Paris; and last of all those of brown paper, which may be used for a single experiment or so, after which they become useless.

At this time, when the art of Electrotype has become such a favorite, the preceding hints will probably meet the approbation of some of your numerous readers.

LONDON ELECTRICAL SOCIETY.

TUESDAY, JUNE 21st.

THE Secretary read—1st, "On Native Malleable Copper," by John A. Phillips, Esq., of St. Austell, in which the author is of opinion that the metal thus designated, as well as noss copper and other forms, are produced by operations of nature not dissimilar to those artificially arranged for Electrotype processes. Specimens of ore were submitted to the Society, which strongly confirm this opinion. 2nd, "On the Transfer of Mineral Substances through various Fluids by Voltaïc Agency," by A. Crosse, Esq. Some years ago, the author observed results connected with a certain natural arrangement of materials which he thought might be easily imitated artificially, which was thus effected. Pipe-clay was well kneaded into a basin, and in the pipe-clay were imbedded a piece of limestone and a shell. Fine sand and pounded sulphate of iron were well incorporated and placed above the clay: in the sand was likewise embedded a shell. The whole being covered with water was allowed to stand for many months: at the expiration of the time it was found that the shells and limestone had lost weight, and round them had gathered groups of crystals of *sulphate* of lime, in place of the *carbonate* lost. The iron, which had been in connection with the sulphuric acid, was about these nuclei in the form of peroxide. This experiment is in confirmation of Mr. Crosse's opinion, that, though many mineral productions may be the result of electric agency, yet far the larger portion are produced by the natural electric affinity between the several elementary substances, developed in a manner somewhat analogous

* *Journal de Chimie Médicale*, Juillet, 1842.

to that which may be readily traced in this experiment. It was not until this arrangement was completed, that Mr. Crosse discovered one point of difference between this and nature's operations—the want of porosity in the basin; and this leads him to remark upon the increase of electrolytical action when the containing cells of a Daniell's battery are porous,—an important fact to those who are practising Electrotype. It is impossible in a brief notice to analyse this important communication as it deserves. A long series of experiments, bearing upon the same point, were minutely described, the results of some being most interesting. We cannot omit alluding to one, in which, by an action analogous to that now described, a mould of a sovereign was made in a piece of marble,—and another, to which a glass rod connected with the *anode* was gilded, even above the liquid in which it rested. For these and a host of other interesting facts we must refer to the Proceedings of the Society. Mr. Crosse is strong in his opinion that the time is not far distant when we shall be able to produce artificially the precious gems, and he thinks that a very wide and important field is thus open to the persevering inquirer. Mr. Weekes's Electro-Meteorological Register was then read.

II. CHEMICAL MANUFACTURES.

ADULTERATION OF THE NITRATE OF SODA OF COMMERCE.

BY CHARLES WATT, JUN.

Among the numerous articles of commerce which have recently become the subject of the most infamous adulterations and sophistications, nitrate of soda occupies a very conspicuous place.

That publicity is the most effectual method of putting a stop to such systems of fraud is, I think, the opinion of all capable of forming a judgment. With this view the following observations are laid before the readers of The Chemist.

It is, I believe, generally known that the consumption of the article under consideration has, within the last few years, increased to a very considerable extent, owing to its application to agricultural purposes, in which it has, doubtless, proved very beneficial. Now, those who purchase nitrate of soda for this purpose are for the most part ignorant, unsuspecting farmers, men upon whom fraud is practised with ease, and without the slightest fear of detection from them.

The article resorted to for this scandalous imposition is chloride of sodium (common salt), which is considered to have any thing but a beneficial effect on some lands.

We have it on good authority that the adulteration of this article is very frequently carried on to the amount of 50 per cent., and we have been informed of more instances than one in which chloride of sodium has been sold for nitrate of soda, when, in fact, it did not contain even a trace of the latter salt. It was some time since proposed to test the presence of chloride of sodium in the nitrate of soda sold in commerce, by throwing portions of it into the fire, as chloride of sodium produces a peculiar crackling noise when so treated, owing to its containing small portions of water mechanically lodged between the plates or layers of its crystals, which, in consequence of its expansion by the heat, bursts the crystals, producing the peculiar noise before mentioned.

This method is sufficient to show whether or not it is adulterated; but we are unable by this means to ascertain the amount of the adulteration, the only method of ascertaining which with precision is the following:—

Dissolve some silver in nitric acid diluted with three or four times its bulk of distilled water, and apply heat to aid its solution; then dissolve a given weight of the suspected nitrate of soda in distilled water, and add portions of the nitrate of silver until it ceases to produce any precipitate. To effect this without the addition of more nitrate of silver than is necessary, after every fresh addition of that salt take a few drops of the supernatant liquor out with a glass rod, place them on a piece of flat glass, and add a drop of the nitrate of silver; if no more precipitate is formed, this part of the operation is completed; if otherwise, more nitrate of silver is to be added, and the operation of testing repeated as before. The precipitate (chloride of silver) thus obtained is to be dried and weighed. Every 4 grains of this precipitate contains about 1 grain of chlorine; this in the chloride of sodium is combined with sodium, so that for every 4 grains of chloride of silver obtained, 1·66 grains of chloride of sodium must have been contained in the nitrate of soda.

In testing by the above method, it is to be

borne in mind, that all the commercial nitrates of soda contain some chloride of sodium, generally amounting to two or three per cent.

It must be evident to any thinking mind, that if this infamous fraud be continued, the nitrate of soda will speedily lose the character which it has obtained and so justly deserves, they who practise it will lose the profits arising from its sale, and the public will be deprived of the advantages to be derived from its use. But the parties who commit these acts are always so narrow-minded that they cannot see their own interest.

In conclusion, I would strenuously advise every purchaser of nitrate of soda to ascertain by the process above given the purity of every quantity before he pays for it, and to tell the dealer of whom he purchases it that he shall do so before he pays: he must not test what may be given to him as a sample only,—that is sure to be pure,—but test from some taken from the quantity, after it is on HIS OWN PREMISES. Should the purchaser not be able to perform the above operation, which is very simple, then he will do wisely to have it tested by a chemist; for on this he may depend, that although he may have to pay for the analysis, he will be a great gainer by having it done, and this is the only method of putting a stop to such rascality.

MEANS TO BE PUT IN PRACTICE FOR DETECTING THE ADULTERATION OF CANE OR BEET-ROOT SUGAR WITH SUGAR OF FECULA.*

BY M. E. KRANTZ.

THE sophistication of commercial products has arrived at such a height, that it is very difficult at the present day to find a pure article which has passed through several hands.

These reflections have been suggested to us by the knowledge of the fact, that cane sugar, which formerly, when its price was very high, was adulterated with sand or sugar of milk, is, now that this product is very cheap, adulterated with sugar of fecula.

In order to detect the admixture of these two sugars, two grammes of the suspected sugar are dissolved in a flask in thirty grammes of distilled water; the liquor is filtered and two decigrammes of alcoholised potassa and one decigramme of deutosulphate of copper are added; it is stirred in order that solution may be effected, and the flask is afterwards corked, and that which occurs must be observed.

If the sugar under examination is mixed with sugar of fecula, a red precipitate is obtained some time after mixing, and if the sugar mixed with fecula is in large quantity, the conversion of the salt of copper into protoxide is complete in twenty hours; the solution, which was of a blue or green color, is completely decolored, and does not contain a trace of copper.

If we act on pure cane or beet-root sugar, no red precipitate is obtained even after eight days, although it is placed in the same conditions.

If the mixture is formed of equal parts of each of these sugars, the precipitation is complete in twenty hours; if the cane sugar is mixed with only two and a half per cent., a slight red precipitate is obtained after twenty-four hours, but the solution is not deprived of color even after a period of eight days.

We should mention, that all these operations must be carried on without the employment of heat.

* *Journal de Chimie Médicale*, Juillet, 1842.

BEAUTIFUL OLYMPIAN GREEN MIXTURE FOR COMMUNICATING THAT COLOR TO FOIL OR TINSEL.

BY J. N. ROWE.

THE plain foil is a sheet of very thin copper, one side of which is silvered and afterwards polished to the highest degree. The liquid is then applied to the surface in the manner I am going to describe. Take about half an ounce of the compound called sesquiferrocyanuret of iron, and the like quantity of bichromate of potassa; grind them to a fine powder on a marble slab; then add about two ounces of gum mastic, picked very clean, and in powder; pour about an ounce of pyroxylic spirit, and keep grinding them all together until the mass appears perfectly homogeneous and of a most beautiful transparent green, the tint of which will entirely depend on the fineness to which the ingredients have been ground; if the chromic salt has been the finest powder, the yellow tinge will predominate; if, on the contrary, the salt of iron has been so, the shade will be of a blue cast.

Take a little more pyroxylic spirit, and thin the mixture so that it may work easily; then with a fine flat brush smear the surface of the foil evenly over, and the operation is complete. The composition dries of itself, being of the same nature as spirit-varnish.

Feeling confident that this recipe has never

been before published, I think it will, on that account, find a spare place in your interesting periodical.

NEW ELECTROTYPE PROCESS FOR ENGRAVING IN THE WOOD-CUT STYLE.

BY M. F. ADELINE.

THE following process for engraving is, I believe, entirely new, and will doubtless prove of service to the practical artist :—

A plate of some hard metal, as, for example, copper or brass, is immersed in a trough containing a solution of oxide of lead in caustic potash, and connected with a voltaïc battery as is usual in electrical depositions, when a film of metallic lead is firmly precipitated thereon (and of about $\frac{1}{30}$ of an inch in thickness), it is taken out, and all the deepest shades of the drawing put in with a pointed tool so as to cut through the lead, but not injure the harder plate ; this done, stop up all these parts with varnish and immerse it in a solution of *sulphate of copper*, connected of course with a battery, till a very thin layer of copper is deposited on the remaining lead. This copper is now covered with a similar coat of lead, and by the same means all the fine parts of the picture are executed thereon, in the manner before described, the thin coat of voltaïc copper preventing the tool from going too deep down. The varnish is now cleaned off, and after being slightly moistened with a mixture of wax in a large proportion of turpentine, is subjected to the voltaïc apparatus to receive a thick deposit of copper, to be afterwards used as the block to print from, the prepared plate of hard metal being merely employed as the mould or matrix.

IMPROVED SKYROCKETS.

BY M. J. BRAZENDALE.

INSTEAD of making the case about six inches in diameter, I make it ten or eleven long. When the case is filled, the bore is half filled with corn powder, on the top of which a ball of damp gunpowder, made into a kind of paste, is pushed in, and placed in contact with the grains of powder, from which it should raise the height of space about two thirds of the interior diameter ; the rocket is now proceeded with in the usual way, as capping with touch paper, and the appendages affixed. You thus have the effect of two rockets with only one case and furniture, all very little heavier than one of the ordinary kind ; they consequently attain extremely great altitudes. I usually form the "*snakes*," and "*golden rain*," by making a flat and even cake of meal-powder made into a stiff paste with water, cutting it into pieces of about three inches long by three sixteenths of an inch diameter. When they have become dry and hard I brush them all over, except one end, with thick glue or flour paste, and afterwards roll them in any dry powder, as flour or chalk, until they are dry. I can introduce a far greater number of them, thus prepared, into a rocket-head, than those with thick paper cases, which must necessarily take up more room, and render the rocket too heavy to ascend properly.

ON ELECTROTYPE MOULDS.

BY C. K. BROOKS.

HITHERTO bees-wax, steärine, and Paris plaster have been the only substances effectively employed in the formation of moulds for electrotype experiments : I employ the following, and with the greatest success :—

Melt a quantity of shell-lac in a ladle, and drop a portion on some flat surface (as a piece of writing paper) ; stick in among it the end of a piece of copper wire ;* and while it is warm, take the medal, which should be previously slightly oiled or greased, and press it neatly thereon. On its being lifted off, a very fine and sharp impression will be found. Good sealing-wax may be substituted for the shell-lac, taking care to grease or oil the medal in both cases, as, on account of oily bodies not having the property of uniting with gum lac, they do not adhere together at all, so that with this little hint the most unpractised hand will succeed in producing the most perfect casts.

The surface will of course require to be metallised either by black lead or bronze powder.

* This is the conducting wire for attaching to the zinc element of the circle.

III. PHARMACY, &c.

THE PHARMACEUTICAL SOCIETY.

To the Editors of The Chemist.

GENTLEMEN,—

Being impressed with the idea that some points in connection with the association lately established under the name of the Pharmaceutical Society of Great Britain ought to be made known for the guidance of the respectable portion of the trade and the public, I shall feel obliged by your allowing your interesting Journal to be the medium for so doing.

In the first place, there can arise no benefit to members, to compensate them for a tax of Two Guineas annually, more especially that portion of them residing in the provinces. An attentive application to a few standard works on Pharmacy and Chemistry will furnish the mind with more knowledge on those subjects in six months, than can be acquired from being a member of the Pharmaceutical Society for as many years.

Secondly, as regards the diploma, (which I believe is intended to be a *smart* affair,) the possession of which (for show) is the main object with a great portion of those who have joined the Society; the most respectable dispensing chemists and druggists of the kingdom who have not become members of the Society, need be under no alarm in consequence of not possessing this rare—"Deed or Title of Degree;" for I can assure you that no species of discrimination has been used in admitting members, so that, while some have joined who have neither been educated nor apprenticed as chemists and druggists, others who have received those necessary qualifications have refused to allow themselves to be taxed to the amount of Two Guineas per annum, considering, and truly too, that their expenses have been sufficient already.

The principal source of advantage expected to accrue to the possessors of this said diploma is from the effect it will have on public opinion; but I am sure the public will not be led astray, when they become acquainted with the fact, that it is not furnished to the possessor for merit of any sort, but simply in return for submission to the above tax during the term of his natural life, paid to certain persons residing in the great metropolis, for no very specific object, unless to establish a monopoly, of which there are already sufficient and to spare—more, I think, than poor John Bull can long continue to support.

Let the public be made acquainted with the fact, that any person may become a M. P. S. G. B., though he may be utterly devoid of qualification, and of the knowledge requisite for practising Pharmacy and dispensing Medicine; let the public once know this, and it is all over with this *learned* body.

In conclusion, I trust Sir James Graham, in framing his Bill for Regulating the Medical Profession, will pay no attention to any suggestions that may come from the Council of this Society. I hope, also, that the trade will see the necessity of not supporting a Society which aims at injuring those who are duly qualified by education and apprenticeship, by furnishing a diploma to those who are not so qualified, which diploma is intended to have the effect of causing the public to trust only those who have *purchased* the right to *exhibit* it.

I remain, Gentlemen,
Respectfully yours,
H . . . . C. ARGASS.

ABUSES OF THE DRUG TRADE.

LETTER IV.

ON THE SALE OF DRUGS BY UNQUALIFIED PERSONS.

To the Editors of The Chemist.

July, 1842.

GENTLEMEN,—

The druggist in this country is subjected to a ruinous competition—a competition not confined to a class, or to those who, having been regularly educated and apprenticed to the trade, comprise its legitimate members, but which is augmented by various grades and denominations, ranging from the dispensing practitioner in his private surgery, to the mountebank quack who vends his infallible specifics in the public market-place; and embracing that extensive though pettifogging class of hybrid practitioners, who combine the sale of drugs, cosmetics, and perfumery with the practice of medicine, midwifery, and surgery—together with the whole body of general dealers, drysalters, grocers, and oilmen.

Can it, I ask, be matter of surprise, under such a state of misgovernment, that pharmacy in this country should be at so low an ebb? On the contrary, it is wonderful that it should have attained to its present state; and it must be admitted as a striking instance of the ingenuity and perseverance of its professors, and redounds to the credit of those by whose indefatigable exertions it has been thus sustained.

The druggist, when claiming protection,

has been taunted with the absence of all proof of his own efficiency—as if the serving a long apprenticeship, and a series of years in the capacity of assistant to the trade, together with the education received, and that description of general knowledge, the acquirement of which is inseparable from the filling of these situations, stood for nought. Surely it will be granted, that this routine must give him, at least, a better knowledge of the properties and qualities of drugs than the *general dealer*, who has undergone no such ordeal, can possibly possess. I assert that it is owing to the absence of all protection whatever, in the sale of drugs, that the druggists of Great Britain, as a body, possess so little scientific acquaintance with the art which they profess; and well may they exclaim, How can it be expected that we should devote both time and talents in the attainment of a thorough knowledge of chemistry, unless we are protected from the fraudulent dealings of the uninitiated? If a knowledge of botany, chemistry, materia medica, and pharmacy, is considered absolutely indispensable, not only in the dispensing of medicines, but the vending of drugs and medicinal compounds, then is it convincing proof that such protection should be awarded; and the fact of other parties being allowed to sell drugs in spite of this consideration, is in itself sufficient cause for existing evils. The question, both with the Government and the parties interested, with whom the public should be classed, should be, Is it expedient longer to sanction this abuse?

It is this unwarrantable and extensive competition which renders the pharmaceutists of Great Britain mere tradesmen, and keeps them in the rear of their continental neighbors as regards their proficiency in pharmaceutical chemistry.

It is worthy of note, that in all those countries in which pharmacy is cultivated as a science, and which have been held up for our imitation, its professors receive the support of the State, and are more or less under the surveillance of the Government, being in fact Government officers; also, that on the formation of their several associations, or the founding of their schools or colleges, the first thing invariably sought, was the protection of the legislature; and we are informed, that on the establishment of the Pharmaceutical Association of Westphalia, the first of its kind in Germany, among the resolutions passed, the third was to the following effect—"That the principles and objects of the Association shall be published, and the Government shall be made acquainted with it, and applied to for sanction and assistance."* How different the proceedings of the Society of Great Britain! In the March Number of their Journal, in a notice of Sir James Graham's Bill to regulate the Medical Profession, the chemists and druggists are somewhat sarcastically told, that "*having undertaken to reform themselves*, they will be allowed to proceed unmolested in their laudable undertaking."

Now, with all due deference to the Right Honorable Gentleman the Secretary for the Home Department, I do consider this rather a left-handed sort of compliment, as it is capable of being construed very differently to that with which the worthy Editor has been pleased to congratulate himself. It appears that Sir James has evinced no desire to benefit us, or raise our standing in the profession, even though he should have shown no disposition to injure us; but rather that he has treated us with utter contempt.

With a full knowledge of the above circumstances, it is remarkable that the Council should continue to overlook this most important object for the protection of its general interests. In the May Number of The Chemist, you have remarked, with much truth, that "the Pharmaceutical Society has done little in the way of improving the political position of druggists." By this time the chemist and druggist ought not only to be *recognised* as a branch of the medical profession, but should be *a legally constituted branch*. And may it not be reasonably asked why the Council neglects the opportunity which presents itself, (on the introduction of a bill by Sir James Graham, enabling Her Majesty to grant new powers to the Colleges of Physicians and Surgeons,) for obtaining a charter for the incorporation of the Pharmaceutical Society? It should not be forgotten, that the bill brought before the House last session under the auspices of Mr. Hawes, would have conferred much benefit upon chemists and druggists, ranking them with the profession, and giving them a standing in society for which they may look in vain either to the forthcoming bill or their representatives, [would be?]—the Council of the Pharmaceutical Society. They spurned the advantages offered in Mr. Hawes's bill—advantages that might have been obtained without the sacrifice of a penny, other than would have resulted from a prohibition of their counter practice, for the protection of which they established their Society, though this, the primary object, has every appear-

* The approbation of the Government was obtained accordingly.

ance of being ultimately discarded, in lieu of which they shall be expected to combine the knowledge of the chemist, pharmacopolist, and toxicologist, in the individual capacity of the druggist.

Furthermore, it would seem that henceforth druggists are to become operators as well as manipulators; manufacturers as well as dispensers; and to combine the prosecution of the science of chemistry with the retailing of drugs; and, in fact, to acquire every description of information connected with chemistry and pharmacy, for the mere purpose of being able to detect and correct the casualties or errors of their superiors in the profession! In a word, they, the subordinates of the medical profession, are to be compelled to acquire more information than many of the professors themselves possess; and yet are to receive no additional advantage by the acquirement, not so much even as the vouchsafing to them the possession of their *ancient privileges!!!*

Gentlemen, in conclusion I would beg to apologise for the length of this communication, and remain,

Yours respectfully,

PHARMACOPOLA.

DRUGGISTS AND THEIR ASSISTANTS.

To the Editors of The Chemist.

Newcastle, July 6th, 1842.

GENTLEMEN,—

I have perused with some attention a communication addressed to you, on "The Treatment of the Assistants of Medical Men and Chemists," in your July Number, and shall be glad, should the following remarks meet your approval, to see them in the forthcoming Number of your very useful Periodical. Did I not consider the letter of your correspondent as conveying unjust imputations upon the master druggists,—imputations as unfounded in fact as they are unmerited,—I should scarcely have taken the trouble of refuting its assertions, as I am quite satisfied that the object sought to be attained by its publication will be entirely defeated by the very illiberal spirit which pervades the whole statement.

Any person not conversant with the subject would imagine, from the way in which your correspondent states the matter, that all druggists' assistants are gentlemen,—in appearance at least,—of education unimpeachable—of chemical and pharmaceutical knowledge commensurate with any demand that may be made upon them,—and all so anxious for the interests of their employers, as to set little store by "life, health, and happiness," when any duty is to be performed. Such a person would also think that the paltry remuneration frequently given for such important and *unselfish* services to one who has not only to keep up the appearance of a gentleman, but "to bear all the ways and tempers" of his master, however harsh, as perfectly scandalous; and he would be quite correct in coming to such a conclusion, were he to argue upon the case as your correspondent places it before him.

Being, Messrs. Editors, a master druggist myself, and having passed through all the phases of a druggist's career, I shall not apologise for setting to right all the parties concerned as to the real state of the case between masters and assistants.

When a chemist and druggist wants an assistant, he is naturally desirous of engaging one who has been accustomed to the general routine of a retail shop, *viz.* compounding and dispensing medicines, physicians' prescriptions, &c. &c., and one who is capable of taking the entire management of *the* shop; which of course implies that he is, in common parlance, master of his business. Here, then, is the gist of the matter: Are the generality of assistants able to undertake such a charge? Gentlemen of your correspondent's calibre would confidently answer in the affirmative. The master druggists, to a man, would *emphatically* answer in the negative; and many of them could furnish damning proofs of the utter inefficiency and ignorance of three fourths at least of those who offer themselves, with as much confidence as if they had all the Pharmacopœias by heart. I need not inform you, Gentlemen, of the hordes of youths who are continually arriving in the metropolis from the country in search of employment, armed with letters of recommendation, and ambitious of first-rate situations. Place one of these candidates for office behind the counter of a respectable retail concern, or in its laboratory,—in both of which the greatest precision and carefulness are required,—and give him an ordinary physician's prescription to compound, or a retort, &c. to set in a sand-bath; having left him for half an hour, return and ascertain the progress he has made, and what would be the probable result? Why, that he could not read the Doctor's scrawl, or perhaps staring with all his eyes at the apparatus put under his charge, wondering what on earth *can* be made of such ugly, awkward-looking lumps of badly-shaped glasswork. Oh! but some one will say, this is a mere caricature. It is nothing of the kind, but a veritable portrait of almost the whole genus—and the cause is obvious. Most of the young men

turned out by country druggists have the principal part of their profession yet to learn,—and why? Because, from almost a total want of a *decent* education to begin with, and the short period they serve as apprentices, together with their own idleness and inattention to study, they are nearly useless when they ought to be far otherwise. Talk of twenty pounds a year indeed!! Why, such *things* as them ought never to be received into a respectable establishment without the accompaniment of a handsome sum for the inconvenience, disappointment, and trouble which invariably attend them. And yet it is of such as these your correspondent speaks, when he contemplates petitioning in their behalf that they may be allowed a portion *of each day* wherewith "to recreate themselves." Bah! the impudence of such a notion turns one's stomach, and is enough to drive one to join the ranks of that conglomeration of humbug, the P. S. G. B. Let your London Assistant and former correspondent descend from his stilts and listen to reason, and I will furnish him with a rule or two for the future government of the whole drug trade, of more value than all the absurdities that have emanated from the erudite brains of the Society of incapables above alluded to:—

Rule 1.—Let chemists and druggists refuse to take any apprentice who has not received a suitable education, and whose parents cannot afford to support him with *every necessary* for a period of seven years.

Rule 2.—Let them refuse to take any youth as an apprentice for any period short of seven years.

Rule 3.—Let no assistant be engaged as such who cannot produce an indenture for that period, as well as a certificate from his master that he has faithfully and fully complied with its conditions.

These rules having been conformed to, the public will at least have the satisfaction of knowing that a sufficient time has been afforded young men to qualify themselves for the responsible situations they are expected to fill; and he must be a dunderpate indeed, who cannot, during such an apprenticeship, acquire the information and manual dexterity which every assistant ought to possess.

Is it not notorious that most of young men seek after situations in London, and other large cities, as much for obtaining information as for salary? and is it for your correspondent and his compeers to seek by act of parliament to compel masters not only to teach these tyros their business, but to give them such an amount of remuneration as I hesitate not to say few of them are worth?

I am, Gentlemen,
Very respectfully yours,
PESTLE.

P. S.—There are other points of your correspondent's communications which require remark; but more of these hereafter.

THE ELEMENTS OF MATERIA MEDICA AND THERAPEUTICS. By JONATHAN PEREIRA, M.D., F.R.S. and L.S., &c. &c. &c. Second Edition, enlarged and improved. London: Longman, Brown, Green, and Longmans. 1842. pp. 1926.

IN conformity with the promise given in our last Number, we proceed to examine the contents of the second volume of this masterly work. Space will not admit of our giving the author's classification in full; we therefore content ourselves with observing, that the author devotes this volume to the organised kingdom of the Materia Medica, divided into the vegetable and animal sub-kingdoms. The principal divisions to which he subjects the vegetable subkingdoms, are into Cryptogamia, or flowerless plants, and Phanerogamia, or flowering plants. We omit the subdivisions.

In that portion of the work allotted to the vegetable sub-kingdom of the Materia Medica, Dr. Pereira gives, in his usual lucid style, the natural history of every vegetable used in medicine, its physical properties, its composition (when determined) and characteristics, its officinal preparations, and their physiological effects, and their uses, with full details concerning the method of producing them. Wherever it is necessary, he resorts to the use of woodcut illustrations.

The animal sub-kingdom is divided into Invertebrata and Vertebrata: the first being subdivided into Acrita, Radiata, Mollusca, Articulata; the second into Pisces, Aves, and Mammalia; under the subdivision of Mammalia, are given the four orders of Cetacea, Ruminantia, Pachydermata, and Rodentia. We here give an extract, as somewhat of an example of the general system of treating the subjects of this volume adopted by the author:—

ORDER XXVIII. PIPERACEÆ—(KUNTH). THE PEPPER TRIBE.

3. Piper Cubeba, *Linn.* L. E. D. The Cubeb Pepper.

Sex. Syst. Diandria Trigynia. Dianebria Trejynia.

(Baccæ cubebæ, L. Fruit, E. Fructus, D.)

HISTORY.—It is uncertain when the cubebs of our shops were first introduced

into medicine, or who first alludes to them. There does not appear to be any foundation for the opinion that the ancient Greeks were acquainted with them. "Many, indeed, pretend that the carpesion (καρπήσιον) of Galen is our cubeb, and that the *round pepper* of Theophrastus, the pepper of Hippocrates, were all names for them; but this is a conjecture founded on a very bad basis. The Arabians are at the head of these blunders. Serapion has translated all that Galen says of carpesion into his chapter on cubebs, and attributed all its virtues to it; and has even added every thing to the account that Dioscorides has left us of the *Ruscus*. Avicenna is also in the same error, and calls the carpesium *cubeb;* and from these authors Actuarius and the other Greeks have collected their accounts. It is plain, from all this, that either the carpesium of the Greeks or the cubeb of the Arabians are the same things, or else that the Arabians have been guilty of confounding different things in a strange manner together; if the latter be the case, there is no judging of any thing from what they say; and if the former, it is very evident that our cubebs are not the same with theirs, that is, with the carpesium of Galen; for he expressly assures us that this was not a fruit or seed, but, as he tells us, a kind of slender woody twig, resembling in smell and virtues the root of the valerian. Nothing is more evident than that the carpesium, therefore, was either a fibrous root, or the small twigs and branches of a climbing plant, not a round, small fruit. If the Arabians, therefore, were acquainted with our cubebs at all, it appears that, not knowing what the carpesium and ruscus were, they ignorantly attributed the virtues ascribed by the Greeks to those medicines to these fruits." *

Cubebs were used in England 500 years ago, for in 1305, Edward I. granted to the corporation of London the power of levying a toll of one farthing a pound on this article in its passage over London-bridge. †

Botany.—*Gen. Char.* Vide *Piper nigrum. Sp. Char.* Stem shrubby, terete, climbing. *Leaves* petiolate, oblong, or ovate-oblong, acuminate, rounded, or oblique, cordate at the base, nerved, coriaceous, smooth. *Peduncles* almost equal to the petiole. *Berries* with elongated peduncles. (Blume. ‡)

Dr. Blume says that the cubebs of the shops are the fruit of *P. Caninum*, which has a smaller and shorter-stalked fruit, having a distinct anise flavor, and less pungency than the fruit of *P. Cubeba;* but Dr. Lindley* observes, that he cannot perceive any difference in the flavor of the dried fruit of *P.* Cubeba and of the cubebs sold in the London shops. *P.* Cubeba is readily distinguishable from P. Caninum by the leaves being coriaceous, smooth, and shining, with the veins proceeding from the sides of the midribs, not from its base.

Hab.—Java, and the Prince of Wales's Island.

Description.—The dried unripe fruit of this plant constitutes the *cubebs* (*cubebæ* vel *piper caudatum*) of the shops.

In appearance, cubebs resemble black pepper, except that they are lighter colored, and are each furnished with a stalk two or three lines long, and from which circumstance they have received their name, *caudatum.* The cortical portion of cubebs (that which constituted the fleshy portion of the fruit) appears to have been thinner and less succulent than in black pepper. Within, it is a hard, spherical salt, which is whitish and oily. The taste of cubebs is acrid, peppery, and camphoraceous; the odor is peculiar and aromatic.

Composition.—Three analyses of cubebs have been made: one by Trommsdorff, in 1811; † a second, by Vauquelin, in 1820; ‡ and a third by Monheim, in 1835. §

VAUQUELIN.

1. Volatile oil, nearly solid.
2. Resin, like that of copaiva.
3. Another colored resin.
4. A colored gummy matter.
5. Extractive.
6. Saline matter.

Cubebs.

MONHEIM.

1.	Green volatile oil	2·5
2.	Yellow volatile oil	1·0
3.	Cubebin	4·5
4.	Balsamic resin	1·5
5.	Wax	3·0
6.	Chloride of sodium	1·0
7.	Extractive	6·0
8.	Lignin	65·0
	Loss	15·5
	Cubebs	100·0

1. Essential Oil of Cubebs.—See p. 1108.

2. Resin of Cubebs.—Vauquelin has

* Hill, *Hist. of the Mat. Med.* p. 473.

† *Liber Niger Scaccarii*, vol. i. p. 478. Also the *Chronicles of London-bridge*, p. 155.

‡ *Ermen. Fl. Java*, p. 70.

* *Flora Medica.*

† Schwartz, *Pharm. Tabell.*

‡ *Ann. Phil.* 2nd Series, vol. iii. p. 202.

§ *Journ. de Pharm.* xx. 403.

described two resins of cubebs: one is green, liquid, acrid, and analogous, both in odor and taste, to balsam of copaiva; the other is brown, solid, acrid, and insoluble in ether.

3. Cubebin (*Piperin*).—From cubebs is obtained a principle to which the term *cubebin* has been applied. It is very analogous to, if not identical with, *piperin*. Cassola, a Neapolitan chemist,* says it is distinguished from the latter principle by the fine crimson color which it produces with sulphuric acid, and which remains unaltered for twenty or twenty-four hours; moreover, cubebin is not crystallisable.

Monheim,† however, declares cubebin to be identical with piperin, and that it is combined with a softer acrid resin. In this state it is soluble in ether, alcohol, the fixed oils, and acetic acid; but it is insoluble in oil of turpentine and dilute sulphuric acid. It fuses at 68° F.

Dr. Görres gave cubebin in both acute and chronic gonorrhœa, to the extent of one drachm, four times daily. But he premised the use of phosphoric acid.

4. Extractive Matter of Cubebs.—Vauquelin says, the extractive matter of cubebs is analogous to that found in liguminous plants.‡ It is precipitable by galls, but not by acetate of lead.

Physiological Effects.—Cubebs belong to the acrid species, already (p. 181) noticed. Their sensible operation is very analogous to that of black pepper. Taken in moderate doses, they stimulate the stomach, augment the appetite, and promote the digestive process. In larger quantities, or taken when the stomach is in an irritated or inflammatory condition, they cause nausea, vomiting, burning pain, griping, and even purging. These are their local effects. The constitutional ones are those resulting from the operation of an excitant,—namely, increased frequency and fulness of pulse, thirst, and augmented heat. It probably stimulates all the mucous surfaces, but unequally so. In some instances, cubebs give rise to an eruption on the skin, like urticuria. Not unfrequently they cause head-ache; and occasionally disorder of the cerebro-spinal functions, manifested by convulsive movements, or partial paralysis, as in a case related by Mr. Broughton.§

Cubebs appear to exercise a specific influence over the urino-genital apparatus. Thus they frequently act as diuretics, and at the same time deepen the color of, and communicate a peculiar aromatic odor to, the urine.

Their stimulant operation on the bladder is well illustrated by a case related by Sir Benjamin Brodie.* A gentleman, labouring under chronic inflammation of the bladder, took fifteen grains of cubebs, every eight hours, with much relief. Being anxious to expedite his cure, he of his own accord increased the symptoms: the irritation of the bladder was much increased, the mucus was secreted in much larger quantity than before, and ultimately the patient died,—"his death being, I will not say occasioned," says Sir Benjamin, "but certainly very much hastened, by his imprudence in over dosing himself with cubebs."

Three drachms of cubebs caused, in Päl,† nausea, acid eructations, heat at the pit of the stomach, head-ache, uneasiness, and fever.

Uses.—The principal use of cubebs is in the treatment of *gonorrhœa*. They should be given in as large doses as the stomach can bear, in the early part of the disease; for experience has fully proved, that in proportion to the length of time gonorrhœa has existed, the less amenable is it to the influence of cubebs. In some instances, an immediate stop is put to the progress of the malady; in others, the violent symptoms only are palliated; while in many (according to my experience in most) cases no obvious influence over the disease is manifested. The presence of active inflammation of the urethra does not positively preclude the use of cubebs, though I have more than once seen them aggravate the symptoms. Mr. Jeffreys thinks the greatest success is met with in the more inflammatory forms of the disease. Cubebs have been charged with inducing swelled testicle; but I have not observed this affection to be more frequent after the use of cubebs, than when they were not employed. Mr. Broughton‡ gave them to fifty patients, and in forty-five they proved successful. Of these only two had swelled testicle. The explanation of the *methodus medendi* is unsatisfactory. Sir A. Cooper§ thinks that cubebs produce a specific inflammation of their own on the urethra, which has the effect of superseding the gonorrhœal inflammation. The occasional occurrence of a cutaneous eruption from the use of cubebs deserves

* *Journ. de Chim. Méd.* t. x. p. 685.

† *Op. Cit.*

‡ *Dierbach, Newton, Eutd. in d. Mat. Meth.* S. 253. 1837.

§ *Lond. Med. Gaz.* vol. i. p. 405.

* *Ibid.* vol. i. p. 300.

† *Arsneim*, ii. Giften, bd. iv. s. 217.

‡ *Med. Chir. Trans.* vol. xii. p. 99.

§ *Lancet*, vol. iii. p. 201. 1834.

especial attention, as I have known it create a suspicion of secondary symptoms.

Cubebs have been recommended in gleet and leucorrhœa.* In abscess of the prostate glands, twenty or thirty grains of cubebs, taken three times a day, have, in many cases, appeared to do good.† They seemed to give a gentle stimulus to the parts, and to influence the disease much in the same way that Ward's paste operates on abscesses, and fistulæ, and ulcers of the rectum. In cystirrhœa also they have occasionally proved serviceable in small doses.‡ In piles, likewise, they are given with advantage.§

The efficacy of cubebs in mucous discharges in not confined to the urino-genital mucous membrane. In catarrhal affections of the membrane lining the acrian passages, it proves exceedingly useful, especially when the secretion is copious, and the system relaxed.

Formerly, cubebs were employed as gastric stimulants and carminatives in dyspepsia arising from an atonic condition of the stomach. They have also been used in rheumatism. The Indians macerate them in wine, and take them to excite the sexual feelings.

Administration.—Cubebs, in the form of a *powder*, are given in doses varying from ten grains to three drachms. In affections of the bladder and prostate gland, the dose is from ten to thirty grains. In gonorrhœa, on the other hand, they should be administered in large doses. Mr. Crawford‖ says, that in Malay countries they are given in doses of three drachms, six or eight times during the day.

1. Oleum Cubebæ, E. Volatile Oil of Cubebs.—(Prepared by grinding the fruit, and distilling with water.) By distillation, cubebs yield about 10·5 per cent. of a transparent, slightly colored (when pure, colorless), volatile oil, which is lighter than water (sp. gr. 0·929), and has the cubeb odor, and a hot, aromatic, bitter taste. It is composed of carbon and hydrogen, in the same relative proportion as in oil of turpentine; but its formula is C^{15} H^{12}.

By keeping, it sometimes deposits crystals (*cubeb stearoptine*, or *cubeb camphor*), the primary form of which is the rhomboid octohedron. Their odor is that of cubebs; their taste, at first, that of cubebs and camphor, afterwards cooling. They are fusible at 133° F., soluble in alcohol, ether, and oils, but are insoluble in water. Their composition is C^{16} H^{14} O, so that they are the hydrate of the oil of cubebs.* Oil of cubebs is an excellent and most convenient substitute for the powder. The dose of it, at the commencement of its use, is ten to twelve drops. This quantity is to be gradually increased as long as the stomach will bear it. In some instances, I have given it to the extent of a fluid drachm for a dose. It may be taken suspended in water, by means of mucilage, or dropped on sugar. *Gelatinous capsules of cubebs*, containing the oil of cubebs, are prepared by Mr. Willdenow. The mode of preparing these will be described when noticing the gelatinous capsules of copaiva. A combination of oil of cubebs and oil of copaiva forms a very useful medicine in some cases of gonorrhœa.

On the Continent, a preparation called the *oleo-resinous extract of cubebs* is used. It is prepared by adding the oil to the resinous extract of cubebs, which is prepared by digesting the cake left after the distillation of the oil in alcohol, and distilling off the spirit.†

2. Tinctura Cubebæ, L.; *Tinctura Piperis Cubebæ*, D. Tincture of Cubebs.—Cubebs, ℥ v [℥ iv D.]; Rectified [Proof, D.] Spirit. O ij [wine-measure, D.] Macerate for fourteen days, and filter. Dr. Montgomery‡ says, "I have found this tincture cure gonorrhœa both speedily and satisfactorily." The dose of it is one or two drachms, three times a day.

other non-officinal piperaceæ.

The Piper Betle is extensively used by the Malays, and other nations of the East, who consider it as a necessary of life. The mode of taking it in Sumatra consists simply in spreading on the *sirih* (the leaf of the Piper Betle) a small quantity of *cluseram* (quick-lime prepared from calcined shells), and folding it up with a slice of *pinang* or Areca nut (vide pp. 203 & 936). From the mastication there proceeds a juice which tinges the saliva of a bright red, and which the leaf and nut without the lime will not yield. This hue being communicated to the mouth and lips, is esteemed ornamental, and an agreeable flavor is

* Dr. Orr, *Ed. Med. Journ.* vol. xviii. p. 318.

† Sir B. Brodie, *Lond. Med. Gaz.* vol. i. p. 396.

‡ *Ibid.* p. 300.

§ *Ibid.* vol. xv. 747.

‖ *History of the Indian Archipelago*, vol. i. p. 465.

* Brooke, *Ann. Phil.* N. S. vol. v. p. 450.

† *Journ. de Pharm.* t. xiv. p. 40.

‡ *Observ. on the Dubl. Pharm.* p. 439, Lond.

imparted to the breath. The juice is usually, but not always, swallowed. To persons who are not habituated to this composition, it causes giddiness, astringes and excoriates the mouth and fauces, and deadens for a time the faculty of taste. Individuals, when toothless, have the ingredients previously reduced to a paste, that they may dissolve without further effort.*

We give the following from the animal sub-kingdom.

ORDER III.—HYMENOPTERA, Linn.

Apis mellifica, *Linn.* L. E. D.—The Hive Bee or Honey Bee.

(1. Humor è floribus decerptus et ab ape preparatus, L.—Saccharine secretion, E.—Mel. D. 2. Cera; Concretio ab ape paratum; Cera alba; Idem dealbatum, L.—Cera flava; Waxy secretion; Cera alba; Bleached bees; E.—Cera alba; Cera flava, D.)

History.—This animal was very anciently known, and is frequently referred to in the Old Testament. In all ages it has been an object of admiration and attention, on account of its industry, curious economy, and policy.

Zoology. *Gen. Char.—Labium* filiform, composing, with the jaws, a kind of *proboscis*, geniculate, and bent downwards. First joint of the posterior *tarsi*, large, compressed. No spines at the extremity of the last two *legs*. Upper *wings* with one radial and three cubital cells. (*Stark.*)

Sp. Char.—Blackish. Abdomen of the same color, with a transverse greyish band formed by the down at the base of the third and following segment. (*Stark.*)

The honey bee lives in societies, called *swarms*, consisting of from fifteen to thirty thousand individuals, *viz.* a female, males, and neuters. The female, called the *queen bee*, is narrower and longer than the others. The males, termed *drones*, are smaller than the females, and are devoid of stings. In each hive there are from 800 to 1000 drones. Towards autumn, when they can be of no farther use, they are destroyed by the neuters. These neuters are termed *working bees*, and are by far the most numerous, since in each hive there are from fifteen to thirty thousand. They are, in reality, females, whose ovaries are not developed, in consequence, as some have supposed, of the nature of the aliment with which they are supplied while in the larva state.

The *digestive system* of the animal consists of highly developed *salivary organs*, communicating with the proboscis; of an œsophagus (which enlarges at one part from the *crap, sucking stomach*, or *honey bag*), *a proper stomach, small and large intestines*, and *biliary vessels*. The latter open into the alimentary canal immediately behind the stomach. The Sexual System, in the *male*, consists of *a pair of testicles*, each having a *vas deferens*, which terminates in a *vesicula seminalis*. From the conjoined extremities of the vesicula proceed *a common duct* terminating in a *penis*. The *female genital organs* consist of *two ovaries* made up of tubes, each containing about twelve *ova*; the *two oviducts* from these ovaries terminate in a *vagina*, into which also opens *a duct*, from *a roundish vesicle*. The Poison Apparatus is found in the females and neuters only. It consists of *two thin convoluted secreting organs*, opening into a *pyriform receptacle*, from which *a small duct* passes to the *sting*, which consists of two portions placed side by side, barbed at the extremity, and contained in a sheath. The *poison* is said to be hot and acrid to the taste. The consequences produced by the sting of a bee, are pain, redness, swelling and hardness of the part; and might prove fatal if a swarm were to attack an individual. The removal of the sting (if left within the wound), and friction with saliva, or with oil and hartshorn, is all the treatment usually required.

Habits.—Old Continent (*Latreille*). In a state of nature they reside in hollow trees; but they are almost universally domesticated, and are preserved in *hives*. Curtis * has described and depicted a remarkable instance of the nest of some hive bees attached to the arm of a tree. It was discovered in 1838, by Lord Malmesbury, in his plantation near the river Avon.

Bees furnish two products useful in medicine, *viz.—honey* and *wax*.

Honey. *Production.* — Honey (*mel*) is secreted by the nectariferous glands of flowers, and is collected by the working or neuter bees, who take it up by suction or lapping, and pass it into the dilatation of the œsophagus denominated *crap, sucking stomach*, or *honey-bag*; beyond which, we presume, the honey does not pass, as it has never been found in a true stomach. When the animal arrives at the hive, the honey is disgorged by a kind of inverted peristaltic motion, and is probably somewhat altered in its properties by the secretions of the crap. It is used by the animal as food.

Physical Properties.—Honey varies in its taste and odor according to the age of the bees, and the flowers on which they have fed. A hive which has never swarmed is considered to yield the best, which is,

* Marsden, *Hist. of Sumatra*, 3rd ed. p. 281.

* *Brit. Entomol.* xvii. 769.

therefore, *virgin* honey. The flavor of the Nartomie honey, which is so much admired, is said to arise from the labiate flowers on which the animals feed; to imitate this, a sprig of rosemary is sometimes added to the honey obtained from other places.

Purity.—Flour, it is said, is now and then mixed with honey. It may be readily distinguished by its solubility in cold water, and by the blue color produced by the addition of iodine.

The London College directs that honey is not to be employed without being despumated. Dissolved in water, iodide of potassium and acid being added, it does not become of a blue color.

Chemical Properties.—The constituents of honey vary somewhat according to the food of the bees, the season, the age of the animals, the mode of extracting it from the combs, &c. It must, however, be regarded at all times as a concentrated solution of *sugar* mixed with *odorous coloring gummy* and *waxy* matters. The saccharine matter is of two kinds; one crystallisable, and analogous to the sugar of grapes; the other uncrystallisable, and similar to the uncrystallisable brown syrup of the sugar-cane. Guibourt has found also mannite, which differs from sugar in not fermenting when mixed with water and yeast.

Physiological Effects. — Honey is emollient, demulcent, nutritive and laxative. When fresh it is apt to occasion indigestion and colic. Collected from poisonous plants, it has been found to possess deleterious qualities. Mr. Abbot* says it causes violent head-ache, vomiting, and a condition like that of a tipsy man. A larger dose produces deprivation of all sense and power for some hours afterwards. These effects agree with those assigned to this honey by Xenophon,† in his account of the "retreat of the ten thousand." Pliny ‡ also speaks of this poisonous honey. Tournefort § ascribes its venomous properties to the bees feeding on the *Azalea pontica*. Many other instances of poisonous honey are on record. ‖

Uses.—Mixed with flour, and spread on linen or leather, it is a popular application to promote the maturation of small abscesses and furunculi. It sometimes forms a constituent of gargles, partly on account of its taste, partly for its emollient operation. It is also used as a vehicle for the application of other more powerful agents to the mouth and throat, especially in children. It is occasionally employed as an emollient and demulcent in inflammatory affections. In troublesome coughs, barley water, mixed with honey, and sharpened with slices of lemon, and taken warm, forms a very agreeable and useful demulcent to allay troublesome coughs.

1. Mel Despumatum, D.; *Clarified Honey.*—(Melt the honey in a water bath, and remove the scum.) The object of this process is to deprive honey of certain impurities which render it apt to ferment; but the flavor and odor of the honey is somewhat injured by the operation.

2. Oxymel.—See p. 404.

We refer our readers to the opinion of this work expressed in our last Number.

ON THE FERRI POTASSIO-TARTRAS OF THE LONDON PHARMACOPŒIA.

BY MR. G. M. MOWBRAY.

The process now recommended by the London and Edinburgh Pharmacopœias, is stated by Dr. Collier and Mr. Phillips as being a *modification* of that of M. Soubeiran, whilst Dr. Christison states that the process now adopted by the London and Edinburgh Colleges *is* that of Soubeiran. The modification appears to me very important, and in modifying it, those engaged therein, evidently, were not practically acquainted with the subject, and this has perhaps given rise to the statement, that "the precise part which ammonia acts in the ferri potassio-tartras of the London Pharmacopœia is far from obvious." See advertisement, Medical Gazette, May 6, 1842. Having prepared a hydrated sesquioxide of iron, the College directs its solution in bitartrate of potassa, (upon this I beg to remark that neutral citrate of ammonia will dissolve hydrated peroxide of iron, and so also will neutral tartrate of potassa. M. Woehler (Ann. der Pharmacie, xxxiv. 235,) finds indeed that carbonate of ammonia (as well as the bicarbonate) will also dissolve it, but Berzelius remarks in reference thereto, (Rapport Annuel, 1842, p. 84,) that he has found, on diluting the solution, it is entirely precipitated). The result of the solution as directed by the College is the formation of a tartrate of potassa and sesquioxide of iron, and also a tartrate of sesquioxide of iron. My view is then opposed to that of Dr. Pereira, whose diagram and theory of its composition may be perused in his most laborious and excellent work, *The Elements of Ma-*

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* *Lond. and Edinb. Phil. Mag.* vol. v. p. 313, for Oct. 1834.

† *Anabas.* lib. iv.

‡ *Hist. Nat.* xxi. 44 ed. Valp.

§ *Hist. de l'Acad. Roy. des Sciences*, 1704, p. 351.

‖ See *Boston Phil. Mag.* vol. xii. p. 121; and in *Rich's Med. Jurisprud.*

teria Medica and Therapeutics, p. 864. Dr. P. quotes the atom of ferrotartrate of potash at 220, as seen by the following diagram :—

PEREIRA.

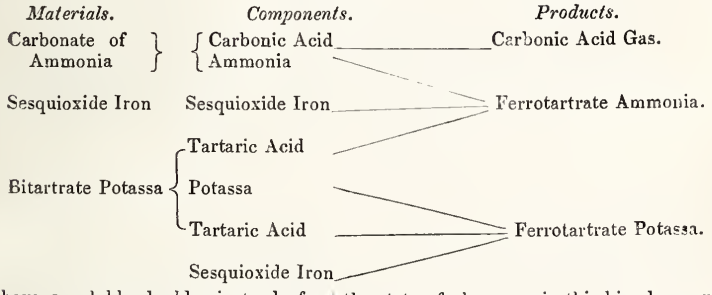
Materials.

1 eq. Sesquioxide Iron 40 ——— } 1 eq. Tartrate Sesquioxide Iron 106
1 eq. Bitartrate Potash 180 { 1 eq. Tartaric Acid 66 } ———— } =
1 eq. Tartrate Potash 114 ———

Products.

= 1 eq. Ferrotartrate Potash 220.

PROPOSED.



To have a soluble double, instead of a quadruple salt, I beg to suggest the addition of carbonate of potash in lieu of carbonate of ammonia, and which yields a beautiful preparation. I have purposely omitted all mention of equivalents in the diagram I have suggested, for this reason, that certain investigations which I have commenced in reference to the salts of iron, lead me to infer that they do not combine simply equivalent to equivalent, but rather perhaps several equivalents of sesquioxide of iron, forming one compound atom, to one atom of acid; and besides, I dislike that conjectural chemistry which Berzelius has stigmatised as the great bane of the science. Unless we (the great masters of chemistry excepted) have absolutely analysed the salts of iron we attempt to describe, I should be inclined to place very little reliance upon conjectures. The tartaric being an acid which, as forming bibasic salts, is very complicated in its combinations, deserves most certainly an attentive study; but conjectures must be deprecated; they frequently deter others from pursuing an inquiry which might really develope the truth, and only serve in themselves as a poor apology for the industry of him who makes them. They are the next unworthy step to false formulæ, of which M. Beral has furnished one in the citrate of iron, thereby rendering himself a very questionable authority in any further communications.

May I add, in conclusion, that I believe the recent introduction of several preparations long since in use on the Continent, for some of which we are indebted to Mr. Bullock, is having the best possible effect on the state of pharmacy in this kingdom, and it is to be hoped that the production of the next Pharmacopœia will be delegated to those who can and will give their whole attention to the subject, and who are perfectly and practically acquainted with every article for which they give formulæ, and by which every conscientious chemist and druggist in the kingdom, be it remembered, is bound.

G. M. MOWBRAY.

22, Regency Square, Brighton.

ON THE SELECTION OF SUGAR FOR THE PREPARATION OF SYRUP OF VIOLETS.*

BY J. B. LABORDETTE.

M. LABORDETTE writes to the Editor of the *Journal de Chimie Médicale*:—I have read in a recent No. of the *Journal de Pharmacie*, a letter from M. Blondeau, relative to the preparation of syrup of violets. That *Pharmacien*, in recommending the cribration of the petals of the violets before using them in the preparation of syrup, gives essentially useful advice, for this operation is in some measure indispensable. But notwithstanding this precaution, notwithstanding the washing advised by the best authorities, notwithstanding the extreme carefulness exercised either in the cutting or in the separation of the petals, I have seen syrup prepared in the same vessels, in the same manner, and in the same circumstances, most frequently not without altering, even

* *Journal de Chimie Médicale*, Juillet, 1842.

after several years, sometimes alter spontaneously, at other times only a few hours after its preparation. Being struck with so singular an anomaly, and not being able to attribute the alteration of color either to the yellow part of the petals, or to the *débris* of the calixes, or to the stamens, I turned my attention to the sugar. I obtained from different grocers some sugar of the finest quality, and I operated separately on the same infusion: one gave me a very fine syrup; two changed into green the blue color of the infusion, and another turned it red. These results, which were somewhat remarkable from their contradictoriness, led me to admit that certain sugars furnish, some acid, and some alkaline reactions; also, in order to avoid the losses which may become important to those who prepare a large quantity of syrup of violets, I have always taken care, since I have obtained these results, to test the sugar with a small quantity of infusion, and I have not regretted the time thus employed.

Note of the French Editor.—An observation made by M. Labordette, concerning the acidity and alkalinity of sugar, deserves to be taken into considcration, not only by pharmaceutists for the preparation of syrup of violets, but also by all persons who prepare liquors containing sugar and vegetable coloring matters susceptible of changing color by acids and by alkalis. We have very recently been consulted by a liqueur manufacturer, who having prepared Curaçoa, for which he has a very good recipe, obtained a black instead of the requisite color. This change of color was found to be owing to the acidity of the sugar employed.

This Curaçoa was brought to the right color by means of a few drops of an alkaline solution.

ON THE ELECTRICITY OF MINERAL VEINS.

INTRODUCTION BY ROBERT HUNT.

In a memoir on Mineral Veins, by Robert Were Fox, Esq., in the *Fourth Report of the Polytechnic Society*, will be found some very valuable remarks and experiments on the filling of fissures; the formation of the mineral deposit therein being in many cases referred to electrical agency.

It has been shown by Becquerel and Crosse, that metallic and earthy minerals and siliceous crystals can be formed by weak electric currents; and the conversion of copper pyrites into grey or vitreous copper by the same agency, has been most conclusively proved by Mr. R. Fox. This last fact becomes the more interesting, when it is known that many striking instances occur in our mines, from which we may conclude that a similar process of decomposition has taken place in nature, as it occurs in the experiment, *i. e.* the separation of the peroxide of iron from the yellow ore* which is, as was first noticed by Mr. Joseph Carne, found in many cases on either side of the lode containing the grey ore.

In addition to these, in proof of the electricity in the formation of metallic lodes, may be adduced the experiments by Mr. R. W. Fox, in which artificial veins of carbonate of copper and zinc were obtained in a wall of clay placed between the poles of a galvanic series. This experiment, successfully repeated by Mr. T. B. Jordan, will be found in the *Sixth Annual Report of the Society;* to which, and to the beforementioned Memoir, I beg to refer.

Numerous experiments instituted in the mines of this county by Mr. R. W. Fox have proved in a satisfactory manner that the mineral veins are traversed by the electric fluid; and in following out these interesting researches, I have had the satisfaction of proving that chemical decomposition may be effected by these currents. With a view to elucidate the phenomena connected with these mineral formations, the present series of inquiries have been undertaken by myself and Mr. John Phillips of Tuckingmill. The object is two-fold:—In the first instance, it is of philosophical importance to ascertain if these currents have any determinate direction independent of the local causes which may sometimes disturb or alter their course. Secondly, which is of the most practical bearing, to examine if the direction and force of the current has any relation to the masses of ore in different parts of the lode, as it regards their richness or quantity, by which indications of the greatest value to the miner might be afforded.

The experiments of Professor Reitch, in the mines of Germany, render it exceedingly probable that these researches may lead to the means of detecting the existence of copper ore some distance behind the rock.

Without particularly describing the instruments used, which are a galvanometer of considerable delicacy, with two needles and numerous coils of clothed copper wire, and another having but one needle and a few coils of wire, I would state the plan adopted to secure perfect contact between the wires and the ore. The copper wire used is about the eighth of an inch in diameter, and one end of each coil is firmly secured by binding screws into the head of large brass screws,

* See *London and Edinburgh Philosophical Magazine*, Dec. 1841.

which are fixed to blocks of well baked mahogany. The blocks are jambed against the rock, and the large screws turned until their points press firmly against the ore, in holes; by which means very perfect contact is obtained. The other ends of the coils—the wires being connected with two parts of a lode, or two different lodes—are brought into the mercury cups of the galvanometer. The circuit being thus made up through the instrument and the ground, the force and direction of the current through the wires are indicated by the reflections of the needle.

It is often necessary to move one of the coils over a considerable extent of the levels of the mine. The end is then removed from the block connective and screwed into a brass ball fixed on the end of a staff, and this ball is pressed by an assistant firmly against any part of the hole.

As platina and zinc have very different electrical relations to those of copper pyrites, they afford us a means of determining in the most satisfactory manner, whether the current is flowing through the mineral vein, or merely excited by the act of making contact. If the latter were the case, the direction of the current would be altered; but it would remain unchanged if the former, which has been found invariably the case.

RESEARCHES AND OBSERVATIONS ON THE ELECTRICITY OF MINERAL VEINS.

BY ROBERT HUNT AND JOHN PHILLIPS.

1. The whole of the researches included in this series were made in East Pool copper mine, about seventy fathoms below the surface, on two lodes, each underlying S.—the north lode about one foot six inches in a fathom, and the south lode one foot in a fathom (*i. e.* where the experiments were conducted). The lodes, it may be remarked, are very irregular in their bearing and underlie, being most productive where they approach the nearest to the perpendicular. It is often observed in this mine, that bunches of ore frequently lie between craggy walls, and Elvan is by no means considered an unfavorable indication.

In pursuing these investigations, several sources of slight error have been detected, which will be noticed as they occurred in each set of experiments; it being a point of great importance to secure other observers, from the inaccuracies to which these give rise.

2. The connection being made between the point 1 (plate 1, fig. 2), and the instrument at *a*, the other wire was carried along the level; on being taken around the corner, marked 5 in the plan, the needle was deflected 15°, although no contact was made with the lode, and when contact was made in the north lode at 4, this deflection was arrested, the needle standing at 2°. This was soon found to arise from the wire being in contact with a pile of iron pyrites, 5, which giving out a positive current occasioned a deflection of the needle to the W.; when this was prevented and close contact was again made with the mass of ore at 3, the deflection of the needle indicated a current passing from W. to E. The greatest deflection we succeeded in getting on this occasion, was 45°. The interposition of plates of zinc or platina were found slightly to check the current, but never to change its direction.

3. Connection was made and broken between the wire and the lode to an isochronous time, by the person producing contact, which not agreeing with the oscillations of the needle, produced variable intensities of deflection, as they were aggravated or retarded by the difference of times.

4. Various points along the top or back of the level, between points *a* and 5, were next tried; it was found wherever the contact was made with ore, that the needle was deflected as before (2), but no deflection could be excited by any contact with the rock. This was most satisfactorily determined; in proof of which may be quoted the remark of the miners, who witnessed the experiment—"The little thing knows ore, but doesn't know the country."

5. The galvanometer was now fixed at the point *b*, and the permanent connection made still near the point 1, being a distance of about fifteen fathoms; the large coil was connected as before with the point 4. The contact being made perfect at both ends, the needle of the galvanometer made several entire revolutions, by the single impulse of the current passing from 1 through point 6 to point 4, and back to point 1, or from W. to E.

This being satisfactorily determined, no further experiment was tried on this occasion, owing to the extremely bad state of the air in the mine.

6. The galvanometer — one having a heavy single needle, and consequently not very readily affected by feeble currents— was fixed at the point *a*, and the connection made with a large mass of copper ore at 3. It was noticed on this occasion that a deflection of 2° or 3° was produced between the *large coil of wire* in contact with the damp rock, and the sulphuret of copper in the lode. To ascertain if the current was at all connected with the rock, the coil was placed on a damp board, when the deflection was found to be the same as be-

fore; clearly proving it to arise from the state of the metal considered in its electric relation to the ore. Contact was made by the large coil at 2, when the needle was deflected nearly 70°, but the direction of the current was now from E. to W.* This, being contrary to the direction ascertained in former experiments, demanded the most rigorous investigation. The current was analysed by the plates of zinc and platina, but its direction still continued the same.

7. It was agreed that the observer should number aloud the oscillations of the needle, in such order that the even number should indicate the moment at which the needle arrived at its extreme deflection, raising or lowering his voice according to the amount deflected. The object of this was, that the person producing contact should make his own observations on the nature of the contact; which arrangement enabled him, first, to discover that contact with the ore of the lode was followed by an elevation of voice; also that the presence of water enveloping the end of the wire in connection with the ore, was productive of greater deflection, but had no effect when applied to the matrix of the lode or its sides.

8. Contact was made in many places along the cross course, between the north and south lode, without producing any deflection of the needle; but as soon as the wire touched the ore in the south lode at 6, the needle was powerfully deflected in the direction indicating a current flowing from W. to E., as in the former experiments.

9. The connections were made exactly as on the last occasion, and the same results were produced, the direction of the current between the point 2 in the N. lode, and point 3 in the S. lode, being from E. to W. The large coil was now connected with that portion of the N. lode lying at the point 7, being carried round through the cross course, giving an extended length of wire of about twenty-two fathoms. The deflections were now 90°, and the current from W. to E., which is the greatest steady deflection yet obtained with the heavy needle.

10. The large galvanometer and wire being fixed as in the last experiment, the wire was carried round to the eastern end of the lode, and the connection made with point 7; the current was found to be in the same direction, from W. to E. On this occasion the current was not allowed to flow through the whole of the wire on the coils, but small wires were attached by binding screws, which completed, by its connection with the galvanometer, the electric circuit; this was found to increase the amount of deflection slightly. We now decided by a repetition of experiments, that from all the points, 1, 3, 4, 6, and 7, the electric current was passing from W. to E., but between the point 2 and any of the others the current appeared to take a different direction.

11. The large galvanometer was now fixed at *d*, and the connection made at 7, and with the point at 2, the current was indicated as from E. to W. Between the point 7 and points 8 and 9, the current was evidently from W. to E., as it was also between any other two parts of the two lodes tried at this time. It is not quite easy to explain the very peculiar local disturbance which was found to prevail in close proximity with the cross course at 2. But our experiments lead us to the conclusion, that the current in these lodes is constant from W. to E., the deviation found at 2 being, no doubt, dependent upon some peculiarly local effect.

Being desirous of endeavouring to effect chemical decompositions by the agency of the electric currents flowing through the mineral lodes, we connected two wires with the points 7 and 2—these being chosen from the convenience of their situation. The ends, pointed with platina wire, were placed in a phial which contained a solution of sulphate of copper. This being arranged, was left in the mine for a few days. When the phial was next examined, one of the wires had been removed, no doubt by the curiosity of the miners, but the wire which remained in the phial was sufficiently colored by copper to convince us we were not disappointed in our expectations. How long the circuit may have been perfect cannot be ascertained.

The experiments in East Pool mine have all been several times repeated with the greatest care, but as the results have been invariably the same as those detailed, it is deemed unnecessary to recapitulate them.

Some experiments have been recently made at Pennance Mine, by Mr. R. W. Fox, assisted by Mr. Joshua Fox; the following results, with which Mr. R. W. Fox has kindly favored me, I have, from their importance, with that gentleman's permission, added to those obtained by Mr. Phillips and myself.—R. H.

Pennance Mine is in Killas, two lodes bearing nearly E. and W. (magnetic); they contain much arsenical and iron pyrites,

* Since this experiment was made, a large bunch of ore has been discovered, and worked out behind the point of contact. Whether the mass of ore has any thing to do with the direction of the current remains to be proved. —R. H.

interspersed with oxide of tin and sulphurets of copper and lead.

The more northerly lode is about five feet thick, and dipping a little towards the North; it has been worked sixteen fathoms under the surface. The other lode has not yet been run deeper than eight fathoms, two feet wide, and at present inclined towards the South; but it has not yet been run deep enough to determine the general dip of the lode with certainty.

All the experiments on the north lode gave currents through the apparatus from E. to W., and so, indeed, did the experiments of the south lode, where they were tried, but they were less energetic.

In the east part of the north lode, at the six-fathom level, the current caused the needle to *revolve* quickly, after producing and breaking contact a few times. Contact was made with the one point, by means of blocks and screws, by plates of copper, zinc and platina in succcssion, and also by the points of the wires merely; but without any marked difference in the results under these different circumstances. Nor was there any great difference in the results, where the contact was made with different ores at the same station; thus the positive current was not less energetic, where the contact was made with galena, as an ore point, than with iron pyrites nearly contiguous to it. All these facts show the independent nature of these currents, as it respects the apparatus, or accidental circumstances.

On connecting the back of the north lode immediately under the surface, containing arsenical pyrites, with a portion of the same lode further west, in the six-fathom level, a maximum deflection of about 50° to 55° was produced with copper wire folded up and pressed into a bundle for making contact with the ore points. There was a plate of zinc afterwards substituted for the copper wire, at the surface from whence the positive current was derived, without any diminution of effect; and under these circumstances Mr. Fox was enabled to import magnetism to a horseshoe bar of iron with several coils of copper wire round it, sufficient to act, though feebly, on a compass needle not very delicately poised. The stronger current below would doubtless have produced a much greater effect.

POISONING BY COPPER.*

It is known that wine merchants are in the habit of placing a small copper vessel at the bottom of the cask of wine which is in draught in their cellars, and of drawing the liquid into this vessel, which is not always in a state of perfect cleanliness. This negligence may be productive of fatal consequences. A few months ago, a young man at Avenay was about to mount the Rheims diligence. Before setting out, he asked his father for "a stirrup cup," and the latter took him to a cask of old wine, which was tapped only on great occasions. But scarcely had the young man drunk this glass of wine, when he was seized with violent pains in the digestive organs; he was poisoned. Happily, the quantity of verdigris which he had swallowed was so considerable, and the vomitings which it immediately caused were so violent, that he almost entirely threw up the poison. Milk, which he afterwards drank in abundance, freed him from the rest. He recovered.

Cases of poisoning by copper increase daily. A chemist of high standing in Paris, as well as two members of his family, lately experienced all the symptoms of poisoning, which were imputed to verdigris. Fortunately, timely assistance arrested the symptoms which had been developed.

* *Journal de Chimie Médicale*, Janvier, 1842.

DIURETIC MIXTURE.*

℞ Fresh horseradish root . 10 grammes.

This root is scraped and sliced, then there is added—

Boiling water . . . . 130 grammes.

An infusion is made in a close vessel until it is cooled; then it is strained, and there is added to the colature—

Tincture of cantharides .	8 drops.
Sydenham's laudanum .	12 drops.
Simple syrup	15 grammes.

Mix.

This mixture, which is recommended by Dr. Rayer in cases of dropsy consecutive to chronic albuminous nephritis, should be administered in three equal doses in the course of 24 hours.

The dose of tincture of cantharides may be generally augmented to 36 drops; the dose of laudanum may also be progressively increased to 18 drops; but these augmentations should be advised only with great circumspection, and in all cases it is indispensable to regulate them by the effects produced by the ingestion of the medicine.

ON THE THERAPEUTICAL EMPLOYMENT OF SULPHURET OF IRON.†

BY DR. CAZENAVE.

Dr. Cazenave administered sulphuret of iron in scrophulous diathesis, and par-

* *Journal de Chimie Médicale*, Juillet, 1842.

† *Ibid.*

ticularly in the cutaneous diseases whose existence is connected with that malady. This agent, hitherto too little employed, appears to him to be more advantageous than the iodurettted preparations themselves, especially, because it is less irritating than them, and because it is less likely to cause suppuration of the strumous swellings.

He administered it in the state of suspension in an appropriate syrup, and the following is the formula which he generally adopted :—

℞ Sulphuret of iron . 2 grammes.

Reduce this to an impalpable powder, and when it is sufficiently fine, add

Syrup of saponaria . 125 grammes.

Mix by shaking.

The dose is a spoonful, morning and evening, either pure or in a cupful of bitter infusion.

Before pouring out each spoonful, it is indispensable to shake the bottle well, in order to mix uniformly the whole mass of the liquid with the powder, which, by repose, is precipitated under the form of a blackish deposit.

EFFICACY OF PYROGENOUS ANIMAL OIL IN CONSUMPTION.*

At the report of Dr. Brann, of Fuerth, a physician of Berlin, Dr. Palmedo has obtained remarkable advantages from the employment of Dippel's Animal Oil, in fumigations, in cases of tubercular phthysis. The following is the manner of using this medicine :—Small quantities of pyrogenous animal oil are volatilised in the apartment occupied by the patient, and the fumigation is repeated several times a day, according to the effects obtained, in such a manner as to be able to increase or diminish the dose of the remedy employed.

Dr. Palmedo thinks that this treatment acts by determining a sort of artificial asthma, by favor of which the tuberculous cavities are finally obliterated, their sides drawing together and uniting by an adhesive inflammation.

It is almost needless to add that, on account of the disagreeable odor which characterises Dippel's oil, the air of the apartment should be carefully renewed, when the fumigation has been kept up for a sufficient length of time.

[Much useful information on this subject will also be found in the Appendix to Mr. Hancorn's *Medical Guide for Mothers*, reviewed in No. XXVI. of The Chemist.]

* *Medicinische Correspondenz-Blatt, bayerischer, Aertzte*, 1841.

ANALYSIS OF THE ROOT OF THE RUMEX OBTUSIFOLIUS.*

This root, which is often employed instead of dock (*patience*) root, contains in 1000 parts—

Water	170·00
Resin	3·60
Rumicine	21·65
Sulphur	0·45
Acetate of magnesia	3·50
Extractive matter analogous to turmeric	87·50
Starch	95·50
Chloride of potassium	1·80
Malate of lime and magnesia	5·30
Mucilage	48·00
Phosphate of lime	2·75
Hardened albumen	40·00
Liqueous principle	341·00
Traces of acetate of potassa and lime	
Loss	2·41

POISONING OF THE EAST INDIA COMPANY'S CAMELS BY DIGITALIS, IN CABUL.†

Sometime since a frightful mortality reigned amongst the camels belonging to the East India Company. A committee was appointed to investigate the causes of this mortality. It is supposed that these animals were poisoned by digitalis, a plant which grows abundantly in the Valley of Cabul. The natives, it is said, maliciously mixed this plant with the forage intended for these useful animals.

Montgiardini, who has studied the effects of this plant on animals, says that mammiferous animals are very sensible to its action, and that it is so much the more dangerous to them, as their stomach more resembles that of man.

He is also of opinion, from his experiments, that the use of digitalis increases the motion of the lymph, and diminishes the activity of other systems.

MEDICINAL PHOSPHORIC ACID.‡

Vakenroder in preference prepares medicinal phosphoric acid by a process already known, but in which he has made several modifications, either in the relative proportions of the matters, or in the manipulation.

* *Journal de Chimie Médicale*, Juillet, 1842.

† From the *Agra Akhbar*.

‡ *Journal de Pharmacie et de Chimie*, January, 1842.

He mixed 200 grammes of calcined bones, reduced to a fine powder, with 1300 parts of water: he adds 150 grammes of concentrated sulphuric acid, which he dilutes with 200 grammes of water; after twelve hours' contact without heat, he digests for half an hour at a gentle heat, replacing the water which evaporates. He then filters, and passes into the liquor a current of sulphuretted hydrogen; he again filters and evaporates until there remains only 160 grammes of liquid; he adds to it 320 grammes of alcohol at 48: at the end of twenty-four hours he washes with 30 parts of fresh alcohol the acid phosphate of lime left on the filter. He distils to remove the alcohol, and he evaporates until the liquid weighs only 36 grammes; he dilutes it with water to 120 grammes, which he digests with a little wood charcoal; he filters and concentrates the acid.

Phosphoric acid, thus prepared, also contains, as Mr. Vakenroder has ascertained, a small quantity of acid to phosphate of lime; but it is sufficiently pure for medical use. This chemist thinks that by a new precipitation by alcohol it may be made perfectly pure. It is then only very nearly in a state of purity; it still precipitates a little by oxalate of ammonia.

TINCTURE OF SESQUICHLORIDE OF IRON IN GLEET.*

To the Editor of The Lancet.

Sir,—It is very gratifying to the profession that its members of late so freely contribute their knowledge to the periodicals for the advancement of science, and more especially in those who have the "tactus eruditus" of condensing their information, thus appreciating the valuable time of a practitioner which is consumed in perusing a voluminous article. Perhaps some of your kind correspondents will take a friendly hint.

In former years I have had considerable difficulty in the thorough cure of "*gleet*," being able to put an end to the discharge for a time, as generally the case, to return.

I will now relate "two cases" out of many where I have succeeded in permanently eradicating this troublesome complaint by the tincture of sesquichloride of iron.

John Thompson, aged 24, had gonorrhœa eighteen months ago, and gleet continued since; has been under treatment of a surgeon, who ordered cubebs, copaiva, &c., but as he was careless of consequences did not apply regularly: he had a discharge of thin, white pus when he applied to me. Ordered nitrate of silver and sulphate of zinc injections, alternately to be used, but without effect. I then made use of tincture of iron, gts. xxv, three times a-day, and gradually increased to gts. xl; in a fortnight he was perfectly well; has had no return since.

Wm. Simpson, aged 21, had gleet twelve months; ordered the tincture of iron, dose, gts. xx, two or three times a-day; to be increased to dose gts. xxxv. After seven or eight days the discharge had nearly ceased; in fifteen days he was cured.

If the above is of sufficient consequence to receive a place in your next Number, I beg you will insert it; and should any gentleman have tried this "valuable tincture," perhaps he will have the kindness to contribute his experience in a future Number. I remain, Sirs, yours very obediently,

S. E. R. Jones.

The Naze, June 25, 1842.

* From *The Lancet*.

INJURIOUS EFFECTS OF THE INHALATION OF IODINE IN PHTHISIS.*

To the Editor of The Lancet.

Sir,—Much has lately been said of the good effects of inhaling the vapour of iodine in phthisis. I have frequently witnessed the favourable effects of iodine when administered internally in scrofula. During the last twelve months I have tried the inhaling plan in five cases, the result of which I will briefly state, and shall be glad to hear whether others have witnessed the same bad consequences attending this mode of using iodine.

The first case was certainly not a fair one for a trial, it being phthisis complicated with carcinoma mammæ, and no material benefit was to be expected from any mode of treatment. The vapour of iodine was inhaled in this case every day, for about two months, without any positive effects, good or bad, and the case pursued its usual unfavourable course and end.

The second case was one of simple phthisis, rather far advanced, and the effect of the inhalation in a few days was that of aggravating the cough and difficulty of breathing, and increasing the fever. It was, therefore abandoned, and I think the case terminated sooner than it would if iodine had not been used.

The third case was also one of pure phthisis, not far advanced. After the use of the inhalation for a fortnight every symptom was so much increased, and the distress occasioned each time the iodine was inhaled was so great, that it was discontinued; and I feel convinced that this case also termi-

* From *The Lancet*.

nated much sooner than it would have done under treatment without iodine.

The fourth case—phthisis unconnected with any other disease, and not far advanced, a case which appeared every way favourable for a trial of the plan. The effect of the first inhaling of the iodine was to produce great excitement, coughing, and quickened respiration, succeeded by fainting fits, which for six hours caused much alarm for the immediate safety of the patient. However, by the aid of free air and stimulants, she recovered from the effects of the inhaling, which of course was never repeated.

The fifth case—phthisis *not far advanced*, but accompanied with diseased liver: the patient, however, was well enough, two days previous to the inhaling the iodine, to walk out, and was by no means in a condition to think his death likely to take place for some months. In this case the patient inhaled the vapour of iodine at twelve o'clock, and such was the *poisonous* effect upon him that he immediately became faint, and died in four hours.

Your Journal for the 2nd instant contains a communication from Mr. Robert Jeffs on the subject, and he states that he is now trying the plan upon one hundred patients, and that *if the results be satisfactory*, he will publish the cases. I think it would be much more useful to the public and the profession if Mr. Jeffs were to favour them with a report of *the effects of every case*, whether satisfactory or otherwise, than to confine his report to the "satisfactory" cases. I send you my name and address, but do not wish them published. I am, Sir, your humble servant,

July 16, 1842. G. H.

Our correspondent is a highly respectable surgeon residing in the country.—Ed.

BOOKS RECEIVED.

Animal Chemistry, or Organic Chemistry in its applications to Physiology and Pathology. By Justus Liebig, M. D., Ph. D., F. R. S., M. R. I. A., Professor of Chemistry in the University of Giessen. Edited from the Author's MS., by William Gregory, M. D., F. R. S. G., M. R. I. A., Professor of Medicine and Chemistry in the University and King's College, Aberdeen. London: Taylor and Walton, 1842, pp. 354.

[This highly important work will be noticed in our next.]

The Ninth Annual Report of the Royal Cornwall Polytechnic Society, price 2*s.* 6*d.* Falmouth: J. Tratham, Market Strand. London: Simpkin and Marshall, Stationers' Hall Court; and Weale, 59, High Holborn.

A full report of the proceedings of this useful Society; it contains much valuable and interesting matter, and we earnestly recommend its perusal. An extract from it will be found in this Number.

ERRATA in No. XXXI.

Page 215, 1st column, 32d line, *for* "half," *read*—"twice." Line 39 and 40, *read*—"add moist hydrated peroxide of iron, $2\frac{1}{8}$ parts." 2d column, Tartrate of Peroxide of Iron; 9th and 10th line, *for* "about 45 parts of the oxide," *read*—"about 145 parts of the moist oxide." Page 216, 2nd column Wine of Iron; sixth line, *for* "1836," *read*—"1824." Page 217, 17th line from bottom, *for* "every ounce will contain 16 grains," *read*—"every ounce will contain 24 grains."

ANSWERS TO CORRESPONDENTS.

O. P. Q.—Wash the bone with a dilute solution of chloride of soda, prepared by passing a current of chlorine through caustic soda. This will purify and bleach the bones at the same time. No advantage will accrue from the use of corrosive sublimate.

W. Patte.—Rose's work on Inorganic Analysis, price 18*s.* For organic ultimate analysis, Liebig's work, forming Part 1, of Griffin's Scientific Miscellany, price 2*s.* 6*d.*

Nota Bene.—*All Communications and Books for Review must be addressed, "To the Editors of the Chemist, Care of Mr. Hastings, Publisher, 13, Carey Street, Lincoln's-inn Fields, London." Communications must be prepaid, and sent before the 15th of each Month; Books for Review before the 10th.*

THE CHEMIST.

I. CHEMISTRY.

ON THE PROXIMATE COMPOSITION OF FIBRIN; ON GLUTEN, ALBUMEN AND CASEUM.*

BY. M. BOUCHARDAT.

OF FIBRIN.

M. CHEVREUL demonstrated that fibrin always contains fat: but is this product, when deprived of fatty bodies, a pure proximate principle? It has hitherto been regarded as such. I shall endeavor to prove that this opinion is incorrect, and that the body which chemists designate by the name of fibrin is a complex substance, formed of three distinct proximate principles.

In order to avoid mistake, I shall begin by stating that the fibrin here spoken of is that which is extracted from the blood, either by beating up that fluid, or by repose, which, in certain conditions, occasions the formation of a pseudo-membrane known to pathologists by the name of "cupping," or the *hæmaceulose* of Hotin.

If we separate the "cupping" from the blood of patients laboring under acute pleuro-pneumonia, or, which is still better, under acute articular rheumatism, carefully wash it, in order to free it from globules, and leave it to macerate for twenty-four hours, so as to obtain it perfectly white, taking care to frequently renew the washing water to deprive it of all soluble albuminous matter,—a tough, opaque, and perfectly white membrane is procured.

This product has hitherto been regarded as humid fibrin, containing variable proportions of fat; but the first experiment that I tried evidently established its complex composition.

If this humid pseudo-membrane be boiled with three or four times its weight of water until the liquid is reduced to one half, and if the boiling decoction be strained, there is obtained, by cooling in a suitable manner, either a liquid of a thick consistence, or a perfectly firm jelly. This jelly may be again liquefied by a gentle heat. If nitric acid be cautiously poured into this liquid, no precipitate is formed, or, at most, there is a scarcely perceptible turbidness; if, on the contrary, a solution of bichloride of mercury or tannin be added, an extremely abundant flocculent precipitate is perceived; if chlorine be driven into it, a plentiful formation of flocks is likewise observed.

These experiments show that the fibrin of the blood contains gelatine.

I endeavored, by numerous experiments, to determine the proportion in which the gelatine exists in fibrin as compared with the other principles, but was unable to do so with accuracy, because this proportion is extremely variable: in the normal state, in the fibrin of the blood of a healthy man, it is sometimes difficult to clearly establish the presence of gelatine; but in inflammatory affections of the *serous* (?) or of the cellular tissue, the amount is very considerable. It likewise will readily be perceived that the proportions of these organic elements must ever vary according to the conditions of nourishment or disease.

The presence of a considerable quantity of gelatine in the blood of persons suffering from acute inflammatory affections of the serous, &c. (?) or of the cellular tissue, is a fact worthy of the attention of pathologists.

Besides gelatine, fibrin freed from fat contains two other substances. This important fact may be established by acting with very dilute acids on humid fibrin. I will repeat in detail an experiment which is described in my investigations concerning digestion.

Add to water $\frac{1}{200}$ of hydrochloric acid; at this degree of dilution, the acidity is scarcely perceptible to the taste, and its action on

* *Comptes Rendus*, No. 25, June 20, 1842.

litmus paper is very weak. If $\frac{1}{10}$ of humid fibrin be put into this water, it immediately swells up, and is converted into a mass of extremely voluminous gelatinous flocks; by a prolonged maceration the turgid vesiculæ are torn, the greater part of the fibrin is dissolved, but there always remains a very manifest proportion of a product which is not attacked by an excess of the solvent, and which, besides fat, is formed of a substance which appeared to me identical with the substance which forms the basis either of the epidermis or of the corneous or hairy structure.

The proportion of this undissolved substance, for which I propose the name of *epidermose*, is very small; it is difficult to determine it accurately, for it can be separated from the dissolved substance only by filtration, and this operation being performed with an essentially variable viscous liquid, it is only with great trouble that we can arrive at a quantitative determination.

I shall return to the properties of epidermose.

We will now consider the dissolved matter, which constitutes the most important part of crude fibrin.

The portion of fibrin which water so slightly acidulated dissolves with such facility, is a matter worthy of attention, for it forms, as will be proved further on, the essential constituent part of the most diffused nitrogenous matters, such as the albumen of the egg, of the blood of the dropsical fluid, and of caseum; indeed the predominant part of the crude gluten of cereals.

The acid solution of fibrin scarcely reddens litmus paper; it is abundantly precipitated by an excess of hydrochloric, nitric acid, &c.; a great excess of acid dissolves the precipitate formed; it is rendered turbid by heat, and thus furnishes light flocks; but the whole of the matter is not thus precipitated: the greater part remains, after the liquid has been carefully evaporated, under the form of fine, transparent, flexible and slightly colored pellicles.

The acid solution of fibrin abundantly precipitates by the addition of bichloride of mercury, prussiate of potassa, and tannin.

Examined with M. Biot's apparatus, it caused the rays of polarised light to deviate to the left; the index of the rotation is, however, inconsiderable, even in a very long tube, for the solutions contain only small quantities of the substance. As the soluble principle of the fibrin which is not gelatine is identical with the predominating matter of the albumen of the egg, I propose for this pure substance the name of *albuminose*.

Does the acid solution of fibrin contain only albuminose? Evidently not: indeed, we have seen that crude fibrin contains gelatine, and the following experiment establishes the solubility of gelatine in the same acid solvent.

If we take isinglass in *laminæ*, or the membranes forming the swimming bladder of the *Acipenser Huso;* if this dry membrane be placed in water, acidulated with $\frac{1}{2000}$ of hydrochloric acid, the whole being brought to the temperature of about 20° C.; the membrane swells up, becomes divided, and the greater part is dissolved. If it be filtered after 12 hours' action, a solution is obtained, which, in cooling a few degrees, becomes a perfectly transparent, consistent jelly, with a very slight acid taste.

This experiment demonstrates the existence of two principles in crude fibrin, which may be dissolved in very slightly acidulated water—gelatine and albuminose; the acid solution of fibrin contains these two principles.

This experiment likewise proves the pre-existence of gelatine in animal tissues; at any rate it cannot be admitted that water, containing so small a quantity of acid as not to redden litmus paper, nor to be perceptible to the taste, is sufficient for operating at the ordinary temperature the transformation hitherto supposed to take place.

ACTION OF VARIOUS DILUTE ACIDS ON FIBRIN.

Extremely dilute hydrochloric acid is not the only acid which exerts a solvent action on fibrin. We have tried comparatively solutions containing $\frac{1}{2000}$ of lactic, acetic, sulphuric, nitric, phosphoric and hydrochloric acids, and in all of these, the fibrin swelled up and partially dissolved; but the solvent action is more rapid and more complete in the hydrochloric than in the other acid solutions: it is not the less established that it is not a special action belonging to extremely dilute hydrochloric acid; it extends also to other acids of the same degree of dilution, and even to those which precipitate albuminous solutions, forming with them insoluble compounds.

GLUTEN.

If gluten extracted from wheat be placed in water, containing one or two thousandths of hydrochloric acid it becomes divided, and gradually dissolves, and there is obtained by filtration a limpid liquor, which acts exactly like the acid solution of albuminose. It becomes turbid by ebullition; it precipitates by hydrochloric, nitric, and other acids: the precipitate redissolves in a great excess of the same acids. It is precipitated by bichloride of mercury, by prus-

siate of potassa, and by all reagents which precipitate albumen.

The rotatory power of this substance is exactly the same as that of the albuminose of fibrin; it deviates in the same direction; the intensity of the deviation is much more marked; the solution may, indeed, contain a much more considerable ponderal proportion of active substance, for we do not find in crude gluten the substance which I have called epidermose, which confines the albuminose in the pores of the fibrin, and which prevents us from obtaining concentrated solutions of albuminose.

ALBUMEN OF THE BLOOD AND OF EGGS.

If we add to the serum of the blood, water containing one or two thousandths of hydrochloric acid (in quantity sufficient for maintaining a slight excess of acid), a solution is obtained which acts absolutely, like the acid solution of the albuminose of fibrin, exerting on polarised light the same deviation to the left, as M. Biot has already observed. If the albumen of the egg be similarly treated, membranes are separated which are not attacked by the solvent, and there is dissolved a matter which we have studied under the name of albuminose, or pure albuminous matter.

CASEUM.

Curdled milk, which had been kept for twenty-four hours, was taken, and the cream was carefully separated; the caseum was laid on a filter, and well washed with cold water; while moist, water containing $\frac{1}{2000}$ of hydrochloric acid was added to it. At the end of a few hours, all the caseum was dissolved, and there remained only a few traces of fatty matter, which rendered the liquor turbid, which was filtered. A very slightly acid limpid liquid was obtained, which presented all the characters of the solution of albuminose: it precipitated with hydrochloric and nitric acids; the precipitates redissolved in the concentrated acids; it likewise precipitated by bichloride of mercury and by ferrocyanuret of potassium. I also examined the rotatory power of this liquid, and I found that it caused the rays of polarised light to deviate to the left.

CONCLUSIONS.

The following are the two most important consequences that flow from the above facts:—

Fibrin free from fatty matters is composed of three proximate principles, in variable proportions: a matter identical with pure albumen, uncoagulated, for which I propose the name of *albuminose*, and which is confined in the reticulation of a tissue, composed of *gelatine*, and of a principle presenting all the properties of the epidermic formation, for which, on that account, I have proposed the name of *epidermose*.

I will not here insist on the consequences which flow from this complex composition of fibrin; I will content myself with remarking that there are two fundamental principles of the tissues of animals, gelatine and epidermose, of which the existence was not admitted in the blood, and which are found in that liquid.

The fundamental principle which is found in the albumen of the egg, in the serum of the blood, in the gluten of cereals, and in the caseum of milk, is always identical; it is albuminose, mixed or combined, either with earthy matters, phosphate of lime and magnesia, with alkaline salts, or with fatty matters, which disguise its real properties. If, by a really inappreciable quantity of acid, this ephemeral combination be destroyed, the solution of albuminose is then presented with identical properties, exactly similar chemical reactions, an action on polarised light exerted in the same direction, an uniform deviation to the left, and whose energy is, *cæteris paribus*, proportional to the ponderal quantity of substance dissolved.

ON THE INSTINCT AND IRRITABILITY OF PLANTS.

BY JOHN S. HILEY, A.B., M.B.

(Concluded from No. XXXI.)

ANXIOUS, however, as Nature may seem in forwarding the fecundation of plants, she has the power, when the roots are luxuriantly prolific, of restraining, in some degree, their productive qualities as to seed. Thus she may, and often does, render a certain number of stamens abortive. This has been noticed by Linnæus, in the Plantain tree. Here Nature, by relaxing, as it were, from her usual solicitude, allows the plants to repose.

On the other hand, when the species begin to fail, she at once takes the alarm; all her energies are exerted to secure the future progeny, even at the hazard of the parent stock, and to send them abroad to colonise more favourable situations. Another fact evinces the wonderful care of Nature. Thus, in place of trusting the access of the pollen to any accidental modes of conveyance, as the wind, butterflies, insects, or other elaborate and ingenious methods, the stamens and pistils, in the majority of cases, are lodged together in the same flower, with the same guard of calyx and corolla encircling them.

Such provision, it is evident, was very desirable in specimens whose flowers were

synchronical with the leaves, because the latter, in cases where the stamens and pistils grew on separate flowers, or on separate trees, would have offered considerable obstacles to the passage of the pollen, from the male to the female. This was particularly needful in warm climates. In the trees of cold regions, however, it was not so necessary, because many blossom before the leaves come forth, and in the windy season of the year, so that the pollen can readily pass from one flower to another, without being opposed by the leaves.

Again, Oaks, &c., which blossom later, are either very much frequented by insects, or, like Firs, have leaves so very little in the way, and pollen so abundant, that impregnation can scarcely fail.

Beautiful and extraordinary as these conditions may appear, there are others not less remarkable, and worthy of our notice. Thus the pollen and stigma are always perfected at the same time. In the Viola tricolor, the Gratiola, the Martynia, and many others beside, the style has been observed furnished with a stigma gaping only at the time the pollen is ripe. Linnæus, with that correctness peculiar to him, has described the Jacobæan Lily, (Amaryllis formosissima), as provided with a drop of clear liquid, which protrudes every morning from the stigma, and at noon seems ready to fall to the ground. It is, however, reabsorbed in the afternoon, having received the pollen, whose vapour renders it turbid, and whose minute husks afterwards remain upon the stigma. This phenomenon takes place for several successive days. In the umbelliferous tribes, the style does not appear frequently until the anthers are fallen, but in such instances, the stigma is previously perfected, and the style shoots out afterwards in a curved form, for the purpose of providing those hooks to the seeds, by which they afterwards fix themselves to the soil in hedge-bottoms, and elsewhere. That, however, the several organs in this family are sometimes brought to perfection in different flowers, at variable times, cannot be denied, but here it happens in order that the anthers of one may impregnate the stigmas of another, whose stamens were abortive, or long since withered.

The same phenomenon is seen in other instances, as in the Iatropha urens mentioned by Linnæus. Here the stamens ripen some weeks before the pistils; consequently in a wild state, when impregnation should take place, some or other of the two kinds of blossom must be ripe together on different trees.

Again, the proportion and situation of stamens with respect to pistils furnish other instances of an instinctive provision in the works of Nature. Thus, that the pollen may pass more readily from one to the other, the stamens are shortest in drooping flowers, and longest in those which stand erect. In either case the pollen is carried by gravity to the pistil. With a similar design in view, the barren blossoms stand above the fertile ones in Carex, Coix, Arum, &c., that the pollen may fall on the stigmas. This is the more remarkable, as the usual order of nature seems in such plants, as well, indeed, as in compound and even umbelliferous flowers, to be reversed; for in simple flowers the pistils are central or internal, and, therefore, would, if drawn out into a monœcious spike, be above the stamens.

The contrivances by which the anthers and stigmas are brought together when within the same floral integuments, are also very curious. The case of the Barberry has already been alluded to. But there are other examples in which the operation of any external agency like the touch, is not requisite. In some of these the motions occur independent of any known action, while in others they are occasioned by the presence of damp, dew, or rain. Hence the benefit which results to some blossoms from a visitation of rain, &c., in causing their fertilisation, whilst others are injured by it, especially if it happen to come before the flowers have reached a sufficiently advanced stage. Moisture often causes the bags of pollen to burst and discharge their contents, and hence dew may, like rain, be injurious or otherwise, according to the state of the blossom.

I will now furnish a few examples of the other contrivances just alluded to. In the Gloriosa, the style is bent at a right angle from the base, evidently that the pollen from the stamen may be shed upon it. In Saxifraga and Parnassia, the stamens lean one or two at a time over the stigma, retiring after they have shed their pollen, and giving place to others. This economy is very remarkable in the Ruta graveolens, whose stout and firm filaments thus evince a spontaneous movement, unaffected by external causes, for they cannot be disturbed from the posture in which they may happen to be. The filaments of the Celosia, (Cock's-comb), are held together by a membranous web at their lower extremity. In moist weather this web becomes relaxed, allowing the stamens to spread and shelter themselves under the concave sepals. On the contrary, when the air is dry, this membrane contracting, brings them together, thus enabling them to scatter their pollen in the centre of the flower. The filaments of the Kalmiæ

are restrained by the minute pouches in the corolla, as are also those of the Parietaria by the calyx. These when ripe relieve themselves by an elastic spring, which, in both instances, strange to say, serves to dash the pollen with considerable force upon the stigma. The Medicago falcata is endowed with some such peculiarity. Thus the growing germen liberates itself with a spring from the keel, by which the pollen is dashed about the stigma by the time the rudiments of the seeds are perfected. It has been already stated, that moisture operates injuriously on many productions, in causing their pollen to explode.

Hence the evils liable to arise from visitations of rain, or heavy dews, in certain stages of their growth, to Rye, Barley, Junipers, &c. To guard against the hurtful influence of nocturnal dews and drenching rains, a beautiful defence has been afforded to plants. Thus, most flowers either fold their petals together, or hang down their heads at such seasons, by which their delicate internal organs are protected. This property is very remarkable in Snowdrops, the Fritillary, the Crown Imperial, several of the Campanulas, and others, and it is worthy of note, that whilst the corolla thus keeps off the rain, the approach of atmospheric air is in no way impeded, so that, when ripe, the pollen may be easily wafted from the anthers to the stigma. It is singular, that the fruit, which is heavier than the flower, does not droop, but stands erect, at once proving that the position of the flower cannot result from its weight. Papilionaceous flowers usually close in dark, gloomy, wet weather. All flowers, however, be their positions what they may, are so contrived, as to enable the air to approach them for the purpose of blowing the pollen from one blossom to another, or from the stamen to the pistil. This fact is in no case better exemplified than in the economy of various aquatic plants. Several instances are furnished in the catalogue of specimens of this kind, rising from and sinking into the water, according as air is likely to be beneficial to them or not; and thus it happens, that they most commonly erect themselves from the water at the flowering season, when the sun assists in bringing about impregnation. Amongst plants gifted with this extraordinary provision, I may mention the Utricularia vulgaris, the Potamogeton densum, Nymphæa alba, Stratiotes Aloides, and the Valisneria spiralis, a native of ditches in Italy. The economy of all the preceding specimens has been entered into pretty largely in the catalogue, with the exception of the Valisneria spiralis. And though this Italian production was fully described before, it will be advisable to repeat our observations, in order that the reader may form some idea of the economy of all aquatic plants. He will from hence be induced to credit many of the stories which have been told by Pliny, and Theophrastus, respecting the Egyptian Lotus, and which the observations of modern botanists have proved to be correct. In the Valisneria spiralis, as described by Micheli, the fertile flowers stand on long spiral stalks, which by uncoiling raise them to the surface of the water, where they become exposed to the air. In the mean time, great numbers of barren or male flowers arise from a distinct root, or short straight stalks, from which they shoot like little separate white bubbles, suddenly expanding when they reach the surface, and floating about in such abundance as to cover it entirely. In this way their pollen is scattered over the stigmas of the first mentioned blossoms, whose stalks soon afterwards resume their spiral figure, and the fruit comes to maturity at the bottom of the water. With but slight shades of difference, this is the kind of economy observed by most of the aquatic plants mentioned above; and if one provision more than another can move our wonder, and lead us to admire the Creator, in the works of the creation, it is surely this. Indeed, we may say with a heathen philosopher, "Here is the finger of God."

It would seem that aquatic plants rise out of the water for the sole purpose of enabling the air to waft the pollen from the anthers to the stigmas. Such being the fact, we may here introduce satisfactorily a few observations as to the other methods which Nature has had recourse to for accomplishing this necessary end. Some allusion has been made to them already, for I have mentioned several cases of irritability which occasioned the passage of the pollen to the stigma.

I will now say a few words as to the manner in which insects effect this purpose. These tiny creatures are particularly busy about flowers in sunny weather in search of honey. Whilst engaged in this occupation, they are the insensible media for conveying the golden dust on their sides and legs from one part of a flower to another, thus effecting the designed fertilisation. The Fig and the real Sycamore are much indebted to a minute fly of the genus Cynips for conveying the pollen from barren to fertile flowers. The Tipula pennicornis, a curious insect, accomplished the same for the Aristolochia Clematitis, according to Schrœder and Willdenow. The Tipula probably performs the same part for the Aristolochia Sipho of America; and in all likelihood, it is in con-

sequence of the want of this insect that the same specimen rarely forms fruit in the gardens of this country. Having thus entered pretty minutely into the instinct and irritability of the calyx and corolla, and of the stamens and pistils of different flowers, I must now remark on any similar economy which may have been witnessed in the seed and its capsule. These, though not coming so repeatedly under our observation, are not less striking. The instinct of aquatic plants leads them to sink under water after impregnation is effected, to deposit their ripened seeds in the mud, where they are to grow. Such is observable in the Utricularia vulgaris, the Potamogeton densum, Ruppia maritima, Nymphæa alba, Stratiotes Aloides, and Valisneria spiralis. It cannot be denied that in the majority of instances, I speak now of such plants as grow in dry soil, the seeds fall immediately around the parent plant; and hence it is, that the localities of certain species remain the same for ages, spreading no faster than the seeds. Other seeds and seed vessels are furnished with the means of being transported by the influence of the wind, or by some other cause, to a considerable distance, and hence the localities of such species are very varied, the seeds sowing themselves in any chance situation, or at least where the soil holds out a possibility of their flourishing. Amongst these may be classed several of the Umbelliferæ, and the Compositæ, as the Valerian, the Dandelion, Thistle, and others.

The fruits of the Ash and the Sycamore are likewise provided with wing-like appendages, which are manifestly intended to assist in the dissemination of the seed. Fleshy fruits either fall to the ground close to the parent plant, or their dispersion is promoted by different animals and birds, whose food they may happen to be. Such become instrumental in the work, by again voiding the seed in a better and fitter state for germination.

In capsular fruits, most of which are many-seeded, the dispersion is commonly effected by the wind, which shakes a few at the time from the capsule. They may be retained for a longer or shorter period, according as dryness may be necessary or not, and they are provided for accordingly. Some capsular fruits are so formed, that they can project their seeds to a distance, by the elastic force with which their valves burst open when ripe; the Impatiens Noli-me-tangere is in the number. The effect is accelerated by a breath of wind or a touch of the finger, &c. The genus Oxalis is likewise similarly situated. The elastic arillus, which covers the seeds, suddenly bursts on the slightest touch or motion after the capsules have opened, and turning the inside outwards, projects the seed to a considerable distance. The legumes of the Ulex Europæus burst elastically in a dry hot weather and scatter their seeds extensively. The same may be said of the Digitalis purpurea, whose seeds may be heard in autumn crackling in hedges. Moisture opens the valves of some seed vessels; this is the case with the Onagrariæ, which flourish in wet places, and in this way their seeds are scattered. According to Drummond, "several species of the Fig-marigold in the South of Africa, when put into water gradually open their seed-vessels, and as they dry again, as gradually close them, so as to lock up the seeds. This singular provision of opening when wet, and closing when dry, cannot arise from chance, for the latter has no share in the economy of nature, whose operations are all the results of the wisest design. And here the final cause is very apparent. These plants grow in the most arid and burning plains; and were their seeds not protected from the excessive heat by being covered, most of them would probably perish; therefore, during the heats they are locked up, but when the rainy season commences, the seed-vessels open, the seeds are discharged, and immediately commence growing in the moistened soil. Had they escaped sooner, they must during the long dry season have been tossed about at the mercy of the winds; their numbers have been dispersed to unsuitable situations, and consequently the parched tracts which they now clothe and beautify, would continue comparatively naked." The Rose of Jericho, by certain singular contrivances, is conveyed to places where it can germinate. Nature, however, instead of trusting to the mechanism of the seeds or seed-vessels for the migration of plants, contrives that the mother-plant herself shall migrate, and bear along with her the seeds, her offspring. Thus we learn from Pallas, that most of the plants of the Russian steppes are so organised that when arrived at full maturity they become very light, and breaking off near the root are carried away by the breeze. Some of these also are assisted in their migration by growing into the form of a globular bush, which rolls under the slightest impulse of the wind. Some plants, as the Antirrhinum, Cymbalaria, Cyclamen, &c., the Trifolium subterraneum, and certain species of the Viola, particularly the Viola hirta, have the property of elongating their flower-stalks, and of burying the ripened capsule, &c., in the soil, when the seeds commence to germinate. The power which the seeds have of retaining their vitality under peculiar circumstances for an unlimited length of time,

is too well known to need any comment. The irritability of plants, so often alluded to in the foregoing observations, would seem to be perfectly analogous to that of animals, though far inferior in degree. The correctness of this view is sufficiently established by the exact nature of the action of mineral and vegetable poisons, as instituted by Marcet, Macaire, Turner, and Christison, upon animals and plants. These experimentalists arrived at the conclusion that metallic poisons act upon vegetables nearly as they do upon animals; and that vegetable poisons, especially those which destroy animals by their influence upon the nervous system, also cause the death of plants. The sleep of plants,—the opening and shutting of flowers under peculiar influences, as light, shade, rain, &c.—the singular motions which have been occasionally noticed, as for example, that contractility instanced in the Spirting Cucumber (Lactuca virosa), and in certain seed-vessels, and in the stamens and pistils, &c., of the Barberry, the genus Stylidium, Kalmia, and Vandeæ, when touched or otherwise irritated, would all appear to be mere modifications of one and the same power. I may now conclude this long Memoir by subjoining the catalogue of indigenous plants, gifted with anything like instinct or irritability, and to which I have so repeatedly alluded. In their arrangement, I have followed the Linnæan classification, since it offers many advantages not met with in other methods.

DIANDRIA MONOGYNIA.

Veronica Chamædrys.—The flowers expand in fine weather only.

Utricularia Vulgaris.—The leaves bear numerous crested bladders, which float the plant at the following season by means of air formed within them. According to some botanists, this air gives place subsequently to water, and the plant sinks to ripen its seeds at the bottom of the ditch or pool in which it may be growing.

TETRAND. MONOG.

Centunculus Minimus.—Expanding its flowers in bright sunshine only.

TETRAND. TETRAG.

Potamogeton Densum.—The head is just above water during impregnation; after which, by the increase of the branches, it sinks and ripens its seeds, whilst other flowers come forth above.

Potamogeton Fluitans.

Potamogeton Lucens, &c.

Ruppia Maritima.—Flower inclosed within the sheath of the neighbouring leaf. After flowering, the flower-stalk, often spiral, is greatly lengthened, rising to the surface of the water, and carrying with it the four impregnated germens, each raised on its own long and firm stalk. In this respect it is the reverse of Potamogeton.

PENTAND. MONOG.

Cynoglossum Officinale.—When touched, a pungent and nauseous scent, like that of mice, or, as some say, the urine of dogs, is emitted.

Anagallis Arvensis.—Flowers close on the approach of rain.

Convolvulus Arvensis.—Flowers close before rain.

Convolvulus Soldanella.—Flowers expand in the sunshine only.

Impatiens Noli-me-tangere. — Capsule, when nearly ripe, elastic, bursting asunder with the slightest touch, and scattering its seeds. According to Villar, the leaves, in Dauphiny, hang down in a flaccid state during the night.

Verbascum Pulverulentum.—When the stem is smartly struck three or four times with a stick, all the flowers then open will in a few minutes throw off their corollas, the calyx closing round the germen, so that after eight or ten minutes none will remain on the plant.

Erythræa Centaurium.—Expanding only in sunshine, and closing as soon as gathered.

Vinca Major.—Stem ascending while in flower, afterwards procumbent.

PENTAND. DIGYN.

Cuscuta Europæa.—A parasitical herb, adhering to, and obtaining nourishment from, other plants, after its own root is obliterated.

PENTAND. HEXAG.

Drosera Rotundifolia.—The whole disk of the leaf, but especially its margin, is beset with red inflexed hairs, discharging from their ends a drop of viscid acrid fluid. These hairs have been thought irritable so as to contract when touched, imprisoning insects somewhat in the manner of the American Dionæa Muscipula, a plant allied to Drosera.

HEXAND. MONOG.

Berberis Vulgaris.—The stamens have been found irritable in one small spot near the base, on the inner side only. They contract suddenly when touched here with the print of a pin or penknife, like the muscles of animals, and thus throw the pollen on the stigma.

OCTAND. MONOG.

Œnothera Biennis.—Flowers expand in an evening.

Chlora Perfoliata.—Flowers open in sunshine only.

OCTAND. TRIGYN.

Polygonum Convolvulus.—Stem climbing from left to right.

DECAND. PENTAG.

Arenaria Rubra.—Flowers open in bright weather only.

DECAND. PENTAG.

Oxalis Acetosella.—Leaflets drooping at night. When ripe, the seeds are projected to a distance, on the slightest touch or motion, by their elastic tunics.

POLYAND. MONOG.

Nymphæa Alba.—The flowers of this beautiful plant expand in sunshine and the middle of the day only, closing towards evening, when they recline on the surface of the water, or sink beneath it. The berry gradually decays at the bottom of the water, scattering its seeds in the mud. The sinking of the flowers under water at night, having been doubted, or at least denied, the circumstance was verified by the late Sir J. Smith. The same is recorded by Theophrastus and Pliny of the Egyptian N. Lotus from the most remote antiquity. The stimulus of light, which indeed acts evidently on many other blossoms and leaves, expands and raises with peculiar force, these splendid white flowers, that the pollen may reach the stigma uninjured; and when that stimulus ceases to act, they close again, drooping by their own weight to a certain depth. The still more ponderous fruit finally sinks to the bottom.

POLYAND. MONOG.

Cistus Guttatus.—Flowers expand very early in the morning, and fall in four and five hours.

Cistus Helianthemum.—Yellow flowers expanding in sunshine only, when their stamens, if touched, spread slowly, and lie down upon the petals.

POLYAND. PENTAG.

Stratiotes Aloides.—The parent plant sinks to the bottom of the water after flowering, and sends out long simple runners, each terminating in a leaf bud, or young plant, which first takes root in the mud, and in the following summer rises to the surface of the water, blossoms, and then again subsides to ripen its seeds, and throw out fresh runners.

DIADELPH. DECAND.

Ulex Europæus.—Legumes of this shrub burst elastically in dry hot weather, with a crackling noise, and scatter their seeds extensively.

Medicago Falcata.—The growing germen liberates itself with a spring from the keel, by which the pollen is dashed about the stigma by the time the rudiments of the seeds are perfected.

SYNGEN. POLYG. ÆQUAL.

Sonchus Oleraceus.—Flowers close at night, and in bad weather.

Lactuca Virosa.—The juice springs out suddenly in large drops on the slightest touch, from the calyx and tender leaves, evincing a considerable degree of irritability in the plant.

Apargia Hispida.—Flowers drooping in bud, erect when expanded.

Tragopogon Pratensis.—Flowers opening early in the morning, and closing before noon, except in very cloudy weather. It is hence called *John go to bed at noon.*

Hieracium Subaudum.—Flowers open in the forenoon only.

Crepis Pulchra.—Flowers closing about noon.

Hypochæris Maculata.—Flowers closing in the forenoon.

Hypochæris Glabra.—Flowers open in the morning only.

Carlina Vulgaris.—Flowers close before rain. The scales of the calyx are hygrometrical, changing their position according to the moisture of the atmosphere.

SYNGEN. POLYG. SUPERF.

Senecia Jacobæa.—Flowers facing the sun.

Bellis Perennis.—Most open in bright weather.

Matricaria Chamomilla.—White rays reflexed at night.

MONŒCIA TETRAND.

Urtica Dioica, &c.—Herbage in all our species copiously armed with venomous perforated bristles, each of which, by a slight pressure of the hand, is transmitted into the skin, causing great irritation. Most of the exotic species have not this stinging property.

MONŒCIA POLYAND.

Arum Maculatum.—Reported to give out at the time of its perfection a considerable degree of heat.

ON THE COMPOSITION OF PHOSPHORIC ACID AND THE PHOSPHATES.*

BY M. LONGCHAMP.

It is here my object to show that the composition of phosphoric acid is PO^2, and not PO^5, as generally admitted; and to establish that in the phosphates the oxygen of the acid is only twice that of the base, and not twice and a half.

* *Comptes Rendus*, No. 2, July, 1842.

The phosphates are salts of too important a nature for this question not to attract attention; but, moreover, chemists will readily conceive that phosphates alone are not treated of, for the composition of this kind of salts necessarily includes that of all those which are formed by an acid expressed by $R^2 O^5$: it is therefore, with the exception of the sulphates and the carbonates, almost all the principal kinds of salts whose composition is here discussed.

In his old tables, as well as in the new, Berzelius gives the following proportions for crystallised phosphate of soda:—

Soda	17·88
Phosphoric acid . . .	20·40
Water	61·72
	100·00

In the third volume of his *Traité de Chimie*, M. Berzelius mentions a result obtained by Mr. Clarke, which, says he, was entirely unexpected, and from which it results that phosphate of soda does not contain 61·72 of water, but 64·15. In my work on the *Analysis of Phosphoric Acid and the Phosphates*, read to the Academy of Sciences on the 30th of June, 1833, I gave 64·00.

Thus it can now no longer be doubted that the phosphate of soda contains a larger proportion of water than is indicated in Berzelius's tables. Indeed, from this single fact, it may be concluded that the admitted composition of the phosphates is incorrect; for instead of having 38·28 of anhydrous salts in 100 of crystallised salt, we have but 36·00, that is to say, a difference of 2·28. By carrying this difference to the account of the base (17·88), we should have a phosphate composed of:—

Soda . . . 43·34 oxygen 11·09
Phosphoric acid 56·66 oxygen 31·75;

and the oxygen of the base is to that of the acid as 11 to 32 or 1 to 3.

If the difference be carried to the account of the acid (20·40), we have a phosphate composed of—

Soda . . . 49·66 oxygen 12·70
Phosphoric acid 50·34 oxygen 28·21;

and the oxygen of the acid is to that of the base as 12·7 to 28·2, instead of 12·7 to 31·75, as the proportion of 1 to 2·5 would require. Thus, from this single fact that crystallised phosphate of soda contains 64·00 per cent. of water, instead of 61·72, it must be concluded that, in the phosphates, the oxygen of the acid is not two and a half times that of the base; and this is the conclusion to which I came in 1823.

In order to ascertain whether this difference of 2·28 should be referred to the acid or to the base, I analysed crystallised phosphate of soda by means of nitrate of baryta and chloride of barium.

Wishing to control this analysis by that of the phosphate of baryta obtained, I dissolved one part of this phosphate in nitric acid and another in hydrochloric acid, and these two solutions, precipitated by sulphuric acid, gave a weight of sulphate from which I concluded that phosphate of baryta is composed of—

Baryta . . . . . 69·37
Phosphoric acid . . . 30·63;

which showed me that 100 of crystallised phosphate of soda contain 18·78 of acid. I should have obtained only 18·12 (20·40—28=18·12), but probably there remained a little baryta dissolved either by the phosphoric acid set free, or by the acid (nitric or hydrochloric) used for dissolving the phosphate. Be it as it may, it is evident that the loss of 2·28 relates to the acid, and not to the base, and that anhydrous phosphate of soda is composed of—

Soda . . . . . 49·67
Phosphoric acid . . . 50·33.

About the time that I obtained these results (1823), I adopted the law which I have since presented, from which it results that *between two elements of contrary nature, there can be only three combinations*:—

A^2B, AB and AB^2,—

A, designating the electro-positive, and B, the electro-negative, element.

My first care was to submit my results relative to the phosphates to this law, and I found that they received from it a perfectly satisfactory explanation: I admitted, then, two atoms of oxygen in phosphoric acid, instead of two atoms and a half. As the 49·67 of soda contained in 100 of phosphate represent two atoms of soda, whose atomic weight is 781·79, the 50·33 of acid will represent an atomic weight of 792·18, which should be that of two atoms of phosphoric acid. By subtracting, therefore, 400 from 792·18, there remains 392·18 for the double atom of phosphorus, and 196·09 for the single atom. Berzelius gives 196·14. Since 792·18 of acid contain 400 of oxygen, 50·33 contain 25·41, and the composition of the phosphate of soda will be—

Soda . . . . . 49·67 oxygen 12·70
Phosphoric acid 50·33 oxygen 25·41.

We cannot find by experiment a more mathematically correct relation, and we shall see that the composition of phosphate of baryta also gives it in a high degree. 6 grammes of crystallised phosphate of soda containing, according to the above analysis, 1·087 of acid, produced 3·7 of phosphate of baryta: this salt is therefore composed of—

Baryta . . . 2·613 70·62 oxygen . 7·38
Phosp. acid 1·087 29·38 oxygen . 14·84.

From these results it is evident that in the phosphates the oxygen of the acid is twice that of the base; hence, we must admit P^2O^4, instead of P^2O^5.

But why P^2O^4, instead of PO^2? We shall see by the composition of the hydrated phosphoric acid, and it will at the same time be perceived, that this composition, which I give, explains the different properties of these acids, which, in the present state of the science, could not be explained; for, since, according to chemists, phosphoric acid heated to redness is not changed in its nature, why does it form a white precipitate with the salts of silver which the acid that has not been heated precipitates of a yellow color? Why does the acid heated to redness precipitate albumen which, on the contrary, the acid that has not been heated dissolves? My theory explains all this, for the heated acid is changed in its nature, as will be seen.

$2(PO^2)$ is the acid arising from the vivid combustion of phosphorus in dry oxygen; it is also that which arises from the phosphate of ammonia heated to a white heat for an hour. In the latter case, it is vitreous, and dissolves in water only after a long contact with that liquid; but then, during this contact, its nature becomes altered. When $2(PO^2)$ are combined with $2(H^2O)$, we have—

$$(c) \qquad (PO^2 H^2O^2) + (PO, H^2O).$$

It is evident that one of the two atoms of $2(PO^2)$ has but an atom of oxygen, which is combined with an atom of water, forming peroxide of hydrogen (oxygenated water). That is the reason why two atoms of phosphoric acid enter into the composition of the phosphates. If we heat to 215° C., there remains $(PO^2, H^2O^2) + PO$ (b), and H^2O is disengaged from the second number of the formula (c). If this new acid be heated to redness, H^2O of the first number is removed, and the second O of the H^2O joins the OP of the second number of the formula (b), in order to form again the primitive acid $2(PO^2)$. We have then:—

(a) $2(PO^2)$ . . . . . metaphosphoric acid;
(b) $(PO^2, H^2O^2) + (PO)$ . . . . . pyrophosphoric acid of the acid pyrophosphates;
(c) $(PO^2, H^2O^2) + (PO, H^2O)$ . . pyrophosphoric acid of the neutral pyrophosphates;
(d) $(PO^2, H^2O^2) + (PO, H^2O) + H^2O$ acid of the new phosphates discovered by Graham.

I conceive that these formula are opposed to the opinions of many chemists; but whether they are correct or not, whether the doctrine of the hydrogenic acids from which they are deduced be admitted or rejected, it is no less true that, apart from every theoretical idea, the balance proves to us that phosphoric acid is not formed of P^2O^5, but of PO^2, and that in the phosphates the oxygen of the acid is not two and a half that of the base, but only twice.

It will be perceived, without my saying any more on this subject, that the numbers given by the tables are no longer accurate, and that the composition of any phosphate will be obtained by combining a double atom of phosphoric acid = 792·18, with two atoms of base.

NEW METHOD OF PREPARING PURE CHROMIC ACID.

BY CHARLES WATT, JUN.

To the plan at present in use for the preparation of this acid in a pure state, there are many objections, the principal of which are the very great trouble in the management of the process, and the expense of the requisite apparatus, which should be of silver or platinum.

Some idea of the labor attendant on the production of this acid in the pure state, by the process at present in use, may be gleaned from the fact, that it cannot be procured much under five shillings per ounce, although the articles from which it is prepared are very cheap.

The plan at present adopted consists in transmitting the gascous fluoride of chromium into water, when by the decomposition of the gas and the water, hydrofluoric and chromic acids are produced; the former is driven off by evaporating to dryness.

The method pursued for preparing chromic acid, where large quantities are required, consists in decomposing the chromate of lead, or even the chromate of potassa, by means of sulphuric acid; but Gay-Lussac has shown, that chromic acid thus prepared always contains some sulphuric acid, even although there might have been a preponderance of chromate of lead, and where the chromate of potassa is used, of course there is a large quantity of sulphate of potassa: these impurities are generally of no consequence in the purposes to which chromic acid is applied in the arts, although they render it quite unfit for use in the laboratory.

Chromic acid has, within the last few years, become so useful an agent in the hands of the chemist, that any method by which it may be easily and cheaply prepared, cannot but be a great desideratum.

The plan to which I have alluded consists in treating chromate of baryta with concentrated nitric acid, of which it is necessary to use a large preponderance.

The principle upon which this is based is, that nitrate of baryta, which results from the decomposition of the chromate of baryta by nitric acid, is quite insoluble in concentrated nitric acid, which I have verified by many experiments, and which fact was, I believe, first observed by Mr. Reuben Phillips, with whose name the readers of this Journal are familiar.

The chromic acid may be separated from the nitrate of baryta by decantation, or, which is still better, by filtration through asbestos. Care must be taken not to let it come in contact with any organic matter, or it will be decomposed.

The chromic acid is then to be evaporated to dryness, when the nitric acid will be volatilised, leaving pure chromic acid.

When the quantity of chromic acid prepared by this plan is considerable, to reduce the expense as much as possible it will be as well to carry on the evaporation so that the superabundance of nitric acid which has been used may be condensed, which may again be used for the same purpose.

To ascertain whether the chromic acid is free from nitrate of baryta before it is filtered, it will be as well just to take a little out, and test it with sulphuric acid; previous to doing which, it should be diluted with about six times its bulk of distilled water, as sulphate of baryta is slightly soluble in concentrated nitric acid; when, if it contains any nitrate of baryta, more nitric acid must be added.

The only precautions necessary to insure the purity of the chromic acid prepared by this plan, are the following;—to use a sufficient quantity of nitric acid, and to take care that the nitric acid is sufficiently concentrated, and that it is pure, otherwise the impurities which it contains will remain in the chromic acid.

The chromate of baryta may be easily prepared by mixing together solutions of the chloride of barium, and any of the soluble chromates; before it is used for the preparation of pure chromic acid it should be washed several times.

Chromate of lead may likewise be used for the preparation of chromic acid in the manner I have described; but I cannot recommend its employment when the chromic acid is required to be very pure, as I have never failed in detecting small portions of lead in it; however, it may be employed in cases where this slight impurity does not signify.

As nitrate of baryta is a marketable article, if large quantities of chromic acid are to be prepared by this means, the nitrate of baryta may be sold, or it may be used instead of the chloride of barium in the formation of the chromate of baryta, which will reduce the expense of the manufacture.

In conclusion, I may remark, that the chromic acid I have prepared by the plan I have described, is remarkably pure when the necessary precautions have been used, and does not contain a trace of nitrate of baryta.

ON THE SALTS OF HYDROGEN.

BY REUBEN PHILLIPS.

The great progress which has been made in the study of the compound radicals, and the undoubted existence of a class of polybasic acids, have gradually recalled the attention of chemists to Davy's profound views relative to the constitution of the chloric and iodic acids: if, observes Liebig, these views be applied generally to all hydrated acids, certain general laws may be deduced. On account of their great simplicity, I adopt these views in this paper.

At present chemists agree in considering that acid as polybasic which is capable of combining with one or more equivalents of metal, at the same time retaining one or more equivalents of basic hydrogen, the separation of one or more equivalents of combined hydrogen as water attending every such metallic equivalent which enters into the constitution of the salt. The general formula of these acids being $H_n + R$, R = the anhydrous acid. These acids may be separated into those in which the salt radical may be divided by the number of atoms of basic hydrogen, and into those in which it cannot; the former are most numerous. A good example is afforded by the cyanuric acid $= Cy_3 O_6 + H_3$; here the whole may be divided by 3 without producing fractions. An opposite example is given by the saccharic acid, the formula of which is $C_{12} H_5 O_{16} + H_5$, and the salt of lead in which all the hydrogen is replaced is $C_{12} H_5, O_{16} Pb_5$; here the quantity of lead can be neither 1, 2, 3, nor 4, as the quotient of the division will contain fractional quantities, and the fraction of the lowest equivalent proportion is impossible.

But it is highly doubtful whether any acids can be represented by their formulæ reduced to their lowest terms; the relative proportion of base and radical being maintained, there is nothing inconsistent with the supposition that they are multiples of these formulæ, in which case all acids may be considered polybasic, although they may be capable of forming only one series of

salts; for if the sums of the forces of attraction of the hydrogen for the radical and the oxygen for the metal exceed the sums of the attraction of the hydrogen for the oxygen and the radical for the metal, obviously no salt can he produced; in this reasoning the modifying causes which determine chemical action may he safely confounded with the attractive forces. As regards polyhasic acids; if the attractive forces of metal for radical, and hydrogen for oxygen, exceed the attraction of metal for oxygen, and radical for hydrogen, in an increasing ratio, that is, if, when one equivalent of hydrogen has been replaced, the next is removed with greater facility, then nothing but a neutral salt can result, and the acid is called monobasic. But if the relative ease with which the separate atoms of hydrogen are replaced follows in a decreasing proportion, then acid salts or double salts of hydrogen and metal may be obtained. The only difference then between monobasic acids, and polybasic acids of the cyanuric acid class, consists in the relative facility with which the different atoms of hydrogen may be replaced by metal. In the saccharic acid class the radical may he combined with a different proportion of hydrogen.

These views may he applied to the constitution of the double salts: let us take for example the sulphates. Let the coefficient of sulphuric acid be 12; many other numbers would do; it will then he $12\ (SO_4 + H) = S_{12}\ O_{48} + H_{12} = R + H_{12}$; then will bisulphate of potassa be $(K_6 + H_6) + R$; the double sulphate of magnesia and potassa will be $(K_6 + Mg_6) + R$; the sesquisulphate of alumina will be $Al_8 + R$, 3 eq. of which interchanging elements with $K_{12} + R$, will produce 4 eq. of $(K_3 + Al_6) + R = 4$ eq. of alum. So that double salts may be considered as nothing but salts of polybasic acids, in which the hydrogen has been replaced by a plurality of metals.

ON THE PRESENCE OF ANTIMONY IN ARSENIOUS ACID.*

BY A. WIGGERS.

M. Wiggers tried, some time ago, to keep transparent fragments of arsenious acid under hydrochloric acid in order to obtain them translucid; he did not succeed. The arsenious acid gradually became tinted and opaque; but the hydrochloric acid, decanted and tested, showed a very large quantity of oxide of antimony, $Sb^2\ O^3$. In many cases it may be important to detect this mixture, and in this respect the communication is not uninteresting. The oxide of antimony is sublimed in great part with the arsenious acid. The hydrochloric acid completely dissolves this impure arsenious acid, forming a solution which yields a white precipitate with water, and which, with sulphuretted hydrogen, at first gives a red precipitate of $Sb^2\ S^3$ (sesquisulphuret of antimony), and then a yellow precipitate of sulphuret of arsenic. Nitric acid dissolves it with the aid of heat, leaving a residue of oxide of antimony containing arsenic acid, which readily dissolves in hydrochloric or tartaric acid, and forms with these acids solutions which present all the reactions of oxide of antimony. M. Wiggers sought to prove the presence of oxide of antimony in several kinds of arsenious acid: he found it in some of them, especially in the vitreous arsenious acid of Andreasberg in the Hartz, and not in the others.

* *Annalen der Chemie und Pharmacie*, Vol. XLV. No. III.

FORMATION OF FORMIC ACID IN OIL OF TURPENTINE.*

BY F. WEPPEN.

The acid reaction of the oil of turpentine of commerce arises from formic acid, whose presence in the water used for purifying the oil may easily be demonstrated.

The formation of this acid can be explained only by the oxidation of the oil in contact with the air. The action might be very simple.

1 A. Oil of turpentine = 5C 8H + 1O =

2 ,, Formic acid . . = 4C 4H + 6O
1 ,, Carbonic acid . 1C 2O
2 ,, Water . . . . . 4H + 2O

It appeared interesting to M. Weppen to ascertain whether things actually occurred thus, or, indeed, whether other products were not formed in the oxidation.

As oil of turpentine oxidises proportionally only very slowly in the air, he sought to effect the oxidation by another means, by distilling with chromate of lead and dilute sulphuric acid oil completely free from acid. Soon after the mixture began to boil, the chromate of lead was reduced; there passed over water with an acid reaction, in which the presence of formic acid was detected, and at the same time there was disengaged by the tubulure of the receiver sufficient carbonic acid to render lime water very turbid: but is this carbonic acid really due to the oxidation of the oil of turpentine? Or is it not rather a secondary product arising from the formic acid?

* *Annalen der Chemie und Pharmacie*, Vol. XLI. No. III.

REVIEW:—

ANIMAL CHEMISTRY; *or Organic Chemistry in its Applications to Physiology and Pathology.* By JUSTUS LIEBIG, M.D., Ph. D., F.R.S., M.R.I.A., Professor of Chemistry in the University of Giessen. Edited from the Author's Manuscript, by WILLIAM GREGORY, M.D., F.R.S.E., Professor of Medicine and Chemistry in the University and King's College, Aberdeen. London: Taylor and Walton, Upper Gower Street. pp. 354. 1842.

ANOTHER excellent contribution to organic chemistry, and one which fully equals, if it does not surpass, all the other works of this illustrious chemist. There is a greater boldness and more originality of thought in Liebig than in any other chemical writer of the day: he exhibits also a far greater power of conception and of deducing theories from facts. In the peculiar field of research which he explores, he stands alone; none can approach him; he has appropriated that field to himself.

This volume is of the highest interest: the most novel and ingenious theories are met with on almost every page.

Dr. Liebig, in his preface, says:—"My object in the present work has been to direct attention to the points of intersection of chemistry with physiology, and to point out those parts in which the sciences become, as it were, mixed up together. It contains a collection of problems, such as chemistry requires to be resolved; and a number of conclusions drawn according to the rules of that science from such observations as have been made."

"These questions and problems will be resolved: and we cannot doubt that we shall have in that case a new physiology and a rational pathology. Our sounding line, indeed, is not long enough to measure the depths of the sea, but is not on that account the less valuable to us; if it assist us, in the mean time, to avoid rocks and shoals, its use is sufficiently obvious. In the hands of the physiologist, organic chemistry must become an intellectual instrument by means of which he will be enabled to trace the causes of phenomena invisible to the bodily sight; and if, among the results which I have developed or indicated in this work, one alone shall admit of a useful application, I shall consider the object for which it was written as fully attained. The path which has led to it will open other paths; and this I consider as the most important object to be gained."

To give an analysis of the contents of this extraordinary work is unnecessary, as we could by that means give only a very faint idea of its character.

The theories promulgated by the illustrious chemist of Giessen it would be unfair to criticise until sufficient time has elapsed to allow of their confirmation or contradiction by further investigation. Be this as it may,—be his doctrines wholly or partially adopted by the scientific world,—this work must ever stand forth as the offspring of a brilliant genius, and we shall admire the ingenuity of the author even in devising theories which he or others may have found less correct than was at first supposed.

But in thus paying a well merited tribute to Liebig, let us not overlook the labors of the eminent Translator of this work, to whom men of science are indebted for other translapertionsof the same author's work. Dr. Gregory has formed his tedious task (we speak feelingly) in a masterly manner.

We give, by way of extract, Professor Liebig's

THEORY OF DISEASE.

"Every substance or matter, every chemical or mechanical agency, which changes or disturbs the restoration of the equilibrium between the manifestations of the causes of waste and supply, in such a way as to add its actions to the cause of waste, is called *a cause of disease*. *Disease* occurs when the sum of vital force, which tends to neutralise all causes of disturbance, (in other words, when the resistance offered by the vital force,) is weaker than the acting cause of disturbance.

Death is that condition in which all resistance on the part of the vital force entirely ceases. So long as this condition is not established, the living tissues continue to offer resistance.

To the observer, the action of a cause of disease exhibits itself in the disturbance of the proportion between waste and supply, which is proper to each period of life. In medicine, every abnormal condition of supply or of waste, in all parts or in a single part of the body, is called disease.

It is evident that one and the same cause of disease will produce in the organism very different effects, according to the period of life; and that a certain amount of disturbance, which produces disease in the adult state, may be without influence in childhood or in old age. A cause of disease may, when it is added to the cause of waste in old age, produce death (annihilate all resistance on the part of the vital force); while in the adult state it may produce only a disproportion between supply and waste; and in infancy, only an equilibrium between supply and waste (the abstract state of health).

A cause of disease which strengthens the causes of supply, either directly, or indirectly by weakening the action of the causes of waste, destroys, in the child and in the adult, the relative normal state of health; while in old age it merely brings the waste and supply into equilibrium.

A child, lightly clothed, can bear cooling by a low external temperature without injury to health; the force available for mechanical purposes and the temperature of its body increase with the change of matter which follows the cooling; while a high temperature, which impedes the change of matter, is followed by disease.

On the other hand, we see, in hospitals and charitable institutions (in Brussels, for example,) in which old people spend the last years of life, when the temperature of the dormitory, in winter, sinks two or three degrees below the usual point, that by this slight degree of cooling the death of the oldest and weakest, males as well as females, is brought about. They are found lying tranquilly in bed, without the slightest symptoms of disease, or of the usual recognisable causes of death.

A deficiency of resistance, in a living part, to the causes of waste is, obviously, a deficiency of resistance to the action of the oxygen of the atmosphere.

When, from any cause whatever, this resistance diminishes in a living part, the change of matter increases in an equal degree.

Now, since the phenomena of motion in the animal body are dependent on the change of matter, the increase of the change of matter in any given part is followed by an increase of all motions.

According to the conducting power of the nerves, the available force is carried away by the nerves of involuntary motion alone, or by all the nerves together.

Consequently, if, in consequence of a diseased transformation of living tissues, a greater amount of force be generated than is required for the production of the normal motions, it is seen in an acceleration of all or some of the involuntary motions, as well as in a higher temperature of the diseased part.

This condition is called *fever*.

When a great excess of force is produced by change of matter, the force, since it can only be consumed by motion, extends itself to the apparatus of voluntary motion.

This state is called a *febrile paroxysm*.

In consequence of this acceleration of circulation in the state of fever, a great amount of arterial blood, and, consequently, of oxygen, is conveyed to the diseased part, as well as to all other parts; and if the active force in the healthy parts continue uniform, the whole action of the excess of oxygen must be exerted on the diseased part alone.

According as a single organ, or a system of organs, is affected, the change of matter extends to one part alone, or to the whole affected system.

Should there be formed, in the diseased part, in consequence of the change of matter, from the elements of the blood or of the tissue, new products, which the neighbouring parts cannot employ for their own vital functions;—should the surrounding parts, moreover, be unable to convey these products to other parts where they may undergo transformation;—then these new products will suffer, at the place where they have been formed, a process of decomposition analogous to fermentation or putrefaction.

In certain cases, medicine removes these diseased conditions, by exciting in the vicinity of the diseased part, or in any convenient situation, an artificial diseased state (as by blisters, sinapisms, or setons); thus diminishing, by means of artificial disturbance, the resistance offered to the external causes of change in these parts by the vital force. The physician succeeds in putting an end to the original diseased condition, when the disturbance artificially excited (or the diminution of resistance in another part) exceeds in amount the diseased state to be overcome.

The accelerated change of matter and the elevated temperature in the diseased part show, that the resistance offered by the vital force to the action of oxygen is feebler than in the healthy state. But this resistance only ceases entirely when death takes place. By the artificial diminution of resistance in another part, the resistance in the diseased organ is not, indeed, directly strengthened; but the chemical action (the cause of the change of matter) is diminished in the diseased part, being directed to another part, where the physician has succeeded in producing a still more feeble resistance to the change of matter (to the action of oxygen).

A complete cure of the original disease occurs, when external action and resistance, in the diseased part, are brought into equilibrium. Health and the restoration of the diseased tissue to its original condition follow, when we are able so far to weaken the disturbing action of oxygen, by any means, that it becomes inferior to the resistance offered by the vital force, which, although enfeebled, has never ceased to act; for this proportion between these causes of change is the uniform and necessary condition of increase of mass in the living organism.

In cases of a different kind, where artificial external disturbance produces no effect, the physician adopts other indirect methods to exalt the resistance offered by the vital force. These methods, the results of ages of experience, are such, that the most perfect theory could hardly have pointed them out more accurately or more justly than has been done by the observation of sagacious practitioners. He diminishes, by bloodletting, the number of the carriers of oxygen (the globules), and by this means the condition of change of matter; he excludes from the food all such matters as are capable of conversion into blood; he gives chiefly or entirely non-azotised food, which supports the respiratory process, as well as fruit and vegetables, which contain the alkalies necessary for the secretions.

If he succeed by these means, in diminishing the action of oxygen in the blood on the diseased part, so far that the vital force of the latter, its resistance, in the smallest degree overcomes the chemical action; and if he accomplish this, without arresting the functions of the other organs; their restoration to health is certain.

To this method of cure adopted in such cases, if employed with sagacity and acute observation, there is added, as we may call it, an ally on the side of the diseased organ, and this is the vital force of the healthy parts. For, when blood is abstracted, the external causes of change are diminished also in them, and their vital force, formerly neutralised by these causes, now obtains the preponderance. The change of matter, indeed, is diminished throughout the body, and with it the phenomenon of motion; but the sum of all resisting powers, taken together, increases in proportion as the amount of the oxygen acting on them in the blood is diminished. In the cessation of *hunger*, this resistance, in a certain sense, makes itself known; and the preponderating vital force exhibits itself, in many patients when hunger is felt, in the form of an abnormal growth, or an abnormal metamorphosis of certain parts of organs. *Sympathy* is the transference of diminished resistance from one part, not exactly to the next, but to more distant organs, when the functions of both mutually influence each other. When the action of the diseased organ is connected with that of another—when, for example, the one no longer produces the matters necessary to the performance of the functions of the other—then the diseased condition is transferred, but only apparently, to the latter.

In regard to the nature and essence of the vital force, we can hardly deceive ourselves, when we reflect, that it behaves, in all its manifestations, exactly like other natural forces; that it is devoid of conciousness or of volition, and is subject to the action of a blister.

The nerves, which accomplish the voluntary motions in the body, are, according to the preceding exposition, not the producers, but only the conductors, of the vital force; they propagate motion, and behave towards other causes of motion, which in their manifestations are analogous to the vital force, towards a current of electricity, for example, in a precisely analogous manner. They permit the current to traverse them, and present, as conductors of electricity, all the phenomena which they exhibit as conductors of the vital force. In the present state of our knowledge, no one, probably, will imagine that electricity is to be considered as the cause of the phenomenon of motion in the body; but still the medicinal action of electricity, as well as that of the magnet, which, when placed in contact with the body, produces a current of electricity, cannot be denied. For to the existing force of motion or of disturbance there is added, in the electrical currents, a new cause of motion and of change in form and structure, which cannot be considered as altogether inefficient.

Practical medicine, in many diseases, makes use of cold in a highly rational manner, as a means of exalting and accelerating, in an unwonted degree, the change of matter. This occurs especially in certain morbid conditions in the substance of the centre of the apparatus of motion; when a glowing heat and a rapid current of blood towards the head point out an abnormal metamorphosis of the brain. When this condition continues beyond a certain time, experience teaches that all motions in the body cease. If the change of matter be chiefly confined to the brain, then the change of matter, the generation of force, diminishes in all other parts. By surrounding the head with ice, the temperature is lowered, but the cause of the liberation of heat continues; the metamorphosis, which decides the issue of the disease, is limited to a short period. We must not forget, that the ice melts and absorbs heat from the diseased part; that if the ice be removed before the completion of the metamorphosis, the temperature again rises; that far more heat is removed by means of ice, than if we were to surround the head with a bad conductor of heat. There has obviously been liberated in an equal time, a far larger amount of heat than in a state of health; and this is only rendered possible by an increased supply of oxygen, which must have determined a more rapid change of matter.

The self-regulating steam-engines, in which, to produce a uniform motion, the human intellect has shown the most admirable acuteness and sagacity, furnish no unapt image of what occurs in the animal body.

Every one knows, that in a tube which conveys the steam to the cylinder where the piston-rod is to be raised, a stop-cock of peculiar construction is placed, through which all the steam must pass. By an arrangement connected with the regulating wheel, the stop-cock opens when the wheel moves slower, and closes more or less completely when the wheel moves faster than is required for a uniform motion. When it opens, more steam is admitted (more force), and the motion of the machine is accelerated. When it shuts, the steam is more or less cut off, the force acting on the piston-rod diminishes, the tension of the steam increases, and this tension is accumulated for subsequent use. The tension of the vapor, or the force, so to speak, is produced by change of matter, by the combustion of coals in the fire-place. The force increases (the amount of steam generated and its tensions increase) with the temperature in the fire-place, which depends on the supply of coals and of air. There are in these engines other arrangements, all intended for regulation. When the tension of steam in the boiler rises beyond a certain point, the passages for the admission of air close themselves; the combustion is retarded, the supply of force (of steam) is diminished. When the engine goes slower, more steam is admitted to the cylinder, its tension diminishes, the air-passages are opened, and the cause of disengagement of heat (or production of force) increases. Another arrangement supplies the fire-place incessantly with coals in proportion as they are wanted.

If we now lower the temperature at any part of the boiler, the tension within is diminished; this is immediately seen in the regulators of force, which act precisely as if we had removed from the boiler a certain quantity of steam (force). The regulators and the air-passages open, and the machine supplies itself with more coals.

The body, in regard to the production of heat and of force, acts just like one of these machines. With the lowering of the external temperature, the respirations become deeper and more frequent; oxygen is supplied in greater quantity and of greater density; the change of matter is increased, and more food must be supplied, if the temperature of the body is to remain unchanged.

It is hardly necessary to mention, that in the body, the tension of vapor cannot, any more than an electric current, be considered the cause of the production of force.

From the theory of disease mentioned in the preceding pages, it follows obviously, that a diseased condition once established, in any part of the body, cannot be made to disappear by the chemical action of a remedy. A limit may be put by a remedy to an abnormal process of transformation; that process may be accelerated or retarded; but this alone does not restore the normal (healthy) condition.

The art of the physician consists in the knowledge of the means which enable him to exercise an influence on the *duration* of the disease; and in the removal of all disturbing causes, the action of which strengthens or increases that of the actual cause of disease.

It is only by a just application of its principles that any theory can produce really beneficial results. The very same method of cure may restore health in one individual, which, if applied to another, may prove fatal in its effects. Thus in certain inflammatory diseases, and in highly muscular subjects, the antiphlogistic treatment has a very high value; while in other cases, blood-letting produces unfavorable results. The vivifying agency of the blood must ever continue to be the most important condition in the restoration of a disturbed equilibrium, which result is always dependent on the saving of time; and the blood must, therefore, be considered and constantly kept in view, as the ultimate and most powerful cause of a lasting vital resistance, as well in the diseased as in the unaffected parts of the body.

It is obvious, moreover, that in all diseases where the formation of contagious matter and of exanthemata is accompanied by fever, two diseased conditions simultaneously exist, and two processes are simultaneously completed; and that the blood, as it were by re-action (*i. e.* fever), becomes a means of cure, as being the carrier of that substance (oxygen) without the aid of which the diseased products cannot be rendered harmless, destroyed, or expelled from the body; a means of cure by which, in short, neutralisation, or equilibrium is effected."

Every chemist, physiologist, and pathologist should carefully study this work; every medical man desirous of rising in his profession, and every chemist and druggist, should give it an attentive perusal. It is of the highest interest to all lovers of science, who, we feel assured, will thank us for bringing it under their notice.

II. CHEMICAL MANUFACTURES.

ON THE PRESENCE OF LEAD IN WATER, OWING TO THE USE OF LEADEN PUMPS.

To the Editors of The Chemist.

Southampton, June 22, 1842.

GENTLEMEN,—

Through the medium of "THE CHEMIST," allow me to suggest the necessity of having the water which is intended for domestic use always submitted to chemical examination, prior to the erection of lead pumps. My reason for promoting this adoption by the public is owing to a medical gentleman of this town, Mr. Ware, having last summer some patients, residing in the country, who had for a long time been laboring under symptoms similar to those produced by the absorption of lead, although none of them had ever been exposed to the influence of that poison. Mr. Ware suspected the water, which they were in the habit of drinking, might be impregnated with lead by some means or other, and desired me to examine it; but, on doing so, I could detect only the slightest trace of that metal. A few days after, however, it occurred to me, that many country people were in the habit of taking their breakfast as soon as the water could be made to boil; consequently, the kettle was generally filled with water before any had been pumped up for any other purpose; and, on making the inquiry, I found this was their usual custom. I therefore desired a bottle to be filled in the morning before any water had been taken out of the pump; and, on submitting it to the usual tests, lead was precipitated in a considerable quantity, and sufficient, as was afterwards proved, to account for the cause of the disease.

Thinking this case rather an important one, I took the liberty of naming the particulars to Dr. Oke, a physician, having an extensive practice in the town and neighbourhood, and who at once determined to have the water examined when a similar case came under his notice; and in the course of a fortnight, a person residing in the country went to consult him for "intestinodynia, with constipation, accompanied by atrophy and loss of muscular power." Dr. Oke suspected the water might be the cause of these symptoms, and requested me to test it in the same manner as I had done before; when, to my great surprise, I found that even a larger quantity of lead was held in solution than in the former. A full account of the case was published by Dr. Oke, in the *Provincial Medical and Surgical Journal*, Sept. 1841; and since then I have had water sent to me from other places, either containing or capable of acting on lead with great energy.

The solvent principle, I found, is owing to an uncombined acid, the hydrochloric, which, although it exists in a very dilute state, is capable of taking up a sufficient quantity of lead, when the pump is at rest for a few hours, to render it highly injurious; and, when poured into the system from day to day, and year after year, acts as a slow and fatal poison. If the hydrochloric acid is found in water in various districts, and at a distance of twenty miles apart, I am led to conclude this solvent principle may also be found in many other parts of England; and I am of opinion that some step ought to be taken to guard against the risk of drinking water containing lead; and this may be easily done by immersing a piece of the metal in a tumbler of water, and allowing it to remain in during the night, and in the morning to test it with a solution of chromate of potash and sulphuretted hydrogen; and if a yellow precipitate from the former, and a black one from the latter, are deposited after standing for twenty-four hours, a leaden pump should on no account be erected, but one composed of iron or some other metal must be substituted, or, which I am induced to believe would answer equally as well, to use chalk instead of brick for staining the well, provided an uncombined acid is detected with litmus paper after concentrating the water. I suggest this mode, because the water which is capable of dissolving lead thus containing the acid, is rendered incapable of acting upon it, by agitating the water with chalk; therefore, I should suppose that chalk instead of brick would answer the purpose for saturating the acid as fast as the water passed into the well from the springs.

If, Gentlemen, you consider the above worthy the attention of your readers, or of any benefit to the public, by giving it a place in THE CHEMIST, you will oblige

Your humble Servant,

HENRY OSBORN.

POTATO STARCH.

BY M. PAYEN.

THE following is the substance of an interesting lecture, delivered by M. Payen, at the *Conservatoire des Arts et Metiers*, at Paris, on the extraction of starch and amidine from the potato, as given in the *Inventor's Advocate*, of January 5, 1842.

The potato appears to have been first discovered in Virginia; it is also to be found at Valparaiso, in Chili, and grows spontaneously at Monte Video. Imported into Italy from the equatorial regions, it was successively introduced in Germany, Spain, and Great Britain, and it was not till the end of the sixteenth century that it was known in France. An unfavourable prejudice prevailed in this country for some time against this valuable root; its use was even forbidden, and its cultivation remained at a stand for many years. The article devoted to it in the *Encyclopædia* of 1765, in exposing the unfounded nature of this prejudice, humorously concludes thus: "The potato is charged with having a tendency to create wind, but what signifies wind to the organs of a peasant or a workman."

In 1785, Parmentier labored with greater success to convince us of the advantages which the potato offered in its economical uses; in 1786, calamities of every kind gave a certain impetus to the cultivation of this plant. An ingenious trick also contributed materially to it. It is well known, that the philanthropic Parmentier pretended to confide to the *gens d'armerie* the care of a field of potatos, in order to induce them to rob it. He attained his end, for in a short time it was completely pillaged. Louis XVI. received with kindness a bouquet that Parmentier had composed of the *solanum tuberosum*. What more ingenious means could be devised for giving popularity to a plant which had been hitherto despised, and which offered the surest guarantee against famine? courtiers and courtezans outvied each other in zeal to cultivate a plant which had been honored by the notice of the king. To court flattery we also owe the impetus that was given to the cultivation of the beet root, and the success of our manufactories of indigenous sugar. When we see the numberless uses to which the potato, and the extracts which are obtained from it, are devoted; when we are every day called upon to appreciate the value of the resources derived from its culture, and its employment a salimentary matter,—is it not to be lamented that the name of philanthropists like Parmentier should have been suffered to fall into oblivion? There is not even a stone which recalls to suffering humanity a memory that ought to be dear to it.

The potato contains an alimentary substance, *fecule*, or potato starch, which is obtained by the simplest and most economical processes, and which can be preserved for almost an indefinite period, as it is insensible to the effects either of humidity or dryness in the atmosphere. The manufacture of potato starch is an industry almost exclusively French; its uses are varied, for it forms the basis of the alimentary pastes, such as tapioca, vermicelli, &c., and serves to manufacture the *dextrine* and syrup, or sugar of starch. The different kinds of the potato are extremely numerous, and it is only necessary to reproduce this plant from seed to obtain from it every possible variety; and each species contains a quantity of starch, which varies according to the nature of the soil, temperature or climate. The more precocious or quick in growth the potato is, the greater amount of profit will the starch manufacturer obtain from it, because one of the conditions of success in this industry is celerity in the operations, the season exercising a powerful influence on the root. Preference should also be given to potatos which contain the least moisture, because they preserve better; they furnish in the same quantity of the root a greater amount of starch, and without any increase of expense—the process of extraction being the same for potatos of a rich or poor quality. The potato should have arrived at its last degree of maturity, which, in some varieties, is known by the skin being furrowed, and peeling off in scales. The starch manufacturer should test with an iron grater, or rasp the samples which are offered to him, in order to determine their selling value by the proportion of starch obtained from them by this preliminary trial. To judge of their real value, however, he must know the proportions of water and dry matter that every variety contains, and which depend on the root itself, on the nature of the soil in which it was grown, or the mode of cultivation. The differences in these respects are considerable, for after many trials the proportion of dry matter varies in the different sorts of potato from 0·33 to 0·24. The following table indicates the influence of the season on the amount of starch obtained from the different varieties of the root.—October, 0·172; November, 0·168; December, 0·156; January, 0·155; February, 0·152; March, 0·15; April, 0·145.

To ascertain exactly the proportions of water and dry matter, which it is necessary to know when the crop is gathered in, a brush is used for the purpose of removing carefully the earth adhering to the surface. The potato is then cut in small slices, and placed on a smooth plate beside each other, but without touching or covering. It is afterwards dried in a hot-house or stove, heated from 40 to 50 degrees. The process of drying is known to be complete, when, after having weighed the slices several times running, at intervals of from forty to fifty minutes, the same weight is each time obtained. The slices are then hard and brittle.

The following is the process by which the extract is usually obtained on a small scale.

When the potatos have been washed, and reduced into a fine pulp by the aid of a grater, the pulp is held over hair or wire sieves. It is then rubbed quickly between the hands in order to draw off, by a current of water which runs through the sieves, all the starch which is freed by the separation of the fibres of the potato; the liquid then runs clear through the sieve. When all the parts of the starch have been drawn off by the water, the exhausted pulp is thrown aside; a new layer of pulp is then put on the sieves, and the same process continued. The starch obtained by the suspension of the pulp in the water, deposits itself at the bottom of the vessel; it is again mixed with the liquid, and left to deposit two or three times successively, the water being changed each time. It is then tasted and delivered for sale in the moist state, or it is dried in the hot-house or stove, when it is necessary to send it to a distance.

After this brief sketch of the mode of manufacture, it is easy to judge how large must be the quantity of water used when the works are extensive. The water employed in these manufactories contains a solution of azotic matter (96 to 100), some portions of the rejected pulp, and a small quantity of starch. At one time the manufacturers suffered the water to be lost in the neighbouring pools or marshes, or it formed a pond in itself. At present it serves both the purposes of manure and irrigation, of which we have begun to recognize the importance in some parts of the country, but especially in the south.

M. Dailly, whose zeal for agricultural improvements has frequently brought him before the notice of the public, has obtained important advantages from the employment of the water of his manufactory in the irrigation of the land which surrounds his establishment near Versailles.

In a single year he has calculated that he has obtained 1,602 francs profit by it. The expenses of directing the water, drying the deposits, and manual labor, cost him about 440 francs. It will be seen from this, that the cultivation of the potato is one of the most productive, relatively to the amount of substance which it furnishes, and one of the most advantageous in other respects that is known in agriculture.

MEMOIR ON CURCUMINE.*

BY M. VOGEL, JUN.

Coloring matters have frequently been made the subject of chemical investigation: Chevreul, especially, has lately devoted himself to that branch of chemistry which relates to the art of dyeing.

The root of the *Curcuma longa*, or *Amomum curcuma*, known in commerce by the names of *terra merita*, and *turmeric*, contains a yellow coloring matter, which, although not of great utility to the dyer, is, to the chemist, an indispensable and valuable re-agent. We know that the yellow coloring matter is accompanied in the root by several other substances,* and that the isolation of this yellow matter, in a state of purity, presents some difficulties.

In the experiments which I have lately made on the root of curcuma, I endeavored to separate curcumine from the other substances which accompany it, and I afterwards determined the elementary combination.

The brown extractive substance of the root is, it is true, soluble in water, but it is not possible to extract it completely by treating the root several times with boiling water. Besides the volatile oil, with a penetrating odor, the root also contains some inorganic salts, of which the chloride of calcium, which had already been found in it by M. Pelletier, forms the chief part.

After several fruitless experiments which I made, with the intention of isolating curcumine, I had recourse to different modes, by which it became possible to obtain curcumine in a state of complete purity, and in sufficient quantity.

One part of the root of curcuma reduced to powder is treated several times with boiling water, until the water is no longer colored. There is poured on the residue, dried and thus freed by water from mucilaginous and gummy substances, and a small portion of extractive matter, alcohol of 0·8, which is boiled with it for some time, and which is several times renewed. The alcohol dissolves the greater portion of the coloring matter, although it is not possible to extract it completely, for the powder of curcuma, treated by alcohol, is still colored. When the matrass is sufficiently cooled, it is filtered, and the alcohol passes through completely clear and of a deep brownish red. In order to again take up part of the alcohol, the tincture is distilled, and the residue evaporated to dryness in a porcelain capsule. A brown viscous mass remains, which still contains brown extractive matter and traces of chloride of calcium. In order to separate these two substances, I treated the residue, as M. Pelletier had already done, by boiling ether, which acquires a brownish

* *Journal de Pharmacie*, July, 1842.

* See the Memoir of MM. Pelletier and Vogel, *Journal de Pharmacie*, July, 1815, p. 259.

yellow color. The extractive substance, which resists the action of ether, presents a blackish mass, which always attracts humidity from the air, on account of the chloride of calcium which it contains. The ether, when decanted, should be slowly evaporated, and after cooling, reddish brown fragments remain, which readily fuse, and which, in the liquid state, may be cooled into fine layers in stone moulds or on plates of glass. In this state, curcumine, heated to redness on a sheet of platinum, does not leave the least trace of residue, and may be considered as perfectly deprived of inorganic substances. By heating curcumine, and fusing it several times, I endeavored to evaporate the volatile oil, whose presence was still manifested by a peculiar odor; but as that did not succeed, I used another mode, which was attended with a more satisfactory result.

I dissolved in alcohol the residue evaporated from the ethereal solution, and I poured into it an alcoholic solution of acetate of lead, which speedily formed a red precipitate. I added the solution of acetate of lead until no more precipitate was formed. After having washed and dried this precipitate, there remained a reddish yellow powder, which is the compound of the yellow coloring substance with oxide of lead. The quantity of lead in this combination is not uniform; it varies, according to several experiments which I have made, between 43·67 and 56·33 per cent. To separate the lead, the dried precipitate is put into a flask filled with water, and a current of sulphuretted hydrogen gas is passed into it for some time. When the decomposition is complete, the powder, which has become of a deep brown by the action of the sulphuretted hydrogen gas, should be washed and dried; it is then treated with boiling ether, which dissolves the curcumine and leaves the sulphuret as an insoluble residue. By slowly evaporating the ether, the curcumine is deposited in fine, inodorous, transparent plates. The plates, well dried, give by trituration a fine powder, of a beautiful yellow. The color is so much the more intense as the powder is finer. The curcumine in fine laminæ is of a cinnamon brown, but held before the light it is of deep red.

According to the process indicated, nearly half an ounce of curcumine may be obtained from a pound of curcuma roots.

Although I have taken much trouble to make curcumine crystallise or sublime, I have been unable to do so; it only remains for me, therefore, to examine curcumine in the state of laminæ.

Curcumine is fusible at 40° C., and at the ordinary temperature the fine powder collects into a mass. It burns with a lively flame, and forms much soot.

When curcumine is exposed to the solar light, it very soon loses its intensity of color, and gradually becomes of a yellowish white.

As curcumine is insoluble in water, but very soluble in alcohol and ether, it seems to resemble the resins in its nature.

M. Chevreul has already announced, that curcumine is composed of oxygen, carbon, and hydrogen, in undetermined proportions. I have ascertained, by fusing curcumine in a tube, with six times its weight of hydrate of potassa, that it does not contain nitrogen, because not the least trace of ammonia was disengaged during the operation.

The curcumine which I used in these analyses was well dried, in fine powder, and was procured from the combination with oxide of lead.

Four combustions of curcumine, with deutoxide of copper, gave the following results:—

I.	0·392 gr. of curcumine gave	0·266 of water, and	0·986 of carbonic acid.
II.	0·253	0·174	0·636
III.	0·283	0·192	0·711
IV.	0·419	0·270	1·053

That is to say:—	I.	II.	III.	IV.
Carbon . . .	69·548	69·507	69·466	69·485
Hydrogen . .	7·539	7·641	7·502	7·159
Oxygen . .	22·913	22·852	23·032	23·356
	100·000	100·000	100·000	100·000

The mean result of these four analyses gives the composition of curcumine as follows:—

Carbon . . .	69·501
Hydrogen . . .	7·460
Oxygen . . .	23·039
	100·000

ACTION OF ACIDS ON CURCUMINE.

Dilute acids have no solvent action on curcumine, but concentrated acids have the property of dissolving it. It was principally the action of sulphuric acid that I at first examined. When concentrated sulphuric acid is

poured on powdered curcumine, it completely dissolves it at the ordinary temperature, and a crimson liquid results. By the addition of water, the red color immediately disappears, and, after some time, greenish yellow flocks, which behave like pure curcumine, are deposited.

Concentrated phosphoric and hydrochloric acids act on curcumine in the same manner as concentrated sulphuric acid. Concentrated acetic acid dissolves curcumine, without causing it to undergo any change of color.

The action of nitric acid on curcumine differs from that of the above-mentioned mineral acids. I mixed one part of curcumine, in a porcelain capsule, with two parts of concentrated nitric acid, previously diluted with twice its volume of water. At the ordinary temperature, no change was perceived; but as soon as the temperature of the mixture was gently raised on a sand-bath, the nitric acid produced a very vivid action. The liquid was in bubbles, so that it was necessary to remove the capsule from the sand-bath, until the violent action had ceased. After that, I continued to heat it gently, until no more gas was disengaged. By the action of nitric acid, curcumine was decomposed into two different bodies,—a resinous matter, which was deposited in yellow fragments, and a yellow substance soluble in water.

The resinous substance, sufficiently washed several times with boiling water, and afterwards dried, may be easily reduced to a fine yellow powder, which differs much from curcumine in its peculiar odor, and in its elementary composition.

The other substance, soluble in water, formed by the action of nitric acid, crystallises in transparent needles; but it is formed in such small quantity, and so easily liquefies in the air, that I have not yet been able to examine its chemical constitution in a sufficient manner.

By the above experiments, concerning the action of acids on curcumine, it is easily explained how turmeric paper is turned from yellow to brown, when treated with a concentrated acid, as well as by the action of an alkali; the concentrated acids having the faculty, as I have said above, of dissolving curcumine, and of forming a deep brown solution. The same thing takes place when a strip of turmeric paper is steeped in a concentrated acid. The thin layer of curcumine on the surface of the paper is dissolved in the acid, and gives the paper a brown color.

ACTION OF ALKALINE SUBSTANCES ON CURCUMINE.

With the alkalis, curcumine forms compounds, which are very soluble in water. When powdered pure curcumine is treated with caustic potassa, a brown mass which is very soluble in water results from it. From this alkaline solution, curcumine is completely precipitated by dilute acids. I extracted by this means a certain quantity of curcumine, by treating the roots of curcuma with a boiling solution of caustic potassa. With sulphuric acid there is formed in the alkaline solution a yellow precipitate, which, being sufficiently washed, acts like pure curcumine.

The frequent use of curcumine as a test for alkalis induced me to examine it in this respect.

It is not, however, the alkalis and alkaline earths alone that change the color of curcumine from yellow to brown; it is also, according to the observation of Kartner, thus affected by the salts of oxide of lead, oxide of uranium, as well as by boracic acid and the borates.

The shades of brown produced on turmeric paper, by either the alkalis or the alkaline earths, do not sensibly differ from one another; they depend on the concentration of the alkaline solution used. All weak acids return to the paper turned brown by alkaline substances, its primitive color. The above experiments give, as it appears to me, the explanation of this phenomenon. When, for example, turmeric paper is steeped in a weak solution of potassa, it turns brown on account of the compound which is formed by the curcuma and the alkali. By wetting the browned paper with an acid, it resumes its yellow color, because the curcumine is separated by the acid from its alkaline combination.

The soluble salts of oxide of lead and of uranium, also change the colour of turmeric paper to brown; the shade produced by the salts of lead differs in no respect from that occasioned by the alkalis, but the shade produced by the salts of uranium, is much deeper, and almost black. Turmeric paper turned brown by a salt of lead, readily resumes its primitive yellow colour by very dilute acids, but paper turned brown by the salts of uranium, must be steeped in a very concentrated acid to restore it to its original color.

An alcoholic solution of boracic acid gives turmeric paper a deep orange color, which does not disappear by the action of any other acids. When the paper thus colored by boracic acid, is touched with a few drops of ammonia, it acquires for a moment a beautiful blue color, which soon disappears, owing to the volatilisation of the ammonia. This blue tint is also exhibited

when turmeric paper changed in color by boracic acid, is steeped for some time in other alkaline solutions.

A solution of borax turns turmeric paper of a blackish grey: the neutral borates with potassa or ammonia for a base, give it a tint of a less deep grey.

Resumé.

From the above it results:—

1. That it is possible to obtain curcumine in a state of purity by separating it from its combination with oxide of lead.

2. That coneentrated sulphuric, phosphoric and hydrochloric acids have the property of dissolving curcumine, from which solution curcumine may be precipitated in yellow flocks, by means of water.

3. That nitric acid decomposes curcumine into a yellow substance, which more resembles the resins than curcumine itself.

4. That with the alkalis curcumine forms brown combinations, which are very soluble in water, and which may be separated by the dilute acids.

5. That curcumine acquires a much deeper tint by the salts of uranium, than by the salts of lead and alkaline substances.

6. That the soluble borates form with curcumine a compound of a more or less deep grey.

7. Lastly, that turmeric paper colored by boracic acid, is not restored to its yellow color by the acids, but that it acquires a blue tint when moistened with ammonia, and likewise, more or less, with other alkaline substances.

PRESERVATION OF METALLIC GOODS FROM CORROSION.

To the Editors of the Chemist.

August 12th, 1842.

Gentlemen,—

I have found that the best method of accomplishing this important object, is to first dip the artieles in very dilute nitric acid, and afterwards to immerse them in linseed oil, allowing the superfluity of oil to drain off; they are by this means very effectively preserved from rust or oxidation.

The prineiple of my method consists in the superior affinity *clean* metallic surfaces have for oily substances, in preference to aqueous bodies. Precisely on the self-same principle, the arts of lithography and zincography are based; the latter has received its name from sheet zinc being employed in lieu of lithographic stone, although *any* other metallic plate capable of being acted on by an acid, would answer equally well, but being easily obtained in firm thin sheets, and at a cheap rate, zinc fully answers the purposed end.

I am, Gentlemen,
Your very obedient servant,
W. J. Lander.

PREPARATION FOR COLORING FOILS OR TINSEL OF A FINE YELLOW.

To the Editors of the Chemist.

Gentlemen,—

I send you a method of preparing a compound for communicating a finc lemon-yellow to foils:—

Take a quantity of hay-saffron, heat it in an earthen vessel along with about four or five times its weight of distilled water, until the latter appears saturated with the color; and when such is the case, pour it off, and mix it with gum arabic or isinglass. With a broad fine brush, cover evenly the surface of the foil or silver-paper, when a very lively yellow is the result.

The foil, when dry, should have a thin coat of colorless spirit or other varnish applied to it, to prevent the yellow mixture from being acted on or rubbed off from the effects of wet or dampness; from the fact of this recipe being scarcely at all known, its appearance will probably be acceptable to some of your numerous readers.

I am, Gentlemen,
Your obedient servant,
T. Simpson.

FIRES ON BOARD SHIPS.

[The following letter has just been handed to us, with the view of calling forth the opinions of those qualified by professional experience to form a judgment on the matter. It has recently been submitted to the Admiralty, and their lordships have returned an answer, declining to adopt the suggestion, under the advice of a chemist of experience. It is surmised, that apprehensions exist as to the destructive effects of the gas on human life, as well as on fire. If this be the real cause of the rejection of the plan, it surely is possible by mechanical means to expel the gas, before again entering the ship's hold. At any rate, the grand point would be obtained of extinguishing the fire —though the crew might have only the *deck* to stand on. As, however, this is an effort in the eause of humanity, through the aid of chemical science, we gladly invite the remarks of such of our practical readers as may have objections, or of such others as may be inclined to advocate it. The frequency of these disasters has become distressing.]

" *To the Right Honorable the Lords Commissioners of the Admiralty of Great Britain.*

" My Lords,

" Considerations of humanity have impelled me to address your Lordships, as the high conservators of the naval interests of this kingdom, on a subject involving the safety and lives of the crews of Her Majesty's ships of war, as well as those of the merchant service; namely, the extinguishment of fires on board of vessels, at sea or elsewhere, by an efficacious application of the aid of chemical science.

" The antidotal effects of *carbonic acid gas* upon combustion are well known to every experienced chemist; and I am convinced, by practical experiments, that a simple and economical apparatus might be attached without inconvenience to every decked vessel.

" My conviction of the practicability of extinguishing such fires has been confirmed by the detailed accounts of lamentable disasters of late, and more especially by that of the 'Georgia,' in the South Atlantic Ocean. The brief recital of the circumstances respecting this vessel, forcibly impressed me with the feasibility of my plan.

" The 'Georgia' caught fire when about one thousand miles from the Cape of Good Hope, and three hundred miles from any land; the captain thus details the catastrophe:—

" 'We immediately tried to the utmost in our power to extinguish it, by throwing water down amongst that portion of the cargo, but to no purpose: I then caused all the hatchways to be closely battened down, which continued so for seven or eight hours, and the fire apparently was being smothered, when the ship 'Thomas Sparks,' came up to us; I hailed her, and requested her commander to remain by us all night, which he did. The fire was confined *beneath* her decks till the second day after the coming up of the 'Thomas Sparks,' when it broke forth and speedily consumed the vessel.'

" It thus appears, my Lords, that the fire was confined under the decks of the 'Georgia' about *fifty hours* before it burst forth; whereas the application of the gas, as proposed, would have extinguished it in less than as many minutes.

" Carbonic acid gas, is, as I have observed, a well known non-supporter of combustion, and will *extinguish fire at the very instant of coming in contact* with burning matter.

" Chalk will yield, with sulphuric acid, (vinegar, or any other acid will do,) 44 per cent. of the gas: hence, a ton of chalk, and a fourth part of that quantity of sulphuric acid, will be found sufficient to extinguish any fire on board a ship.

" *The plan is peculiarly adapted to a ship, because she can be battened down so as to exclude the atmosphere.* A small leaden gasometer is all the apparatus required, having a curved tube, and which, being portable, may be placed over the burning part, whilst a hole may be cut in the deck sufficiently large to admit the tube.

" Carbonic acid gas *expands with heat*, and being *heavier* than the atmosphere or smoke, will immediately *descend, by its own gravity*, upon the burning mass.

" As respects men-of war, room being of the utmost importance, the apparatus may be stowed away between the beams of the quarter-deck or forecastle.

" The pipes lately introduced for the purpose of ventilating ships, may be made available for the transmission of the gas in case of the whole ship being on fire; and the expense of materials as well as of the apparatus will be so trifling, as to be unworthy of consideration.

" I may further mention the utility of the apparatus in destroying vermin in ships, such as rats and cock-roaches, being more easily applied, and more effectual than the usual method.

" My Lords, should these suggestions be deemed worthy of your Lordships' serious consideration, I humbly propose that the plan may be tested by an experimental trial, which may be accomplished by setting fire to an old decked barge, capable of being battened down.

" My Lords, allow me to say in conclusion, that I feel the most perfect conviction of the ultimate success of this plan for saving many valuable lives, as well as property.

" I have the honour to be,
" My Lords,
" Your Lordships' very humble Servant,
" J. R. Hancorn, *Surgeon.*

" 34, High Street, Shoreditch,
August 3, 1842."

III. PHARMACY, &c.

ON CERTAIN ATTEMPTS WHICH HAVE BEEN RECENTLY MADE WITH THE PROFESSED OBJECT OF IMPROVING THE POSITION OF CHEMISTS AND DRUGGISTS AS A BODY,

BEING A PAPER (NO. 3) READ BEFORE THE JUNIOR DRUG CLUB, ON WEDNESDAY, AUGUST 3, 1842.

GENTLEMEN,—

In pursuance of a suggestion thrown out at a recent friendly meeting of this Junior Club, I wish to bring under your attentive consideration certain attempts which have been recently made, with the view of improving the present position of chemists and druggists as a body, and also the practice of Pharmacy in Great Britain. Our meeting tbis evening, Gentlemen, is an earnest of our desire to further sucb objects, and although it may occur to some of us, that we differ as to the means best adapted for attaining the end proposed, certainly a consideration of those already practised, can in no way impede, nay, it must facilitate and assist in the attainment of such object; for a calm and dispassionate view of our conduct, if succeeded by a determination to "hold fast by that which is good," under all circumstances of life, has ever been found an assistance to a right purpose.

The position of chemists and druggists at this moment, I think, bears some analogy to that of those persons who practised medicine previous to the year 1814. An act of parliament had then been proposed in reference to the duty on pbial bottles, which it was felt would affect medical practitioners; this caused them to unite. In 1841 we have an act of parliament proposed, which has been strongly felt would affect chemists and druggists, and whicb has rendered union desirable among them. The reflecting mind will at a glance perceive, that I cannot with advantage carry the analogy further; for whilst the apothecaries, in their prospective arrangements, granted a diploma to all who could sustain their examination, and left the profession as then constituted, "alone in its glory," the chemists and druggists propose to grant their diploma without examination, only to those who *are* in business, or who may go into business, (also without examination, if preferred,) as if the termination of a contract with the druggist's fixture dealer could not be concluded without a handsomely engraved diploma, to be a sufficient guarantee, *per se*, to the public, that one of the contracting parties is competent to conduct the business of a chemist and druggist. Unless this be remedied, Gentlemen, I fear we shall have the enterprising firm of Ancell and Hawke furnishing diplomas *on hire*, at a rate which the Pharmaceutical Society will term much below par, or, in other words, at a less sum than two guineas per annum, as arranged by this latter.

Even as the apothecary of 1814 felt, so must the conscientious chemist and druggist of 1842 feel, that his occupation involves so many grave interests to society at large, that some opposition should be offered to those who might at any time feel inclined to throw aside the plough, the paint-brush, or the bodkin, and forthwith dub themselves Pharmacists, Pharmaceutical Chemists, or plain Cbemists and Druggists: and herein are we not all agreed? In the means only of attaining this we differ; and against the means which are now in progress to this end, I beg respectfully, yet firmly, to enter my humble protest. I do not here intend to propose any chimerical scheme, which is to render us all chemists; to obtain for every assistant a good salary, or such advantages as intense application, and untiring industry alone can secure; nor to render any one present unmindful of those barriers, which custom has established for the benefit of society, between ourselves and others; but I wish to arouse reflection—to invite druggists and their assistants,—especially those engaged in the wholesale trade,—to think on what is passing around them, that we may not, at the last hour, find peculiar interests consulted to the detriment of cbemists and druggists as a body.

A Society has been formed, rules have been framed, and the machinery set in motion, for compelling every one, who has been hitherto engaged in obtaining a livelihood as a chemist and druggist, to obey these rules. The Pharmaceutical Society of Great Britain is looking askance at the legislature for power to compel obedience thereto.

As a Society, then, its proceedings are public property, may be freely canvassed, and any attacks on it may be freely noticed; you need not fear to give offence by doing that to which you are invited by themselves; whence I beg, as chiefly important, not only to us, but to our country, to observe how fares that country's fame, in reference to the practice of Pharmacy, under the auspices of the Pharmaceutical Society.

Experience has taught us that science

and commerce progress in different paths; to combine the two under present circumstances of life, is, to say the least of it, a very difficult task. Yet such, you will observe on perusing the early transactions, was the task which the Pharmaceutical Society had set itself. To perform it with ordinary success, unusual self-denial, personal sacrifice, and much public spirit is requisite. Commerce, you must have observed, is ever tending to a monopoly, whilst Science lays open her store to all who assiduously pursue her. No little tact, and, I might add, talent, is required to control such dissimilar means to the attainment of objects involving various interests. While Science would have all her associates members of the same family, Commerce draws a line between the associate and member, ay, and can even distinguish the "bright halo" which surrounds "the founder." Such distinctions, unless accompanied by reasons clearly enunciated, are apt to give rise to jealousies and heartburnings; and the question arises, whether the benefit to be derived from such an association will not be meted out with similar distinctions. This question, I regret, Gentlemen, is no longer matter of doubt in reference to "diplomatic policy."

The next reflection which has forced itself upon my consideration is, that the *Pharmaceutical Transactions* will be esteemed by many as a reflex of the practice of Pharmacy in England, and in fact the character of those enrolled in its list will be estimated therefrom. This is a conclusion to which the world and our continental neighbors are fairly entitled to hasten, unless the constitution of the Society is shown to be such, that the transactions are merely a mirror, available only for a portion of the *members*, wherein to display to themselves and admiring friends the extent of their acquirements. Feeling that the before-mentioned conclusion in reference to England's Pharmacy has already been arrived at, it has given me much pain to peruse many *ex officio* portions of the *Pharmaceutical Transactions*, and I must be allowed, in justice to the Assistants of Chemists and Druggists throughout Great Britain, to show, as well as I can, that the constitution of the Society is such as I have intimated, and not, as hastily but fairly inferred, a record of the state of Pharmacy in England.

The number of papers read at the meetings taken consecutively, until we arrive at fifty-four, if analysed, give not one by a member being a wholesale chemist:—

51 by members, honorary and otherwise,
3 by associates (or assistants);

—

54 total:

of these, nine are by Mr. Jacob Bell, three of which nine are on Adulterations,—a subject upon which, Mr. Bell will pardon me if I observe, I trust he has had very little practical experience at his shop, No. 338, Oxford-street. One sixth of these papers being the lucubrations of the amiable Editor, and one eighteenth by associates; I beg to deny, on the part of these latter, the reflection attempted to be cast on Pharmacy in England, by an advertisement headed "Citrate of Iron," and inserted in the *Lancet* of Saturday, July 2, 1842, and also on the cover of the *Pharmaceutical Journal* for July, 1842, commencing:—

"What a reflection does the Editor of the *Pharmaceutical Transactions* cast on the state of Pharmacy in England, and of his own competency to edit a journal devoted to Pharmacy, when he says he was obliged to write to a friend in Paris, to know what the salt (called by Mr. Bullock, the publisher of the advertisement now quoted, citrate of iron) should be called."

I may here mention that a statement in reference to the ferri potassio tartras, as it is termed in the *Pharmacopœia*, which appeared in another advertisement inserted in the *Medical Gazette*, May 6th, 1842, in which it was stated by Mr. Bullock, "that the precise part which ammonia acts in that preparation, was far from obvious," has been the subject of remark in the August number of THE CHEMIST, page 248. Having endeavored to ward off the blow attempted to be struck against England's Pharmacy, chiefly because the Journal supported for that purpose has neglected to do so, I will occupy but a few moments more of your time, in reference to the attacks which have at various times appeared in the Journal, maintained with the presumed view of promoting the interests of the wholesale in common with those of the retail chemists and druggists; and, Gentlemen, let me now ask you, is that the most honorable path to distinction, which is paved with the detraction of your brethren in trade? Aid me, at least with your approval, and the cause of the wholesale druggist, which is emphatically the cause also of the mass of retail druggists, in discountenancing the senseless hue and cry of, "The public is in danger from adulterations,"—a cry which causes your customers not to run to the shop of the purblind man, him who raises it, but to avoid physic altogether. If Mr. Mackay had witnessed the incredulous shrugs with which his paper on Cantharides and Glass Beads (see July No. *Transactions*, 1842, page 12,) was received, and Mr. Bell could understand Mr. Payne's observation in reference to a strong term,

(see *Transactions*, Aug. No., 1842, page 65), tbis subject need not have been mentioned; but the former was not present on the occasion alluded to, and the latter does not appear even to have remembered what was observed by Mr. Herring when the subject of a former compilation by Mr. Jacob Bell was discussed. Mr. Herring's remarks were introduced with—

"That with every respect for the talents of Dr. Christison, yet that he and others who were not practically acquainted with the subject, had been misled by exaggerated statements."—(See *Transactions*, No. 6, Dec., page 263.)

In conclusion, Gentlemen, my views as an assistant, in reference to what is passing around us, may be summed up in a few words.

Although an associate of the Pharmaceutical Society, yet I cannot, from the laws and constitution as adopted by the Council, communicate any objections, grievances, or other matter relating to their rules,—a privilege, be it remembered, which, in reference to the House of Commons, is open to me.

That, although to an associate by their laws the Pharmaceutical Society profess to grant all privileges, except holding any office or being present at a general meeting, yet the possession of a diploma is denied to associates; conduct, so inconsistent with their professions, deserves the severest reprehension, as being a glaring breach of faith.

That, under tbe present arrangements of the Pharmaceutical Society, their diploma must be considered as a picture, the cost of which has been paid by members and associates mutually; every one who seeks to possess the same must be provided with a shop wherein to frame it, and as regards the respectability it will confer, after ten years, if any one will ask Mr. Jacob Bell, he will be able to give a more definite and satisfactory answer tban he can at present. (*Vide Transactions*, for January, page 358.)

That if the object of the Pharmaceutical Society be, as professed, to give tbat encouragement to Pharmacy in England, which its importance deserves, such Society requires chiefly for its own stability, important modifications; and if professions have been held out merely *ad captandum*,—a conclusion I avoid,—public opinion is sufficiently strong to compel them to alter their laws, so as to admit that branch of the Drug family, whom they at present treat as illegitimate; and that, if we feel disposed to represent our case in temperate terms, at the proper quarter, we shall find our application treated with every consideration.

DRUGGISTS AND THEIR ASSISTANTS.

To the Editors of The Chemist.

London, Aug. 1842.

GENTLEMEN,—

Should you have sufficient space, I should feel obliged by the insertion of the accompanying remarks in tbe pages of your valuable periodical.

The letter of your correspondent (in your August Number) who has not *inappropriately* designated himself "PESTLE," is so extremely *ultra* in its statements, and furnishes so strange a specimen of utter want of gentlemanly feeling and liberal spirit, tbat I cannot permit its strictures on a numerous class of respectable young men to pass unnoticed or unanswered.

If it indeed be true, that there exists among the young men offering themselves, and acting as assistants to the druggists of this country, so deplorable a state of mental darkness, and "apathy to tbe interests of their employers," I would ask, in what light do those men stand, who, from the ages of fourteen or fifteen, had been *entrusted* with the charge of affording them a thorough practical knowledge of their business? To whom under such circumstances does the greater blame attach?—to the young man, nay, to the *boy*, who is placed under such person, voluntarily undertaking for the amount of an agreed upon premium, and in the space of *five years* to initiate him *thoroughly* into the mysteries of his future calling, and at the expiration of that period, turns him off; and, to use the language of your communicant, as great a "dunderpate" as when he first began to handle his "*pestle*," or to clean his master's *windows?* Or to that master himself, who has thus broken his engagement, and is, in fact, too frequently found positively incapable of acting up to it?

Your correspondent, Gentlemen, would doubtless affirm, that ample as are the opportunities afforded to such young men, it is tbeir own fault if tbey do not progress or improve themselves; and I would here avow, tbat the remarks of your "London Assistant" are most applicable; they are, in the majority of cases, neither more nor less than the "white slaves of England;" in proof of which, Sir, I, too, could furnish Mr. "PESTLE," with such "DAMNING" PROOFS of the utter ignorance of the *masters*, their positive dislike to, and attempts to prevent any scientific or literary

subject connected with their profession, being introduced on the *tapis*, either by their apprentices or their assistants, their frequently brutal treatment of the former, as would, I can scarcely flatter myself, *astonish* "Mr. Pestle;" but, at least, teach him to somewhat "calm the hysteric blast," and to confine within decent limits his illiberal censures upon a respectable class of the community. Far be it from my purpose to assert that there are none who act an honorable and a liberal part. I feel a pride and a pleasure in stating my belief to the contrary. I feel a pride in the knowledge of those gentlemen, whose ambition it is not only to improve tbose for whose after education they are responsible, not only in the "manual dexterity" attached to their business, but in cultivating their mental faculties, and bringing before them subjects calculated to make them intelligent as well as respectable members of society; and I am enabled to speak with confidence as to the fact of those Master Druggists being notorious for turning out industrious, intelligent, and practically informed young men.

It appears evident, Gentlemen, that your Correspondent has little thought beyond manual labor, and the accumulation of pounds shillings and pence in the till; and will, therefore, utterly contemn any "legislative proceeding" which might tend to make those who, although not possessed of minds and sympatbies in common with his own, are yet deserving of such interference, and whose annual stipend of the munificent sum of *twenty pounds*, is really not too much to enable them to stand behind the counter with respectability to themselves, or credit to their employers.

In conclusion, I would briefly ask what additional guarantee of improvement (*if any* at present they possess) is offered to the public for the lengthened period of apprenticesbip so *ably* advocated! At another period I shall return to this subject, and, with much respect for your scientific labors, and desire to forward the study of Chemistry, I remain,

Gentlemen, faithfully yours,
A Reader of "The Chemist."

REMARKS ON PHARMACEUTICAL NOMENCLATURE.

BY THOMAS HOLT, M.D., EDINBURGH; M.R.C.S. LONDON AND EDINBUR GH, &c.

[The following is from *The Lancet* of June 18; the importance of the subject speaks for itself, and we wish to see the matter more generally taken up.]

It is greatly to be regretted that the London College of Physicians should have been so hasty in breaking off the agreement with the Edinburgh and Dublin Colleges for the publication of a general *Pharmacopœia* for the whole United Kingdom, by which we should have been in some measure relieved from the confusion arising from the different processes adopted in the preparation; also from the inconvenience and danger attending the whims of a College, in changing and giving what names it pleases to the different medicines in general use.

In the last edition of their *Pharmacopœia*, the London College has endeavoured to follow chemists in giving the names expressive of their composition to the chemical substances, by which many important articles have had their names altered, causing great danger to be apprehended from prescriptions; the Edinburgh College, however, has made a great advance (to use an Hibernicism) in going back to the old and generally used names. "We believe there are few physicians, and not many chemists, who entertain any doubt that the Colleges committed a great error when they were first seduced by the philosophical attractions of modern chemical nomenclature, to abandon, for the terms of scientific chemistry, the trite names formerly used in pharmacy and medical practice. The more decorous dress of science or philosophy has been dearly purchased at the cost of being compelled to follow the changing fashion of the day. We apprehend that practitioners will not submit much longer to the constant fluctuations which have been for some time forced upon them in pharmaceutic nomenclature."—(*Ph. Edin.*)

It would be well to adopt the scientific names indicating the constitution of substances, providing that chemists were agreed in adopting an uniform system of nomenclature; and also if the atomic weights could be certainly and positively decided: but such a state of perfection is not likely ever to be attained; and it will be endeavored to consider the composition of some of the more important pharmacopœical substances, for the purpose of showing that the names adopted by the London College are, in many instances, liable to objection.

The mercurial salts, and especially the chlorides, have been brought forward to show the advantages of the terms chloride and bichloride of mercury, given in the *Pharm. Lond.*, as expressing at once the composition of calomel and corrosive sublimate: there is no doubt that calomel contains eighty-five parts of mercury and fifteen of chlorine, or thereabouts, per cent.; but whether to one equivalent of chlorine it contains one or two equivalents of mercury, is not agreed upon. The College takes the equivalent of mercury at 202; while others,

including our highest chemical authorities (Professors Graham and Kane), take 101 as more prohable; and if the College were to follow the same rules of nomenclature in the next edition of the *Pharmacopœia*, we should then have chloride of mercury representing corrosive sublimate; and calomel, heing a subchloride, would become dichloride of mercury: so that prescriptions containing hloride of mercury, heing made up in accordance with one edition of the *Pharmacopœia*, would give a most corrosive and virulent poison, and according to the other edition a comparatively mild and harmless medicine. And the altcration of names would not rest with calomel and corrosive sublimate; but the oxides, iodides, cyanides, sulphurets, —indeed, the whole of the mercurial salts, —would undergo a corresponding change, for instance, the red oxide (the present binoxide) would he named hydr. oxydum; and the black oxide, now called hydrarg. oxydum, would become a dinoxide; and the salts of mercury are so frequently prescribed, calomel especially, that it is impossible to conceive the mischief (too often fatal) which would ensue.

The equivalents of arsenic and antimony are also doubtful; the College taking 64·6 for antimony, and 37·6 for arsenic; whereas Graham (*chemicorum princeps*) doubles the numbers; which of course will alter the names of such compounds as sesquisulphuret of arsenic, sesquioxide of antimony, (*Ph. L.*) &c.

The ammoniacal salts may be variously considered as to their constitution; *e. gr.*, sal ammoniac is viewed by the College, and named hydrochlorate of ammonia. Graham, Liebig, &c., adopting the most prohable view, viz., that of Berzelius, from its various analogies, consider it to be a chloride of the compound hypothetical basyle-ammonium; and I recollect Dr. Kane taking considerable pains to show that it was neither one nor the other, but a kind of double salt, composed of the amide with the chloride of hydrogen. The London College has applied the term "sesquicarbonate of ammonia" to the substance named hy the Edinburgh College "carbonate of ammonia;" but Graham's opinion appears to be, that it is nota direct sesquicarbonate, but a compound of the bicarbonate with the neutral carhonate.

The carbonate of soda of the former *Lond. Pharm.* is now named hy the same body "sesquicarbonate;" whereas the Edinburgh College, Graham, &c. consider it to be a "bicarbonate." Dr. Christison says it is not, and never ought to be, a sesquicarbonate of soda.

The cream of tartar, named in the *Pharmacopœias* "bitartrate of potash," consists of only *one equivalent of tartaric acid*, united with one equivalent of potash and one of water; and the tartrate (neutral tartrate), having hoth equivalents of water of the acid replaced by corresponding quantities of potash, consists of one atom of tartaric acid and two of potash.

The bleaching substance has been named calx chlorinatum, and its constitution has heen very variously stated. Dr. Ure considers the elements not to he in regular atomic and definite combination, but that it contains a variable proportion (from 1 to 40 per cent.)of chlorine; Dalton, that it is a hydrated subchloride: by Berzelius it is thought to consist of chloride of lime with chloride of calcium. Thomson appears undecided which of the two last to adopt; in his *System of Chemistry*, and in *The Annals of Philosophy* (1820), inclining to the former, and in his article in the *Encyclopædia Britannica* to thelatter view. Balerd considers it hypochlorite of lime with chloride of calcium: Millor, however, offers the most satisfactory view, viz., that it is a kind of oxychloride of calcium, —the second equivalent of the peroxide of calcium being replaced by chlorine, analogous to the oxychloride of hydrogen, or hydrate of chlorine, &c.; and holds the same opinion regarding the corresponding compounds of sodium, potassium, &c.: thus its old and every-day name, chloride of lime (chloride of oxide of calcium), represents its composition as well as any, and better than most, of the others.

By the German and French chemists, who deduce the equivalents from their volumes, the atomic weights of chlorine, iodine, bromine, fluorine, hydrogen, and nitrogen, are only half as high as those used by the British chemists; consequently such compounds as hydrochloric acid, &c. will also he only half as high, and water, considered here as the neutral oxide (HO), is represented on the Continent as a suboxide, or two atoms of hydrogen to one of oxygen (H_2O); and therefore Thénard's oxygenated water is looked upon hy the Continentals as containing equal atoms of oxygen and hydrogen: whereas in Britain it is considered as a peroxide of hydrogen, containing two equivalents of oxygen to one of hydrogen. For the like reasons, common salt, being in this country *generally** considered as chloride of

* Professor Clark, of Aberdeen, inclines to double the atomic weight of sodium, making it 46·6, instead of 23·3, in which case soda would become a peroxide or binoxide; common salt, a bichloride, &c. On the other hand, Johnston, of Durham, from its isomorphous relations to silver (whose

sodium (atom to atom), is on the Continent represented as bichloride of sodium—two atoms of chlorine to one of sodium (Na Cl_2).

In the formation of the "potassii sulphuretum" the product appears to vary with the temperature applied, perhaps generally containing sulphate and undecomposed carbonate of potash, and the ter and quinto-sulphurets of potassium. Might not the quinto or penta-sulphuret of potassium be considered as the analogue of sulphate of potash? The formula for sulphate of potash is KO, SO_3; and suppose sulphur to be substituted for oxygen (as it is frequently), we should then have $K\,S + 4S = K + 5\,S$, or pentasulphuret of potassium.

For the sake of uniformity the College uses the term chloride of barium, but it might be considered as a hydrochlorate or muriate of baryta, as viewed by the Edinburgh College, being either muriate of baryta with one atom of water, or chloride of barium with two atoms of water. It is right to add, that the latter is generally received as the more probable among chemists, though Dr. Hope, of Edinburgh, still clings to the former view. Graham, also, in his lectures, proposed to view the salts of the alkalis in the same manner: for instance, he considers that hydrochlorate of morphia may be "chloride of morphyle."

However beautiful and striking the new views in chemistry may be, it does not appear advisable to keep altering or changing the names of important and dangerous medicines, which appears to have been done not infrequently for the sole purpose of appearing scientific. Even Graham himself only wishes *to prove the new theory of salts*, without recommending the consequent changes of nomenclature for chemistry itself. How much more unadvisable is it that the names should be changed in medical practice, when on the prescription may depend the very life of the patient? for before a new catalogue of names has come into general use, or has been published half a dozen years, it might be shown that half, at least, are not in unison with our highest authorities in chemistry: and there is some reason to doubt not a few of the remainder! What real objection can there be to the use of such terms (medically) as calomel, tartar emetic, arsenical solution, red oxide of mercury, black oxide of mercury, corrosive sublimate, &c., when by their use so much greater security is afforded against mistakes, too often irremediable?

Bury, Lancashire, June, 1842.

atomic weight he would also divide, and consider 54 instead of 108), is inclined to halve the atomic weight of sodium, making in that case soda a suboxide, common salt a subchloride, &c.; the same also with potassium.

POISONING BY BINOXALATE OF POTASSA.

On the 26th of November, 1841, the wife of a captain in the French army, who had recently been confined, sent, by order of a physician, for thirty grammes of tartrate of potassa, to a chemist in Algiers. The chemist, by a fatal mistake, of which one of his assistants was the first cause, sent thirty grammes of binoxalate of potassa divided into two equal packets. One of these was dissolved in lukewarm water, and taken by the lady, notwithstanding the disagreeable taste which she perceived. Scarcely was it swallowed, when the patient was seized with violent pains in the stomach, and soon after was affected by horrible convulsions, and died in less than ten minutes. She was able to articulate only the words: "It burns me, I am killed; I am poisoned!" It was impossible to administer any antidote.

On opening the body, twenty-two hours after death, incipient putrefaction of the lower parts of the abdomen was discovered. The orifices of the stomach were hermetically closed, in consequence of the contraction of the fleshy bundles which surround them. The mucous membrane of the stomach was much inflamed; the duodenum and small intestines presented the same alteration. The blood in the heart and vessels was fluid.

To this deplorable fact may be added an observation of a case of poisoning by this same substance, in a woman of the same age, reported in the March No. of the *Journal de Pharmacie*. In this case, the dose was double, the woman having swallowed, at one time, thirty grammes of this salt dissolved in water. She experienced serious symptoms only at the end of an hour and a half, and the case was successfully treated by administering first a mixture of chalk, and afterwards antispasmodics.

What a difference in the termination of these two cases, which are presented in the same circumstances, except that the person who was cured had taken twice as much poison as she who died almost immediately! Every thing shows that in the latter the poison had been rapidly absorbed: but why was not the same phenomenon presented in the other patient? What were the essentially different conditions which caused so striking a variation in the issue of the two cases? The observa-

tions just quoted, unfortunately are deficient in a host of details which would be indispensable for solving this problem.

But it is no less evident, that the salt of sorrel (*sel d'oseille*) acts on the nervous system with great energy, and that, swallowed by mistake, even in a small dose, it may cause death. The following case, taken from the April No. of the *Annales D'Hygiène*, is remarkable in this respect :—

A woman, aged 28, wishing to make her milk pass away, procured some salt, of which she was recommended to take a teaspoonful every morning. The first day, soon after having swallowed the prescribed medicine, she was attacked with very abundant vomiting, which the "mothers" of the neighborhood did not fail to attribute to the revulsion of the milk. The next day, after a second dose, the symptoms became more alarming; the vomitings were bloody, blackish and more abundant than the day before; there was great pain in the epigastric region. The third day, at 5 o'clock in the morning, the woman took another dose of the salt; she lost her reason, and became insane: vomiting supervened, and death occurred so promptly, that the medical man called in found her dead. She died at 6 o'clock in the morning.

The salt which the deceased had swallowed was examined, and it was ascertained to be binoxalate of potassa. The teaspoon which she had used would hold about 5 grammes, consequently she had taken altogether 15 grammes.

The matters vomited before death having been analysed, it was ascertained that they contained a large proportion of salt of sorrel, for about 2 grammes of perfectly crystallised oxalic acid were extracted from them.

POISONING BY LAUDANUM.*

A YOUNG writer, M. CAMILLE B., whose *débuts* at the *Comédie Française* had been remarked, has lately perished in consequence of a fatal imprudence. Being attacked with a slight indisposition, M. C. B. was directed by his father, who is a physician, to apply a cataplasm, into which a few drops of laudanum were to be poured, to the stomach. To alleviate his pain, which was very severe, he poured, instead of three or four drops, the whole contents of the bottle into the cataplasm, and went to sleep thus.

The poisoning was almost instantaneous. The most prompt assistance which was rendered him was in vain; he died in a few minutes.

* *Journal de Chimie Médicale*, August, 1842.

SYMPTOMS CAUSED BY CANTHARIDES.*

AN attempt to poison was lately made at Evran (Ille et Vilaine), on two young girls, one aged 22, the other 14, servants to an innkeeper of that town. A tinker named N., who occupied a room near that of the two girls, mixed with their food some powder of cantharides, doubtless hoping to accomplish some abominable design, aided by the properties attributed to this powder; but the violent colics which supervened, soon disclosed the culpable project of N., who has been committed to the prison of Dinan.

MELTING BAG.

℞ Iodide of potassium . . . . 10 grammes.
Hydrochlorate of ammonia 80 ,,

Let these salts be well dried and separately pulverised; mix them by trituration, and enclose them in a small linen bag.

This preparation is recommended by Dr. Breslau, physician to the King of Bavaria, as one of the most simple and efficacious means that can be adopted for the procuring the resolution of wens and indolent tumors. This practitioner, who has often used it with great advantage in his own practice, applies the bag to the diseased parts, and allows it to remain there for a long time.

SCHIRRUS OF THE UTERUS CURED BY THE EXTERNAL AND INTERNAL EXHIBITION OF IODINE.†

BY DR. ZIMMERMANN.

A LADY, aged 45 years, was affected about a year ago, with a schirrous induration of the cervix uteri, to which was added a hectic fever. Dr. Zimmermann, who was called in to attend her, prescribed for her the following mixture :—

℞ Iodide of potassium . 1·0 grammes.
Iodine . . . . . 0·4 ,,
Distilled water . . 30·0 ,,

M. and dissolve.

She commenced by taking eight drops, three times a day; then the dose was gradually increased, and with great caution, until it amounted to fifteen or eighteen drops.

At the same time, the physician ordered the

* *Journal de Chimie Médicale*, August, 1842.

† *Journal de Pharmacie*, June, 1842.

topical employment of the following ointment :—

℞ Lard . . . . . 30 grammes.
Iodide of potassium . . 2 ,,
Volatile oil of rosemary . 6 drops.
M. S. A.

This ointment was applied three times a day; sometimes to the perineum, sometimes to the inguinal regions, and sometimes, but with the greatest circumspection, to the schirrous part of the cervix uteri; in the latter case, a little while after the application had been made, mucilaginous narcotic injections were made into the vagina.

After four months of this treatment, sustained without any interruption, the patient was completely cured.

POWDER OF CAMPHOR AND ANTIMONY.

℞ Powdered camphor . . . . 2 grammes.
Powdered ipecacuanha . . 65 centigrms.
Golden sulphur of antimony 65 ditto.
White sugar . . . . . 24 grammes.
M. and F. S. A. a perfectly homogeneous powder; divide into 12 doses.

This formula is due to Dr. Mursinna; it is employed with marked advantage in cases of asthenic pneumonia, in chronic pulmonary catarrhal affections, when the *bronchiæ* are choked with a large quantity of thick and viscid mucus, the expectoration of which is very difficult.

One dose is taken every two hours, either by mixing it with a small quantity of an appropriate liquid, or by enclosing it in a piece of unleavened bread, slightly moistened with water.

ON THE LACTATE AND VALERIANATE OF QUININE.*

Prince Lucien Buonaparte, after having caused some lactate of quinine, some valerianate of the same base, and some phloridzine to be prepared, had these substances administered by several physicians, in various febrile diseases, especially those of an intermittent character, and in the quartan ague, which prevailed in the neighbourhood of Rouen.

From what has been observed, the lactate of quinine produces good effects in the cases in which the action of sulphate of quinine is too violent and too powerfully felt by the organs; its prompt solubility facilitates its assimilation.

The valerianate of quinine is very soluble in water, less disagreeable to the taste, less bitter, and less expensive than the lactate and sulphate, and the use of which the author thinks should be propagated, especially in country places.

Phloridzine is a neutral and soluble body which has been administered in cases of intermittent fever, with as much advantage as the lactate and valerianate of quinine. According to Prince Lucien, phloridzine is recommended by the facility of its preparation and by its cheapness.

* *Journal de Chimie Médicale*, August, 1842.

EMPLOYMENT OF BELLADONNA IN PHTHYSIS.*

Dr. Delhaye has derived much advantage in practice, from the employment of belladonna in the early stages of pulmonary tubercular phthysis, and against those prolonged nervous coughs which are the precursors of consumption. He administers the powdered root of this plant in fractioned doses of from 25 to 50 millegrammes, in the course of twenty-four hours. In the case of irritability of the stomach, he prefers the extract or tincture of belladonna: the first of these two substances is prescribed in the same dose as the powder; the second, in doses of 20 to 30 drops, but always fractioned.

According to Dr. Delhaye, this tincture is also one of the best palliatives to which we can have recourse, to moderate the colliquative diarrhœa which so often cuts short the life of phthysical subjects.

He considers it indispensable to the success of this treatment, that it should be adopted only when the stomach is healthy, and, according to him, gastro-enteritis is a formal contra-indication of its employment, the existence of which is frequently the first cause of the fatal termination to chronic diseases of the chest.

REMEDY FOR TOOTH-ACH.†

℞ Finely powdered alum . . . 2 parts.
Nitric ether . . . . . 7 ,,

It is mixed as quickly as possible, in such a manner as to obtain a semi-fluid mixture, which is applied to the tooth affected.

HYDRAGOGUE PILLS IN ASCITIS.‡

BY M. DUBOURGES.

No. 1.

℞ Aleppo scammony . . { 5 gram. / 21 centigr.
Sublimed calomel . . 4 gram.

* *Archives de la Médecine Belge*, 1841.
† *Journal de Chimie Médicale*, July, 1842.
‡ *Ibid.*

F. S. A. by means of an extract, or gum and syrup, 30 pills.

No. 2.

℞ Aleppo scammony	. .	8 gram.
Sublimed calomel	. .	8 ,,

F. S. A. 36 pills.

PHLORIDZINE, A NEW MEDICINE.*

Phloridzine is a new medicine, which is now very highly spoken of by French practitioners, as a useful adjunct to cinchona preparations, and has been used for some years in Germany, Poland, and France. It is extracted from the bark of the roots of the apple and wild cherry trees, and is thus prepared:—The bark of the recent roots is boiled with water sufficient to cover them, for half an hour. This is poured off, and the same quantity is again used; these two fluids are mixed together, and at the end of 6 hours deposit the phloridzine, in the form of a deep red velvety-looking matter. M. Lebandy, editor of the *Journal de Connaissances Médico-Chirurgicales*, says, "Its efficiency is so decided, that we cannot hesitate to class it with the most powerful febrifuges; and it has this advantage over quinine, that it never induces gastralgia."

EMMENAGOGUE SOLUTION OF CHLORO-AURATE OF AMMONIA.

BY M. BOUCHARDAT.†

M. Bouchardat recommends the following as an emmenagogue:—

Chloro-aurate of ammonia	.	10 grains.
Distilled water	. . . . .	300 scru.
Alcohol (30°)	. . . . .	300 scru.

Make a solution, to be preserved in a well-stopped bottle.

This preparation is used with much advantage in cases of amenorrhœa and dismenorrhœa, arising from debility, and is far superior to other preparations of gold, such as, for instance, the cyanuret of that metal. It is given morning and evening in the dose of a tea-spoonful, in a cupful of pure water duly sweetened. The chloro-aurate of ammonia is obtained by dissolving one part by weight of the perchlorate of ammonia in a sufficient quantity of distilled water, aiding the solution by the addition of a few drops of nitro-muriatic acid (aqua regia). The compound salt must be afterwards dried by a gentle heat.

* *Braithwaite's Retrospect*, No. 5, Jan. to June, 1842, p. 119.

† *Annuaire de Thérapeutique*, as quoted in *Prov. Med. and Surg. Journal*, July 2, 1842.

PRESERVATION OF NITRATE OF SILVER.*

BY M. DUMERIL.

M. Dumeril has for a long time employed a very simple process for preserving the nitrate of silver from the injurious effects of exposure to the air when run into sticks. It consists in merely coating the caustic with engraver's sealing-wax, which contains a large quantity of shellac. This wax adheres very well, and forms a strong and smooth varnish, as it were, which remains unaffected by the atmosphere. Thus protected, the nitrate no longer stains the fingers, injures the caustic case, nor is in any way changed by the moisture in the air; possesses a greater degree of solidity, and at the same time the process is of exceeding service in practice, inasmuch as when wanted for use, a small part only of the caustic need be uncovered by means of a penknife, so that its application can be restricted to the part where it is required. This is of peculiar utility in ulceration of the throat, apthæ, fissure, &c.

BOOKS RECEIVED.

Elements of Chemical Analysis, Inorganic and Organic. By Edward Andrew Parnell, Chemical Assistant in University College, London. 1842. pp. 309. London: Taylor and Walton, 28, Upper Gower Street.

Elements of Electro-Metallurgy; or the Art of working in Metals by the Galvanic Fluid. By Alfred Smee, F.R.S. Part IV.

The London and Edinburgh Monthly Journal of Medical Science. Edited by John Rose Cormack, M.D. August, 1842. London: John Churchill, Princes Street, Soho. Edinburgh: Maclachlan and Stewart.

TO CORRESPONDENTS.

M. P. M. will find many articles on the subject, in Vols. I. and II. of The Chemist.

Nota Bene.—*All Communications and Books for Review must be addressed, "To the Editors of the Chemist, Care of Mr. Hastings, Publisher, 13, Carey Street, Lincoln's-inn Fields, London." Communications must be prepaid, and sent before the 15th of each Month; Books for Review before the 10th.*

* *Annuaire de Thérapeutique*, as quoted in *Prov. Med. and Surg. Journal*, July 2, 1842.

THE CHEMIST.

I. CHEMISTRY.

INVESTIGATIONS CONCERNING THE COMPOSITION OF WATER.*

BY M. DUMAS.†

I HAVE communicated to the Academy the experiments undertaken with M. Stass, fixing the composition of carbonic acid; M. Boussingault and myself have published analyses which establish the composition of the air on a firm basis; I am now going to make known the result of investigations which I have pursued without relaxation, relative to the true composition of water and ammonia, which will complete the determinations necessary to the speculations of general physiology.

Water is formed of oxygen and hydrogen, and it has been tried to define the exact proportion in which these gases combine, by measuring them, or by weighing them. These two methods put in practice by Berzelius and Dulong having conducted them to exactly the same result, it has been admitted, without dispute, to be correct.

I shall show that this fortuitous coincidence arises from a twofold error, a knowledge of which had with difficulty escaped the observation of chemists, had they not been so long in the habit of admitting, without discussion, the atomic weights adopted by Berzelius.

From my investigations it results that water is formed, by weight, of 1000 parts of hydrogen, and 8000 of oxygen; that is to say, that these bodies combine in the simple proportion of 1 to 8.

Berzelius and Dulong have admitted nearly the same proportion, for they consider water to be formed of 1000 parts of hydrogen to 8008 of oxygen. If this cipher truly expressed the result of their experiments, we must regard as insignificant the correction which I now propose, and, as useless, the long, expensive and tedious series of investigations to which I have devoted myself.

But on referring to the experiments even of my illustrious predecessors, it is found that they are based on erroneous determinations of the densities of hydrogen and oxygen; for it is well proved, now, that the density of oxygen is not represented by 1·1026, and I shall show that that of hydrogen cannot be expressed by the numbers 0·0688 or 0·0687, between which Berzelius and Dulong hesitated.

Indeed, when we supposed that hydrogen and oxygen combine in the exact proportion of 2 to 1 in volumes, and when we tried to deduce from it the composition by weight of the water, according to the density of these two gases, setting out from the density of hydrogen established by Berzelius and Dulong, and from the density of oxygen established by M. Boussingault and myself, we did not find the proportion of 1000 to 8000, nor that of 1000 to 8008, but, indeed, the proportion of 1000 to 8040, which, evidently, is inadmissible. However, as every thing tends to show that hydrogen does not sensibly differ from oxygen in the manner in which these two gases act under different pressures, and as their coefficient of dilatation cannot exert any appreciable influence on the respect under consideration, Dulong's density of hydrogen must be incorrect, or Gay-Lussac's law relative to the combination of gases must be only an approximation to accuracy.

At all events, therefore, it was indispensable to verify the density of hydrogen, and nothing could be concluded, as to the exact composition of water, from the densities of the gas hitherto known.

* *Comptes Rendus*, No. 15, Avril 11, 1842.

† These investigations were to have been executed in concert with M. Stass; but his nomination to the chair of chemistry in the Polytechnic School of Brussels prevented him from assisting in any other experiments than those which we regarded as preparatory. I ought, therefore, to take on myself all the faults committed in the execution of this work.—J. DUMAS.

But Berzelius deduced the composition of water from a more direct experiment. He reduced oxide of copper by means of hydrogen, and, collecting the water formed by a known quantity of oxygen, he thus arrived at the composition of the water.

Berzelius made three experiments of this kind, the mean of which gave for 1000 of hydrogen, 8008 of oxygen. Dr. Prout had already given his opinion that water might contain 1000 of hydrogen to 8000 of oxygen, but the result of these experiments was set up in opposition to him, as showing that such proportions were mere *jeux d'esprit*, unworthy of consideration.

To show the extent to which men of science have allowed themselves to be influenced by undue confidence in the mode of proceeding in determinations of this nature, it is sufficient to compare the following ciphers resulting from the three experiments made by Berzelius:—

1st Experiment . .	1000 hydrogen	=	8042 oxygen.
2nd ,, . .	1000 hydrogen	=	7936 oxygen.
3rd ,, . .	1000 hydrogen	=	8053 oxygen.
			24031
Mean . . .			8010

From having found the numbers 805, 804, and 793, the mean of which is 801, certainly nothing warrants the conclusion that the true number was not 800. It is impossible to answer to $\frac{1}{800}$, when the experiments differ from each other to the extent of $\frac{12}{800}$, and it is not with experiments whose numbers deviate to $\frac{1}{66}$ that we are authorised to repel this correction of $\frac{1}{800}$, which is sufficient for making the mean coincide with the views of Dr. Prout.

We do not hesitate to say, that, hitherto, Prout's views have not met with that sincere and profound discussion which their high importance deserves. I do not know that these views are correct to their fullest extent, but, to ascertain it, it will be necessary to revise the determination of atomic weights on a large scale, by means based on numerous experiments, and neglecting none of the corrections taught by physical science.

If these corrections had been introduced into the experiment of Berzelius, the results, already so far from the admitted mean, would have been still farther from it.

The first correction to be made in the crude result of the experiment, consisted in bringing into a vacuum the weight of the water formed in order to obtain the absolute weight. This correction does not amount to less than from 10 to 12 milligrammes in the weight of hydrogen, in experiments in which it was thought that precision to 1 milligramme might have been reckoned on.

For the same reason the weight of the oxygen employed must also be brought *in vacuo*.

On the other hand, the desiccation of hydrogen requires careful precautions of a different nature from those used by Berzelius. He supposed that a stream of gas arrives at absolute dryness by passing rapidly through a tube filled with chloride of calcium. Experiment and reasoning prove that it is not so. Now, the gas which disappeared, being converted into water, presented to the vapor a space, which, in ceasing to exist, determined its condensation. Thus, all the hygrometric water of the gas consumed is added to the water arising from the experiment when the hydrogen burned is not dry.

Finally, supposing the weights reduced *in vacuo*, and the gases perfectly dry, the experiments of Berzelius still admit of much doubt concerning the real composition of water, having been made with only 10 or 12 grammes.

These experiments, then, were too few in number; they were made on too small a scale; they were not subjected to indispensable corrections, which would completely alter the ciphers deduced from them. All these circumstances determined me to revise them.

My first care consisted in procuring perfectly pure hydrogen. For this purpose, I thought I could not do better than employ the very simple means which I have generally seen approved and adopted by the chemists who, for a long time, have been able to take cognisance of my experiments.*

The impurities of the hydrogen obtained by means of zinc, water and sulphuric acid, may consist of oxides of nitrogen, sulphurous acid, arseniuretted hydrogen and sulphuretted hydrogen. The oxides of nitrogen arise from impure sulphuric acid; its purity must always be ascertained before it is used. Sulphurous acid is sometimes found in sulphuric acid purified from

* The processes which I employed for the synthesis of water, were presented in my course at the *Ecole de Médecine*, last year.

nitrous combinations by a current of sulphurous acid. If retained by the hydrogen, it would pass off with it and would cause serious errors. Arseniuretted and sulphuretted hydrogen almost always occur in these experiments, especially the latter.

It is therefore necessary to make use of pure sulphuric acid, and to drive the gas through reagents capable of removing from it the traces of sulphuretted or arseniuretted hydrogen which it contains. A solution of nitrate of lead stops the sulphuretted hydrogen, and a solution of sulphate of silver takes up the arseniuretted hydrogen. I place these solutions in U tubes filled with pieces of glass, which give to the liquid a developement of surface suitable to the action which they are destined to exercise.

Ordinarily, at the end of the experiment, in tubes of nearly a metre in length, the colored part forms a zone, which scarcely exceeds three or four centimetres.

The gas afterwards passes through similar tubes filled with pumice stone moistened with a concentrated solution of potassa; then into a tube containing ordinary potassa in pieces; and lastly, into another containing caustic potassa, which has been heated to redness.

Hydrogen which has undergone these purifications is perfectly inodorous. I have often disengaged a hundred litres without perceiving the slightest odor.

But this gas is not yet dry, and I employ for drying it sometimes concentrated sulphuric acid, and sometimes anhydrous phosphoric acid. Sulphuric acid answers very well in winter, or when care is taken to maintain the drying tubes at zero, by surrounding them with ice. But I have often employed anhydrous phosphoric acid. In this case, I divide it by means of large fragments of pumice stone.

The pure and dry hydrogen is lost for several hours, in order to remove all the air from the apparatus.

The oxide of copper was placed in a flask of very hard glass, in which it was subjected to a red heat for a whole day, without the flask being altered in form, or even in the lustre of its surface. To heat it, I used spirit lamps with a double current of air, of a new construction, in which I kept the alcohol at a low temperature by means of an envelope of water.

The flasks which I used in these experiments were furnished to me by the Baron Von Klinglin, who, at his fine glass works in the plains of Valsch and Valeristhal, obtains all the objects in hard glass which chemists can require. These were globes or bulbs, with two necks—a short one through which the hydrogen passes in, and a much longer one by which the excess of gas and the water formed are disengaged. The singular difficulties which are presented in the fabrication of these globes have caused us a thousand contrarieties, but they are at length surmounted.

We have, indeed, flasks sufficiently burned to resist all the changes of temperature, sufficiently hard to support a prolonged red heat without losing their glaze, and furnished with a point of a metre in length, in which are operated the cooling and condensation of the aqueous vapor formed.

The oxide of copper being introduced into the flask, a stop-cock was adjusted to the little neck, and the opposite side was closed by means of a small piece of caoutchouc. After having ascertained that the arrangement kept the vacuum, a current of air dried by sulphuric acid was driven into the bulb, and the bulb was heated to redness. When 15 or 20 litres of air had thus passed in, the lamp was removed, and the apparatus was allowed to cool, while 15 or 20 litres more of perfectly dry air were allowed to circulate through it.

All accidental moisture being thus avoided, the flask, being perfectly cooled, was exhausted. The vacuum being verified, it was weighed *de novo*.

The flask was then put in communication with the apparatus, from which hydrogen was disengaged.

I then adjusted the apparatus for collecting the liquid water, and the drying tubes for retaining the hygrometric water of the excess of gas. These tubes were always arranged in exactly the same manner as those which precede the oxide of copper.

They were previously weighed, so that by again weighing them after the operation, the weight of water formed might be known.

The oxide of copper was heated to dull redness, the reduction commenced, and the water trickled down in abundance; but in a few hours the formation of water became slower, and the operation terminated only after ten or twelve hours. Consequently, it is not easy to devote less than sixteen or eighteen hours to the execution of each experiment, setting aside the preliminary arrangements, which commonly occupy two or three days.

If I add, that I have obtained in my different experiments more than one kilogramme of water, which I submit to the Academy; that it is the product of nineteen operations, whose numbers are given in the following table; and, finally, that reckoning those which have failed, I have made no less than 40 or 50 similar experiments,—a just idea of the time and fatigue which

this determination has cost me may be formed.

It must likewise be added that the necessary duration of these operations, in compelling me to continue my work until very late, by taking the weights towards two and three o'clock in the morning, in many cases, constitutes a real cause of error. I dare not assert that such weights merit as much confidence as if they had been taken in more favorable circumstances, and by an observer less oppressed by the inevitable fatigue consequent on 15 or 20 hours of unremitting attention.

SYNTHESIS OF WATER.

Nature of the drying Bodies.	Weight of the exhausted Flask containing Oxide of Copper.	Weight of the exhausted Flask containing reduced Copper.	Weight of Vessels for collecting the Water.	Weight of Vessels containing Water.	Oxygen consumed.	Water obtained.	Crude Equiv. of Hydrogen.	Equiv. of Hydrogen corrected for the Air contained in the Sulphuric Acid employed.
Sulphuric Acid	291·985	278·806	480·807	495·634	13·179	14·827	1250·5	1249·6
Id.	344·548	324·186	488·227	511·132	20·362	22·905	1249·0	1248·0
Id.	316·671	296·175	439·711	462·764	20·495	23·053	1248·1	1247·2
Phosphoric Acid	625·829	568·825	884·190	948·323	57·004	64·044	1250·6	1249 0
Sulphuric Acid	804·546	728·182	887·331	973·291	76·364	85·960	1256·2	1254·6
Id.	533·726	490·155	867·159	916·206	43·571	49·047	1256·3	1255 0
Id.	661·915	627·104	839·304	878·482	34·811	39·178	1254·6	1253·3
Phosphoric Acid	612·625	566·738	824·624	876·244	45·887	51·623	1250·0	1249·0
Id.	904·643	844·612	822·660	890·246	60·031	67·586	1258·3	1255·1
Sulphuric Acid	642·325	590·487	741·095	799·417	51·838	58·320	1250·4	1248·9
Phosphoric Acid	587·645	535·137	874·832	933·910	52·508	59·078	1251·2	1249 0
Id.	673·280	613·492	931·487	998·700	99·789	67·282	1253·3	1250·8
Sulphuric Acid	660·855	598·765	682·374	752·273	62·090	69·899	1257·7	1254·8
Id.	642·325	590 487	741·097	799·455	51·838	58·360	1258·1	1256·2
Id.	937·845	881·362	1064·762	1128·319	56·483	63·577	1255·8	1252·2
Phosphoric Acid	756·352	719·563	878·640	920·030	36·789	41·390	1250·6	1249·1
Id.	764·162	720·000	887·817	926·275	34·162	38·458	1257·3	1255·1
Id.	759·762	727·632	888·662	924·837	32·133	36·175	1257·5	1254·7
Id.	747·652	716·825	877·862	912·539	30·827	34·677	1248·8	1248·0
						Mean	1253·3	1251·5

I have done my best; and in here retracing all the circumstances of my experiments, I have but one sole object, that of putting all chemists in a situation to appreciate their value, and to give every one his portion of the chances of error which they may suffer.

If it were thought that these experiments may be abridged, the contrary will very soon be seen by looking over the summary of the operations of which they are composed:—

1. Disengagement of hydrogen into the apparatus to cleanse it from air:
2. Weighing the flask filled with oxide of copper, and exhausted of air:
3. Weighing the apparatus for collecting the water:
4. Adjustment of the apparatus:
5. Reduction:
6. Cooling the flask, the stream of hydrogen being maintained:
7. Weighing the cold flask exhausted of hydrogen:
8. Removing the hydrogen from the apparatus containing the water, by means of a current of dry air:
9. Weighing the apparatus containing the water:

Considering, also, that the day and night had been completely devoted to passing dry air over the heated oxide, and to preparing all the weights.

Indeed, these experiments might be abridged by diminishing the quantity of water which may be produced in each of them; but attention must be paid to a peculiar circumstance, in order to ascertain the extent to which this diminution is permitted.

Of all the analyses which a chemist can propose to himself, that of water carries most uncertainty along with it. Indeed, one part of hydrogen combines with eight parts of oxygen to form water, and nothing would be more exact than the analysis of water, if we could weigh the hydrogen as well as the water resulting from its combustion.

But it is impossible thus to perform this experiment. We are obliged to weigh the water formed and the oxygen used for its production, in order to deduce from it, by

difference, the weight of the hydrogen. Thus, an error of $\frac{1}{90}$ in the weight of water, or $\frac{1}{800}$ in the weight of the oxygen, affects a quantity equal to the weight of the hydrogen to the amount of $\frac{1}{90}$ or $\frac{1}{80}$. If those errors which are in the same direction be added, we shall have errors amounting to $\frac{1}{40}$.

We must not, then, be surprised that Berzelius and Dulong have really determined the atomic weight of hydrogen only to about $\frac{1}{60}$. It is surprising only that they could believe that this determination was accurate to about $\frac{1}{1000}$.

I shall deem myself very happy if the future proves that my experiments give the atomic weight of hydrogen to nearly $\frac{1}{100}$. I wish I could have arrived at $\frac{1}{1000}$; but I have not been able to do so, and I leave it to more skilful hands. I found that in proportion as I increased the weight of water formed, and the duration of the experiments, different causes of error complicated the weights and diminished their precision.

At all events, the atomic weight of hydrogen cannot be below 12·50, oxygen being taken at 100. My experiments place it between 12·50 and 12·56, and, if they leave anything to be desired in a philosophical point of view, they are abundantly sufficient for all practical purposes.

By considering water as formed of one of hydrogen and eight of oxygen, no chemist will ever be led to commit an error in his experiments or calculations, since it has been found that it contains:—

8	of oxygen and	1 of hydrogen.
80	*id.* . .	10 ,,
800	*id.* . .	100 ,,
8000	*id.* . .	1000 or 1003.

I now know what causes of error I have met with, and what means must be adopted to avoid them. Perhaps I may one day resume this investigation, which I regard as one of the most delicate and most important in natural philosophy.

Indeed, if the molecules of elementary bodies are all multiples of the molecule of hydrogen, as Dr. Prout supposed, no one can foresee the consequences to which a relation of this nature will lead chemists, when it is well proved, and when they dare to confide in it.

The density of hydrogen can teach us nothing on this subject more than we are able to deduce from the analysis of water itself. M. Boussingault and I found that this density is comprised between 0·0691 and 0·0695, a number sensibly higher than that of Berzelius and Dulong, which, as is known, is estimated at from 0·0687 to 0·0688.

The densities of gas taken by Berzelius and Dulong are generally too low, with the exception of nitrogen. It is probable that this is owing to some fault relative to the measure of the temperature of the gas and to the fortuitous mixture of air with the gas weighed.

If it were possible to determine the density of hydrogen in such a manner as to answer to the fourth decimal, this determination would be of great importance in the discussion with which we are occupied; but to attain it a great number of weights will be required, and hitherto we have been able to execute only five. Hereafter we will make known the process followed for executing them, and the mean of the results with which they will have furnished us.

What I now wish to establish is, that the determinations of atomic weights of Berzelius, and his synthesis of water in particular, leave undecided the question raised by Dr. Prout. I am not ignorant that, in England, some years ago, Dr. Turner examined whether the atomic weights of Berzelius were more conformable to experiment than those which resulted from the views of Dr. Prout; and that he concludes his investigation by pronouncing in favor of the tables of Berzelius; but I should add that Dr. Turner did not use sufficiently delicate methods for deciding the question.

I may conclude from my own experiment, that the weight of the molecule of hydrogen being 1, that of the molecule of carbon is 6, that of the molecule of nitrogen 7, and that of the molecule of oxygen 8. These proportions admit of only very insignificant errors.

In order to verify their accuracy, and to control the other atomic weights, we must enter upon the road opened by the new analysis of carbonic acid; that is to say, we must perform analyses and syntheses on a very large scale, by operating very simple reactions on very pure bodies.

I may here give the analysis of Iceland spar as an example. That on which I operated was collected by M. Eugène Robert, who was kind enough to sacrifice it in favor of my investigations.

According to an analysis performed on 30 grammes, it contained:—

Carbonate of lime .	29·991	9997·0
Silica	0·004	1·3
Peroxide of iron . .	0·005	1·7
Magnesia	a trace.	trace.
Oxide of manganese .	a trace.	trace.
	30·000	10000·0

Submitted to calcination until its weight underwent no further change, this spar fur-

nished the following results in three experiments, the weight being taken *in vacuo* :—

Weight of the Spar.	Weight of the Lime.	Lime per Cent.
49·916gr.	28·016gr.	56·12gr.
50·497	28·305	56·04
64·508	36·167	56·06.

If these very simple and easily repeated experiments are confirmed by new investigations, it must be concluded that the atomic weight of calcium is exactly equal to twenty times that of hydrogen.

I have said, and I now repeat it, that all the atomic weights require a careful revision; that, without adopting or repelling Dr. Prout's opinions, I am forced to admit that they are generally in accordance with my own experiments; and that, consequently, there is a road open to investigations, when every ulterior investigation might be deemed useless.

Having long been pre-occupied with labors of another nature, I am unable to follow up in this new path; but I hope that it will not long remain uncultivated, and that it will not delay to furnish its fruits to science.

ON THE COMBINATIONS OF CHLORINE WITH BASES.*

BY M. GAY-LUSSAC.

Most chemists had adopted the opinion of Berthellet, that chlorine, in coming in contact with an alkaline solution, combines directly with the base, and forms a chloride of oxide which is maintained until the moment when the insolubility, rendered active by the saturation of the base and a sufficient concentration of the solution, determines its division into metallic chloride, and into chlorate.

Berzelius was the first who shook this opinion, by showing that a solution of carbonate of potassa, saturated with chloride of potassium, into which a current of chlorine was directed, very soon deposited, without any appearance of oxygen, chloride of potassium which he supposed to be newly formed. This result would seem to prove that at the same time as the chloride of potassium is precipitated, an oxygenous compound remaining in solution should be formed. However, it may be asked if the precipitation of chloride of potassium should not be attributed to a perturbation of solubility caused in the solution by the admission of chlorine, rather than to the formation of a new quantity of this salt. At least it is certain, that the experiment of Berzelius, highly interesting as it is, is not sufficiently clear to demonstrate that the chlorine received into a solution of potassa forms in it, not a direct combination with that base, but indeed, simultaneously, chloride of potassium and a salt of potassa different from the chlorate, since it possesses a very energetic decoloring power.

We owe to M. Soubeiran two experiments which fortify the theory of Berzelius, and even render it very probable.* This skilful chemist decomposed a solution of chloride of lime by carbonate of ammonia, and he obtained a decoloring liquid, which, if it be not too concentrated, is maintained without sensible decomposition at the ordinary temperature. Now, a solution of chlorine and ammonia being instantly decomposed under the same circumstances, it should appear very probable, that in the pretended chloride of ammonia, and consequently in the chloride of lime with which it was produced, the chlorine was not combined with the lime or with the ammonia, and that it should exist in it in the state of a peculiar acid, of a true chloracid.†

The other experiment of M. Soubeiran is much more decisive. By evaporating a solution of chloride of soda *in vacuo*, at a low temperature, he obtained crystals of common salt, and a residue which, brought by means of water to the primitive bulk of the solution, had preserved its decoloring power. Consequently this decoloring residue must have contained all the oxygen of the portion of soda which had been converted into salt; and this oxygen, according to all analogies, should be combined with chlorine, and not with soda.

It therefore became very probable that at the moment when chlorine combines with an oxybase, an oxygenous compound of chlorine is formed. But of what nature? The experiments of Berzelius and Soubeiran teach us nothing in this respect.

Berzelius was disposed to admit that this oxygenous compound of chlorine was that discovered by Count Stadion, to which Sir H. Davy's and my experiments had given the formula of $Cl\,O^4$. But the illustrious chemist, prepossessed with the opinion that the series of the oxygenation of chlorine should be 1, 3, 5, 7, is doubtful as to this formula, and has instinctively adopted $Cl\,O^3$. M. Soubeiran, yielding to so powerful an opinion, likewise admits the same formula.

* *Comptes Rendus*, No. 25, Juin 20, 1842.

* *Annales de Chimie et de Physique*, Vol. XLVIII. p. 113.

† It might correctly be remarked that, although ammonia is decomposed by the immediate contact of chlorine, it does not follow that it should be so in Soubeiran's experiment. The circumstances are not really the same.

Be it as it may, whether the compound of chlorine and oxygen discovered by Count Stadion is Cl O^3 or Cl O^4, it nevertheless remains to demonstrate its existence in chlorides of oxides, and even its composition put in doubt by theoretical analogies.

Such was the state of the question when M. Balard published his beautiful *Investigations concerning the Nature of the Decoloring Compounds of Chlorine*, (*Annales de Chimie et de Physique*, Vol. LVII. p. 225). This talented chemist, treating with chlorine the red oxide of mercury in water, obtained a peculiar acid, formed of four equal equivalents of chlorine and oxygen, to which he gave the name of *hypochlorous acid*, on account of the analogy which he thought he recognised between it and hyposulphurous acid. Chlorine is absorbed by oxide of mercury with great rapidity. According to M. Balard, hypochlorite and oxychloride of mercury are formed, and, by heating, the hypochlorous acid abandons the oxide of mercury and is volatilised with the water. But M. Balard was able to separate it and collect it under the gaseous form, by means of dry nitrate of lime, which frees it from water.

This remarkable compound has the same color as chlorine, only rather deeper. Its odor, although resembling that of weak chlorine, is quite peculiar. It is not very stable, and is decomposed in a few minutes by the solar light, without detonation. Water dissolves at least a hundred times its volume of it: the solution is colorless and powerfully decoloring. It disengages, with effervescence, carbonic acid from the carbonates, displaces even acetic acid, and forms, with the alkalis, decoloring compounds which have all the characters of those obtained with chlorine. Its action on bodies is most energetic, and, as much by its easy decomposition as by the affinity proper to each of its elements, chlorine and oxygen, it exceeds even nitric acid in the energy of its action. It is thus that it acidifies bromine and iodine, and that it immediately converts selenium into selenic acid. Finally, M. Balard found that it was formed of 2 volumes of chlorine and 1 of oxygen, and that, the same as with water, the condensation was one third of the total volume of its two elements.

These properties of hypochlorous acid do not admit of the least doubt concerning its nature, as a new peculiar compound; but are they sufficiently authentic, according to the circumstances of its formation, to decide clearly that it is this acid which is formed at the moment of the combination of chlorine with bases?

Berzelius, who published several volumes of the new edition of his *Elements of Chemistry* subsequent to M. Balard's investigations, speaks of them, indeed, simply under the form of an addition, in the fifth volume of the German translation, page 440, without discussing them or drawing any conclusion from them, waiting, doubtless, for further light to be thrown on the subject before declaring himself.

M. Marteus, Professor of Chemistry at Brussels, to whom we are indebted for an interesting dissertation on the combinations of chlorine with bases, (*Annales de Chimie et de Physique*, Vol. LXI. page 263,) after having discussed the experiments of M. Balard, nevertheless persists in thinking that the decoloring chlorides are direct combinations of chlorine with bases, and not hypochlorites.

Finally, recently, M. Millon (*Journal de Pharmacie*, Vol. XXV. page 595), in admitting the chlorides of oxide, has supposed that the chlorine which combines with monoxidised bases was equivalent to all the oxygen which they could take to be peroxidised: thus potassa, whose peroxide contains 3 equivalents of oxygen, should take, as indeed it does, 2 equivalents of chlorine, while soda combines with but 1,—its peroxide, according to the most recent experiments, containing only 2 equivalents of oxygen.

This theory of M. Millon is only the application to the combinations of chlorine of the general principle that bodies of very similar properties, like chlorine and oxygen, may form analogous compounds in the same proportions, and mutually replace one another in whole or in part; but here it is not realised, and no fact comes in support of it. It would, therefore, be uninteresting longer to dwell on it, and I will proceed to my own observations. I should say, to justify them, M. Balard has left only to glean, and I shall deem myself fortunate if, in the task which I have imposed on myself, I am able to dissipate the uncertainties which exist in the minds of some chemists as to the real nature of the decoloring compound, which chlorine forms in combining with bases.

Chlorine, as I have already mentioned, acts on oxide of mercury* in water with astonishing rapidity. By employing proper proportions, there is formed only chloride of mercury and M. Balard's hypochloric acid, both of which remain in solution in the water. With an excess of oxide, oxychlo-

* Instead of oxide of mercury, the residues of sulphate of mercury, so frequently obtained in laboratories, may be employed.

ride of mercury would likewise be obtained; but chlorine would afterwards attack it like the oxide, and the same results would be obtained.

The analysis of this experiment may be made in a manner which is as simple as satisfactory. An aqueous solution of chlorine, of known strength, is taken, and very finely divided oxide of mercury in water is added to it by small portions at a time.* A gentle agitation causes the oxide to disappear; nothing is disengaged, and the liquor becomes perfectly transparent. As soon as it remains slightly turbid and yellowish after the addition of a small excess of oxide which remains in suspension, it is allowed to become clear by repose. Then, tried with the chlorometer, the strength of the solution is accurately determined, taking into account the trifling increase of volume caused by the addition of the oxide of mercury.

A given volume of this same mercurial liquor, distilled to about 5-6ths, gives a product which, brought to the primitive volume by the addition of water, has precisely the same strength as previous to distillation.

Finally, during distillation, the mercurial liquor does not allow any oxide of mercury to deposit, and the residue is very pure chloride of mercury crystallisable in fine needles.

From these facts, it results :—

First, that hypochlorous acid, arising from the action of chlorine on oxide of mercury, remains entirely free in the liquor, without combining with that oxide; for, if it did so, distillation, by destroying the combination, would necessarily cause a precipitation of oxide of mercury :

Secondly, that, since chloride of mercury is formed, without oxygen being disengaged, the chlorated liquor must necessarily have acquired the oxygen equivalent to the chloride formed :

Thirdly, that, since the decoloring power has not undergone alteration, notwithstanding the abstraction of chlorine, the loss which results from that abstraction must be exactly compensated by the decoloring power of the oxygen acquired :

Fourthly, and lastly, that, since the hypochlorous acid is free, in the liquor, from all combination, it should contain the chlorine employed, minus that of the chloride of mercury, and the oxygen abandoned by the metal contained in the latter compound. The analysis of hypochlorous acid is reduced, therefore, to ascertaining the amount of chlorine before the saturation by oxide of mercury; to decomposing by an alkali,—potassa, for example,—the chloride of mercury formed, and to carefully collecting the oxide precipitated. This oxide will give at once the chlorine abstracted and the oxygen which replaced it. The following are the data of one experiment :—

Solution of chlorine	265^{cc}
Strength of that solution	$219^{\circ}\cdot 5$*

Consequently, it contains—

$$265^{cc} \times \frac{219^{\circ}\ 5}{100} = 581^{cc}\cdot 7 \text{ of chlorine.}$$

Oxide of mercury collected . . .	$2^{gr}\cdot 855$
Whose oxygen is equal to	146^{cc}
And represents one volume of chlorine	292^{cc}

Thus, $581^{cc}\cdot 7$ of chlorine had been employed; 292^{cc} of it, or about one half, replaced the oxygen of the oxide of mercury; the other half of the chlorine combines with the 146^{cc} of oxygen, which is very nearly its equivalent. It is therefore clear that, in the action of chlorine on oxide of mercury, the chlorine is divided into two equal parts, one of which combines with the mercury of the oxide, and the other with its oxygen, to form hypochlorous acid, which is thus found to contain equal equivalents of each of its elements. M. Balard assigns it to the formula $Cl^2\ O^2$; but we will prove that it ought to be modified.

M. Balard bas been able to obtain hypochlorous acid under the gaseous form, by putting a concentrated solution of it with very dry nitrate of lime over mercury; but this process, which I have repeated several times, has but imperfectly succeeded with me; and, if I may judge of it by the yellow color,—deeper than that of chlorine,—which M. Balard gives as a characteristic of gaseous hypochlorous acid. Whilst the acid which I prepared by a different process was always colorless, he obtained it only very impure.

This process, which is of very easy execution, consists in putting in contact well dried chlorine and oxide of mercury, in a flask of from 100° to 150^{cc} in capacity, the stopper of which is slightly greased round the top. Tbis arrangement is with the view of hermetically sealing the flask, without

* Tbe oxide arising from the decomposition of tbe chloride of mercury by potassa answers this purpose very well.

* A solution at 100 contains exactly once its volume of chlorine at 80°, and under the pressure of $0^{m}\cdot 760$; at $219\cdot 8$ it contains $2\cdot 195$ as much.

the chlorine or the hypochlorous acid being able to reach the grease and attack it.

The flask of chlorine prepared, I take a glass tube, closed at one end, capable of entering into the flask, fill up to about two thirds with oxide of mercury and the remaining third with fine dried sand. The tube is then introduced into the flask, the closed end downwards, and, after having placed in the stoppers, the sand and oxide of mercury are made to fall in, with a few shocks. By a few minutes agitation, the color of the chlorine disappears, and the operation is terminated. By opening the flask under mercury, it fills to about half; with water the absorption is very rapid and almost complete.

I have tried several methods of ascertaining the contraction of volume of the elements of hypochlorous acid; but having encountered some difficulties owing to the subsequent action of the excess of chloride of mercury on the acid,—an action which successively determines its decomposition with a production of oxygen,—I thought it useless to stay to overcome these difficulties, especially considering that M. Balard had obtained a contraction similar to that which takes place with the elements of water, and that my results are of the same tendency as his.

M. Balard, as I have already observed, attributed to hypochlorous acid a color of a deeper yellow than that of chlorine. For my part, I have always found it colorless, even in aqueous or alkaline solutions which contained more that twenty times their volume.

The chief characteristic of this acid is its want of stability: in the gaseous state, it sometimes explodes at the ordinary temperature; in solution in water, it has more stability; nevertheless, it gradually and spontaneously decomposes in it. The solar light singularly accelerates its decomposition, especially when it is concentrated: it is resolved into chlorine, oxygen and chloric acid; the solution also contains a little hydrochloric acid, but it is surely produced by the subsequent action of chlorine on the water.

Hypochlorous acid is very soluble in water. Without having made the experiment accurately, I think, with M. Balard, that water may dissolve more than 100 times its volume.

[M. Gay-Lussac's interesting Memoir being too long for insertion in one No. of "The Chemist," we have divided it into three portions, to be inserted in three consecutive Nos. We adopt this plan as the only means of enabling us to furnish our readers with it.]

ON THE CONSTITUTION OF ATOMS.

BY REUBEN PHILLIPS.

I am aware that many chemists are averse to theoretical considerations, and think it best to adhere to the immediate results of experiment, without deducing from them any far distant consequences; experimental chemistry presents an immense collection of phenomena operated in the profoundest mystery, the cause and mode of operation being concealed from our senses; and I will remind them that, unless we can trace out the cause from the effect produced,—which can only be done by trains of reasoning founded on certain admitted facts,—this obscurity must remain. Biot has very properly demonstrated this branch of chemistry "rational chemistry." I hope to show that our notions concerning the constitution of atoms are more complex than they might be, and, therefore, as we must keep to the most simple, they should be discarded.

An atom has been defined to be an indivisible particle of matter. Every class of atoms with which we are acquainted possesses certain general attributes, inertia, &c., which constitute what are called the physical qualities of matter. Every class of atoms has some attribute, diverse from all other classes, which diverse property determines the chemical character of the atom. These attributes are supposed to be attached to a certain something which has nothing to do with the attributes, but to act as a point of suspension; from this consideration it is plain that if this nucleus be removed, the existence of the properties being continued, a set of attributes will remain exercising all the functions of the previous compound; for an atom, according to pre-existing notions, is not simple, but consists of an inert particle and its active qualities; but qualities are also plural, and the most simple class will consist of only one property as inertia, but whether all or any part of the aggregate properties of matter is capable of separate existence, cannot at present be deduced; we know, however, that certain properties are always found in the same ratio to each other, as inertia and the attraction of gravitation,—but they may, for ought we know, proceed from the same property. May not electricity be an atomic compound, minus some of the ordinary properties of matter?

We know that qualities exist, but we can form no conception of the use of this certain something which performs the part of a nucleus. All we know concerning atomic combinations has been the result of actions operated by properties, and the only use of the presupposed nucleus seems to have been to give to the mind something tangible on which to suspend atomic properties;

therefore, it is not the most simple view—and in every thing, the most simple view, consistent with facts, is the one to be preferred.

An atom may be defined to be a point in space endowed with certain property or properties—and moreover to consist of quality only, without any inert nucleus. The creation of matter would hence consist in creating in a void these attributes; the annihilation of matter would be the destruction of these qualities: at present, we can operate neither one nor the other of these conditions; we can only work with them.

CHEMICAL EXAMINATION OF ARTIFICIAL MUSK AND OF A NEW OIL (EUPIONE OF AMBER), DISCOVERED IN RECTIFIED OIL OF AMBER.*

BY M. ELSNER.

Margraaf discovered, as is known, that there is formed, by mixing about three parts of fuming nitric acid and one part of rectified oil of amber, a peculiar resin, which has the odor of musk. Heyer failed to find the musky odor of this artificial resin. I have endeavored to ascertain whether the negative result obtained by the latter chemist was due to his having used dilute nitric acid, or, indeed, to another cause.

To obtain artificial musk, I added gradually by small portions at a time, to three parts of fuming nitric acid in a porcelain capsule, one part of rectified oil of amber. The resin obtained was washed with water, until it contained no trace of acid. It was afterwards evaporated in a sand-bath to the consistence of an extract.

Thus prepared, this resin possessed the following properties:—

Its consistence was that of an extract; it was of a deep red brown color, transparent and of a hyacinth red in fine streaks; it was very soluble in alcohol, ether and the essential oils; its alcoholic solutions reddened litmus paper; the addition of water rendered them turbid. With great quantities its odor was that of the colophane of amber; when this residue was attenuated, especially in the state of solution, the odor was evidently that of musk. Its taste was burning, bitter and aromatic. Triturated with caustic potassa, this resin gave rise to a developement of ammonia, recognised by the clouds which were formed on a glass rod moistened with hydrochloric acid. Heated on a sheet of platinum, it burned with a very fuliginous flame, and left a shining porous residue.

It has no action on the alcoholic solutions of the chlorides of copper and mercury, and nitrate of silver: the solution of artificial musk forms in the alcoholic solution of lead a brownish yellow pulverulent precipitate completely soluble in an excess of the solution of acetate of lead, but insoluble in an excess of the resinous solution. This combination is formed of—

15 at C	=	1125·00	=	32·34
16 ,, H	=	99·68	=	2·81
2 ,, N	=	177·04	=	5·09
7 ,, O	=	700·00	=	20·13
1 ,, Pb O	=	1394·50	=	39·63
Weight of the combination . Total .		3496·00		100·00
Subtract the atom of Pb O		1394·00		
The atomic weight of the resin remains . .		2102·00		

By calculating the weight of the resin according to that of the combination of lead, we find—

C	=	53·67
H	=	4·79
N	=	7·33
O	=	34·21
		100·00

Perfectly rectified oil of amber, which had been kept over fused chloride of calcium, gave by analysis—

C	=	84·00
H	=	8·63
O	=	7·40
		100·03

The comparison of this latter result with the composition of the resin contained in the combination of lead evidently shows that, in the action of nitric acid on rectified oil of amber, a certain portion of carbon and hydrogen is eliminated, and that there is, on the contrary, an absorption of nitrogen and oxygen; hence, the formation of the nitrogenous electro-negative resin.

Supposing, from the variation of the boiling point of oil of amber, that it was not a simple oil, but a compound of several substances, as are all the products of the dry distillation of organic bodies, I endeavored, and, in fact, succeeded, to isolate its different principles. But the most interesting product is an oil possessed of the following properties: It is as clear as water; it dissolves iodine, acquiring a red brown color,

* *Journal für Praktische Chemie.*

and without fulmination; put in contact with shining fragments of potassium, it does not undergo any alteration, even with an elevation of temperature; it makes greasy stains on paper, which disappear, however, by heat; it readily dissolves in alcohol of 0·810, and in sulphuric acid, as well as in the essential and fat oils. Mixed with three parts of fuming nitric acid, it acquires a deep red brown color; the liquor becomes turbid and milky by the addition of water, and in a few hours deposits a yellow resin which possesses *all* the properties of *artificial musk*. From these facts, then, it results that this oily substance is the real cause of the formation of the latter body. It greatly resembles eupione, although it is distinguishable from it by peculiar properties; I think this substance should be called *eupione of amber*, and the artificial musk, *resin of eupione of amber*.

This oil has the following composition:—

C^{35}	=	85·02
H^{58}	=	11·74
O^{1}	=	3·24
		100·00

Its rational formula would be 7 (C^5 H^8) + H^2.

The greater or smaller proportion of this oil in rectified oil of amber may render the odor of musk more or less perceptible in the treatment of the latter by nitric acid, and this fact would explain the observation of Heyer mentioned at the commencement of this article.

INVESTIGATIONS CONCERNING THE OIL OF THE MADIA SATIVA.*

BY M. RIEGEL.

The oil of madia has a very deep yellow color, a thick consistence, a peculiar weak odor, and a sweet, greasy taste. The specific gravity of the impure oil has been found at + 15° C. = 0·935, that of the purified oil = 0·9286. This oil, which belongs to the series of drying oils, absorbs a very considerable quantity of oxygen gas. In the space of five months, it absorbed 150 volumes of oxygen and acquired a much greater consistence. Exposed in very thin layers to the contact of the air, it gave rise, at the end of six months, to a viscous mass, perfectly white, with a very strong rancid taste and odor. This soil solidifies at − 22·5° C. It is very soluble in 30 parts of cold alcohol and 6 parts of boiling alcohol.

Mixed with one or two per cent. of concentrated sulphuric acid, this oil is immediately colored of a dark green, and deposits a very small quantity of a colored substance. It may be purified by sulphuric acid according to the known method; it then loses its color and its odor, becomes more fluid, and, in this state, burns with a clear flame.

Nitrous gas in a short time colors impure oil of madia of a brown red; if the reaction lasts long, and if the oil thus heated be exposed to the air, it is almost entirely decolored. It becomes perfectly clear and transparent by washing and filtration.

Digested for a long time, at a gentle heat, with oxide of lead, this oil is decolored; it deposits at the bottom of the vessel an orange yellow combination; the oil after some time turns thick, and acquires the consistence of castor oil. The mass finally becomes clear and transparent, very much resembling Venetian turpentine. Boiled with oxide of lead, this oil gives a good plaster, and with a solution of soda a solid, inodorous soap, which makes a good lather.

The soap of potassa, which may likewise replace the potassa soap of other oils, was decomposed by hydrochloric acid. The liquor freed from oil, which was separated, was submitted to distillation; baryta water was added to the product of this operation, and the liquor was evaporated until greatly reduced in bulk, sulphuric acid was added to it, and it was distilled. The liquid arising from the distillation was tasteless and inodorous, and contained neither oil nor acid (volatile principle).

* *Jahrbuch fur Praktische Pharmacie.*

REVIEW:—

ELEMENTS OF CHEMICAL ANALYSIS, *Inorganic and Organic*. By Edward Andrew Parnell, Chemical Assistant in University College, London. London: Taylor and Walton, 28, Upper Gower Street.

A work devoted to analytical chemistry has long been wanted: that deficiency is now in a very great measure supplied by the work before us.

Chapter I. is devoted to manipulations and reagents; Chapter II. to the behaviour of substances with reagents; Chapter III. to Qualitative analysis, and Chapter IV. to Quantitative analysis. In the Appendix the author gives the method of calculating the atomic constitution of a body from its percentage composition; directions for taking specific gravities; tables for analytical calculations, tables of atomic weights, &c.

Want of space compels us to be brief in our observations.

This work will prove of great utility to

students, and will also be of service, as a book of reference, to the proficient in chemistry. There are, however, a few points to which we would direct the author's attention in any future edition. For instance, at p. 15, in giving the methods of detecting the various impurities contained in the reagents in common use, he says:—"Chromate of potash occasionally contains some sulphate of potash. This is detected by the precipitate produced in the solution by nitrate of barytes not being entirely soluble in free nitric acid. Chromate of barytes is soluble, but sulphate of barytes is insoluble in nitric acid." Chromate of baryta is decomposed by nitric acid, and if there is much free nitric acid, the resulting nitrate of baryta will be insoluble in that liquid. Nitrate of baryta is quite insoluble in strong nitric acid. To use this process so as to produce accurate results, any insoluble matter which may remain after the addition of the nitric acid must be treated with a large quantity of distilled water, in order to ascertain whether it is sulphate or nitrate of baryta, and to dissolve out any of the latter salt that may be present.

This book is not extractable. We give, however, the author's remarks on Qualitative and Quantitative analysis.

"QUALITATIVE ANALYSIS."

"The performance of a satisfactory qualitative analysis of a complex substance by the aid of the preceding tables, would be a matter of some difficulty in their present arrangement. Instead of applying each reagent successively, the method in examining the composition of single or mixed bodies, consists in determining the presence or absence of certain *classes* of substances by the application of a single test for each class; these classes can be divided and subdivided by other tests, and the subdivisions examined for their individual members. A particular class of metal is, for instance, precipitated by sulphuretted hydrogen; by the behaviour of this class with hydrosulphate of ammonia, it admits of a farther division, some of the metals precipitated by sulphuretted hydrogen being soluble, the remainder insoluble, in that liquid; thus affording two divisions, each of which, if both are found to exist, is then examined for its individual members."

"The advantage of this method of procedure arises from its being easier to discover classes than particular bodies; and the absence of a class being demonstrated, it is of course unnecessary to examine by other tests for any particular member. The principal feature in this system is, therefore, that of exclusion; on the application of a test, hydrosulphate of ammonia, for example, when we perceive none of the appearances which that test should produce with certain metals, we reject these as absent from the combination. We thus have a negative result; but to prove the absence of particular substances, is in itself just as important in qualitative analysis as to demonstrate their presence."

"A substance may occasionally be presented for examination, whose composition can be very well ascertained by the application of two or three reagents; but it is advisable, and will mostly, in the end, be found more satisfactory, especially when we are not assured of the purity of the substance under examination, to follow some general rule, or particular system, rather than to try the effect of a reagent at random, for some vague idea we had before entertained of the composition of the substance. Such systems are developed in the following tables, where, it will be seen, classes are formed, divided, subdivided, and, if necessary, divided further, characters being described by which the particular members of each subdivision are distinguished from one another."

"An examination of the physical characters of a substance must always precede its analysis. It is of importance to know its density, volatility, combustibility and solubility. Before proceeding to determine the inorganic constituents of a substance, it is also necessary to ascertain whether or not any organic matter is present, this materially affecting many metals in their behaviour with reagents, as may be seen by the preceding tables. Almost all fixed organic bodies possess the property of becoming blackened or carbonised by heating out of contact with the air. To discover organic matter, therefore, a small portion of the substance should be heated in a glass tube, free from lead, when, in general, blackening will occur, and an empyreumatic odor be developed, if organic matter be present. The tube should be held slightly inclined, and be heated by a spirit lamp."

"There are, however, a few inorganic bodies which become black on heating; and, on the contrary, some organic bodies which do not blacken. But there is another character of organic bodies, which, taken with the blackening by heat, may be considered decisive. This is the property they all possess of deflagrating when thrown into fused nitre; a property possessed, too, by a few inorganic bodies, but by none which also blacken by heating. The two tests conjoined are therefore conclusive."

"By the same operation of heating in a tube to discover organic matter, we ascertain (when heated gradually) whether the sub-

stance contains *water*. If so, this condenses on the sides of the glass tube before the heat is sufficient to carbonise the substance. If the condensed water is alkaline, this shows the presence of *ammonia*."

"If organic matter is present, and the object of the analysis is merely to discover the fixed bases, and not the acids of the substance, it should be destroyed by combustion in the open air, either in a platinum or a porcelain* crucible, uncovered, heated very strongly by a spirit lamp with a circular wick, or by the inflammable mixture of gas and air described at page 10."

"The proper solvent of the substance must next be sought. Pure water is first to be tried; if neither wholly nor partially soluble in this, hot hydrochloric or nitric acid or aqua regia must be tried successively, until it dissolves entirely, or leaving only a slight residue, which must be collected and examined by other means. Acids frequently leave insoluble residues, which have been formed by their action on the substance under examination: these may be easily distinguished in appearance from the original substance; thus silica remains as a jelly when some silicates are treated with acids; sulphur when sulphurets, and selenium when seleniurets are acted on by nitric acid. In the last two cases, sulphur and selenium are slowly dissolved by the further action of the acid, sulphuric and selenic acids being formed."

"The course of qualitative analysis to be followed differs somewhat for substances soluble and insoluble in water; but the following tables have been so arranged as to include the necessary modifications for both. When insoluble both in water and acids (as is the case with most silicates), the substance must be rendered soluble in acid by fusion with an alkaline carbonate. This operation consists, 1. in reducing the substance to a state of extremely minute division in an agate mortar; 2. mixing with twice or thrice its weight of pure carbonate of soda or potash; and, 3. fusing in a covered platinum crucible. The resulting mass is then treated with pure hydrochloric or nitric acid, and is in almost every case dissolved, leaving, in the case of silicates, a gelatinous residue of silicic acid. (See the Analysis of Silicates for details of this operation.) When fusion with carbonate of soda is inadequate, caustic potash (*potassa fusa*) is to be substituted, and the fusion performed with care in a silver instead of a platinum crucible."

"The first section of this Chapter contains directions for the qualitative analysis of a salt consisting only of a single acid and a single base, comprising all those which are of ordinary occurrence: with such, rather than with complex substances, the beginner will do well to exercise himself."

"Respecting the quantity of matter to be operated on, it will be found more convenient to work with small than with large amounts; from ten to twenty grains will be sufficient in almost every case. A small quantity must always be reserved, in case of accidents, or to examine the action of any special test."

"QUANTITATIVE ANALYSIS."

"The portion of material employed in a quantitative analysis ought not to exceed thirty grains; from twenty to twenty-five grains of an inorganic body is the most convenient quantity, except in those cases in which a substance existing in minute proportion is to be determined, when more may be used. When a larger amount is operated on, the chances of error in the analysis are seldom reduced, while, from the greater quantities of precipitates to be filtered and washed, and of liquids to be evaporated, a much longer time is necessary for the execution of the analysis. If the substance to be analysed is solid and pulverisable, it must be reduced to a fine powder, and cautiously dried, to get rid of all hygrometric moisture without expelling any of its chemically combined water. A temperature of from 100° to 150° Fahr. is quite sufficient to effect this; but if the body parts with its combined water at that degree of heat, it is then necessary to dry it at common temperatures, by exposure to a surface of oil of vitriol in a close vessel. The oil of vitriol is most conveniently contained in a shallow basin, across the top of which are placed wires to support a piece of paper, on which the substance to be dried is strewed. The basin is placed on a level surface, and covered with a bell jar, having a ground bottom; or, if necessary, under the receiver of an air-pump, in which a vacuum is maintained. Six or eight hours exposure is generally sufficient. To prevent the absorption of any water during weighing, the substance should be weighed in a tube about two inches in length and a quarter of an inch in diameter, transferred directly to the vessel in which it is to be dissolved, and the weight of the empty tube with its adhering particles ascertained and deducted from the entire weight of the tube and substance. If the substance for analysis is a liquid, it may

* "When the substance can contain metals reducible by charcoal, and which would alloy with platinum at a red heat, a porcelain crucible must be used; in other cases platinum is preferable."

be weighed in a specific gravity bottle, whose weight when empty has been previously ascertained: the liquid is poured out into a convenient vessel, and the bottle washed out several times with distilled water; the washings to be added to the original liquid."

"It has already been stated that a substance is generally obtained in a fit state for weighing by precipitation from a state of solution. Particular processes will now be described which are practised in the quantitative estimation of bodies, supposing those bodies or their compounds to exist in a state of purity, with the various methods of separating substances from one another in a state of mixture, for the purpose of their quantitative determination, commencing with the alkalis and earths, and proceeding to the metals proper and non-metallic bodies, those mixtures only being considered which occur in nature or are likely to be presented for examination. I have pursued as far as possible the plan adopted by Rose, in his valuable "*Handbuch der Analytischen Chemie*," of treating, under each substance, of the mode of separating it from those bodies which have been previously considered; so that a method of separating any two substances being required, the process, if given, will be found under the last of the two bodies in question."

"In quantitative as in qualitative analysis advantage is sometimes taken of certain similarities in the properties of bodies, which permit of their classification into groups, separable from each other by a single agent: thus sulphuretted hydrogen removes one class of metals proper from a solution, and hydrosulphate of ammonia another class, leaving earths and alkalis, the former of which may be precipitated by an alkaline carbonate."

In conclusion, we may observe that the work before us will be found of incalculable utility to all who are engaged in chemical pursuits. To chemists and druggists it will be invaluable.

ON THE COMPOSITION OF MUCUS.*

BY GEORGE KEMP, M. B., PET. COLL. CANTAB.; FELLOW OF THE CAMBRIDGE PHILOSOPHICAL SOCIETY.

THE extent of surface in the animal body covered with mucous membrane—the numerous diseases either originating in this membrane, or in which it participates—the remarkable modifications and visible physical changes which the mucus undergoes, and our total ignorance of the elementary composition of this secretion—must render it an interesting subject of investigation to the chemist, and one of great practical importance to the physiologist and pathologist.

At the suggestion, and under the direction, of Professor Liebig, I have endeavored to contribute some information on the subject, by undertaking a series of analyses in the laboratory at Giessen. The subject operated on was obtained from the gall-bladder of the ox, and treated in the following manner.

After tying up the *ductus choledochus communis*, and removing the gall-bladder, the whole of the liver, &c. attached to it was carefully dissected off, and every trace of blood removed with filtering paper; an opening was now made with a lancet, and the bile slowly poured off, retaining the last gelatinous portion, which consisted principally of mucus mixed with the bile: this portion was allowed to run into a glass containing a mixture of absolute alcohol and water in equal proportions. After the gall-bladder had been apparently emptied, a longitudinal section was made, and the mucus scraped off the mucous membrane with a platinum knife; this was added to the preceding portion, and the further operation conducted as follows.

The mucus, after being well agitated with the mixture of alcohol and water, was separated on a filter, and the gelatinous matter thus left treated with ether, which removed the fatty matter, together with a small portion of cholesterine. The substance was again washed with very dilute alcohol, until the liquid passed off colorless, and gave no precipitate with nitrate of silver, and dried at a temperature not exceeding 100° Celsius. When moist, the mucus thus obtained may be described as a greyish, gelatinous, opaque mass, not soluble in water, but becoming transparent in this menstruum. When dried at 100° C., the color changes to a dark olive.

The following were the results of the elementary analysis of this substance:—

I. 0·135 gramme gave carbonic acid = 0·232	or carbon	= 0·0638 = 47·29	per cent.
water . . . . = 0·087	or hydrogen	= 0·0096 = 7·15	,,
II. 0·121 gramme gave carbonic acid = 0·208	or carbon	= 0·0570 = 47·26	,,
water . . . . = 0·075	or hydrogen	= 0·0830 = 6·88	,,
III. 0·238 gramme gave carbonic acid = 0·407	or carbon	= 0·1118 = 47·00	,,
water . . . . = 0·153	or hydrogen	= 0·1690 = 7·14	,,

* *From the London Medical Gazette.*

ANALYSES FOR NITROGEN, AFTER THE METHOD OF VARRENTRAPP AND WILL.

I. 0·132 gr. gave platino-bichlo. of hydrochlo. of ammo. = 0·269 or 12·93 per ct. of nitro.
II. 0·154 ,, ,, ,, 0·315 or 12·98 ,,
III. 0·112 ,, ,, ,, 0·234 or 13·19 ,,

The analyses I. II. in each case were made from the mucus obtained from one ox; but the quantity was too small to make an accurate determination of the ash, or a qualitative analysis of sulphur and phosphorus. The analysis III. was made from the mucus obtained from the gall-bladders of six oxen. The quantities of carbon, hydrogen, and nitrogen, were so similar, that it appeared unnecessary to repeat the analyses, especially as it was desirable to reserve as much as possible of the substance for the determination of ash. Again—

I· 0·338 gr. gave ash = 0·034 = 10·05 per ct.
II· 0·400 ,, 0·041 = 10·29 ,,

Deducting the ash from the above analyses, we obtain—

	I.	II.	III.
Carbon	= 52·54	52·46	52·25
Hydrogen	= 7·95	7·64	7·83
Nitrogen	= 14·33	14·46	14·84
Oxygen & sulphur	= 25·18	25·44	25·08

Considering the formula of protein as $C^{48} H^{36} N^6 O^{14}$, and assuming that the quantity per cent. of carbon in the above analyses represents 48 atoms, or, in other words, that the equivalent of carbon is a constant quantity, we deduce the following formula for mucus: viz. $C^{48} H^{39} N^6 O^{17}$, and the quantity of the different elements per cent. should be as follows:—

Carbon	= 52·84
Hydrogen	= 7·09
Nitrogen	= 15·40
Oxygen & sulphur	= 24·67
	100·00

If now we compare the formula of mucus with that of protein, we have—

Mucus . $= O^{48} H^{39} N^6 O^{17}$
Protein . $= C^{48} H^{36} N^6 O^{17}$

$H^3 \quad O = 3$ H. O.

∴ Mucus = protein × 3 atoms of water.

Scherer found the middle coat of the arteries to consist of $C^{48} H^{38} N^6 O^{16}$, or protein + 2 atoms of water; we may, therefore, suppose a series of the following nature:—

Protein . . . . . = $C^{48} H^{36} N^6 O^{17}$
Protein + 1 atom of water = $C^{48} H^{37} N^6 O^{15}$ not yet described.
Protein + 2 atoms of water = $C^{48} H^{38} N^6 O^{19}$ = middle coat of arteries.
Protein + 3 atoms of water = $C^{48} H^{39} N^6 O^{17}$ = mucus.

The remarkable resemblance of the composition of mucus to that of the middle coat of the arteries reminds us of the following fact:—In cholera, in inflammation of the mucous membrane of the bowels, particularly of the colon and rectum, and also in inflammation of the bladder and urethra, large pieces of consistent flaky substance are often found, which very much resemble the middle coat of the arteries, with the exception that no fibres can be detected. The attention of the medical practitioner is often directed to this circumstance by the patient, who suspects that he is actually voiding portions of skin. Bronchial polypi, and the false membrane in croup, present us with cases of large dense masses, most probably of altered mucus, retaining the shape of the parts on which they have been formed, and possessing considerable elasticity; though, like the middle coat of an artery, remarkably weak in proportion to their thickness.

The earliest opportunity will be embraced to ascertain the actual composition of these altered secretions.

If the mucus be treated with absolute, instead of dilute alcohol, it becomes coagulated, and in this state it is not possible to diffuse it through water.

It has been observed by L. Gmelin and Wöhler, that coagulated albumen, when exposed to the temperature of 200° C., is entirely dissolved by water. In order to try the effect of a similar temperature on mucus treated with absolute alcohol, a portion was inclosed in a strong glass tube with water, and the tube sealed. At 120° the fluid became slightly coloured, at 210° the solution went on rapidly, and at 213° the tube exploded.

Carbazotic acid furnishes us with a very delicate test for mucus, provided no albumen nor other protein-compound be present in the fluid under investigation. These bodies can, however, be easily removed; and with this precaution the above acid is a valuable reagent, as it can be much more generally applied than the acetate of lead.

Sulphur exists in mucus, but the quantity has not yet been ascertained.

Giessen, July 16, 1842.

II. CHEMICAL MANUFACTURES.

ADULTERATION OF TOBACCO.

This adulteration is carried on to a much greater extent than many of our readers have any idea of. It is productive of very serious injury to the revenue, the fair dealer, and the consumer. The duty on tobacco amounts to about two millions and a half per annum, and would doubtless realise much more but for this fraud. The fair dealer suffers because adulterated tobacco is frequently sold at an inferior price, thus being put in competition with the genuine article. The consumer is injured because, from the scarcity of genuine tobacco, he is compelled to smoke rubbish. One of the greatest of a poor man's luxuries is his pipe. Adulterated tobacco is not always sold at a lower rate than the pure; it is sometimes sold at the same price as the latter. When it is sold at a low price it is particularly injurious to the Government and the fair dealer, as the poor, who are by far the largest consumers of tobacco, are induced by their poverty eagerly to snap at the bait. Not only must all parties be benefited by an exposure of this adulteration, but the importer of tobacco will derive considerable advantage from it.

The Chemist has ever been the zealous advocate of the honest dealer, and the determined, unflinching enemy of every kind of fraud and imposture. How often have the dishonest writhed under our lash! How diligently have we made ourselves acquainted with their infernal practices, unrelentingly and unsparingly to expose them!! We are resolved to continue our strictures on adulterations, and we shall from time to time lay before the public all the adulterations of articles of great consumption that come to our knowledge. We shall in future take even greater pains to acquire information connected with these frauds.

Impressed with the seriousness of the injury arising from this adulteration, Mr. Charles Watt, Jun. sent a letter to the editor of *The Times*, which was published in that paper on the 6th ult., commenting on its evils and mentioning some of the substances used for the purpose.* This letter was replied to in *The Times* of the 9th ult. by a person signing himself "A. B. Bremner, *tobacco broker*, 155, Fenchurch Street," whose ire was considerably raised by the *exposé*. A more puny attempt was never made. We never read a more pitiful exhibition of blundering stupidity, lamentable ignorance, and gross inconsistency. It afforded us infinite amusement in the perusal, and excited our risible faculties much more than our anger. The silly production is infinitely below derision; nothing that we can say can possibly make it appear more absurd than it is.

Mr. Charles Watt, Jun., however, thinking that if it were allowed to remain unanswered, it might cause injury to him in his profession, sent to the editor of *The Times* a reply demonstrating its utter absurdity. This reply the editor did not choose, for reasons best known to himself, to insert, although common justice demanded of him to do so. If he intended to deprive Mr. C. Watt of the opportunity of vindicating himself from Mr. Bremner's imputations, he should have refused insertion to the letter of that person: having inserted it, however, we cannot see why he omitted the reply. We are perfectly aware that an editor is quite at liberty to decline inserting any communication if he think proper; but we cannot but regard the present as an undue and arbitrary exercise of that prerogative. When a man makes a statement in a public paper, and when a reply is made to that statement, he is always considered to be entitled to the insertion of an answer to that reply, supporting his former allegations. As the editor of *The Times*

* We have remarked that since this subject has been brought forward by Mr. Charles Watt, a better quality of tobacco has been current than before. We have this on the best authority.

has thought proper to deny to Mr. C. Watt this opportunity, we are induced to publish a copy of his letter.

(*Copy.*)

ADULTERATION OF TOBACCO.

To the Editor of "The Times."

Omnia patefacienda, ut ne quid omnino
Quod venditor nôrit, emptor ignoret.

"Sir,—The fairness with which you treat your correspondents induces me to expect that you will give this letter insertion in your Journal, in reply to a letter which appeared in it on the 9th inst. signed "A. B. Bremner, tobacco broker, 155, Fenchurch Street." By a letter which appeared in your valuable columns on the 6th inst. I had called public attention to the adulteration of tobacco; and as Mr. Bremner has ventured unscrupulously to contradict my statements, it is but fair that I should be allowed the opportunity of testing the value of such contradiction. As I do not think it could conduce either to my respectability as a member of society, or to my professional character as a chemist, to follow the example of Mr. Bremner in the use of abusive epithets, it is probable that he may feel some disappointment, that I have not permitted my attention in the exposure of a public grievance to be diverted into a private quarrel. The epithets of 'blockhead' and 'maligner,' and the imputations of 'falsehood and ignorance,' which he has so lavishly bestowed on me, I will not retaliate on him on this occasion; but the public will judge between us as to the accuracy of our respective statements, from the facts and arguments on both sides, and by the motives by which we may be considered to have been actuated. I am a disinterested person, and I am in no way connected with the tobacco trade, except so far as being occasionally employed in analyses is concerned, and I am exposing what I conceive to be a fraud on the public. Mr. Bremner, on the other hand, is a tobacco broker; and by volunteering himself as the advocate of that trade, he will be in the unenviable position of champion of its abuses, as he has failed to prove that the imputed adulterations have not existed. I know nothing of Mr. Bremner, and I am not aware that he can have any knowledge of me, and it is impossible that he can be a competent judge of my professional qualifications. The question is, whether the adulterations which I have attributed to the manufacturers, have been practised or not; and I am happy to find that a correspondent, S. W. F., in your paper of the 10th inst., concurs with me in my remarks.

"I will at once proceed to answer those points in Bremner's letter which require a reply.

"He states, 'It is quite true that the adulteration of tobacco had increased very much; but this is not new.' This is an admission of an adulteration. I never said that it was new to manufacturers, but it was my object to draw public attention to the fact. Had it been new to the former, my charge would have been untrue. I do not for a moment suppose that it is new to Mr. Bremner. The announcement that 'The adulteration that was practised, was quite according to law,' is somewhat startling. This certainly is new, but not true. I suppose that Mr. Bremner means that all deleterious substances were not named in the Act of Parliament prohibiting adulterations; but such an omission could not legalize any adulteration whatever, although it might preclude the enforcement of a penalty.

"In reply to the assertion, that no 'vegetable can be used wholly in the place of tobacco,' I will content myself with submitting to your readers the following fact:—

"Bengal safflower is preferred, at the price of 28*s.* per cwt. It is infused in a weak solution of potassa or ammonia, the former giving a dark brown color resembling 'Shag,' and the latter a light brown, approaching in appearance to 'Returns.' Considerable loss, however, having occurred from the vegetable matter dissolved out, an improvement has lately been introduced; the safflower, having been moistened, is placed in trays in a cask, into which the ammoniacal gas is allowed to pass. By this process, the weight is increased, whereas, after the earlier methods of preparing it, a loss of one half was sustained.

"Having thus explained to Mr. Bremner the particulars of a process which, doubtless, he well knew before, I will add the price at which it has been sold, *viz.* 1*s.* per lb.,* and I beg to enclose (in confidence) the name of the firm by whom the tobacconists have been supplied with this adulteration, one of whom, when the new Act was introduced, pathetically exclaimed, 'Behold another fortune lost!' I am in possession of samples, and I know that orders for a ton were obtained in the course of a month, by a traveller on his journey.

"In the case of the ammoniacal preparation, a ton would afford a profit of about £84.—Perhaps this will explain the ticketing of tobacco at 2*s.* 6*d.* per lb., the duty alone being above 3*s.*

"It may not be amiss to inform the public, that the substitutes for tobacco are always

* Wholesale.—Eds. Chemist.

in shorter threads than genuine tobacco. The vegetables used for the adulteration of tobacco, are far too numerous to be mentioned here.

"Mr. Bremner observes, 'I do not deny that vegetables can be mixed with tobacco, but I deny that they were or are so; and my proof is, that the penalties for their being even found on a manufacturer's premises were so heavy that no one would have run the risk.' It is a new doctrine to urge as evidence of innocence, the fact that a punishment is awarded to guilt. People run risks expecting to escape detection. The existence of penalties shows some necessity for legislative interference, and Mr. Bremner admits, 'that it is quite true the adulteration of tobacco had increased very much'!!! Hence the necessity of the new Act of Parliament on the subject. He denies that vegetables were or are mixed with tobacco; but, doubtless, some of your readers will remember having read, a few days since, of a seizure at Peckham of 12 cwt. of some vegetable substitute, manufactured to sell as tobacco. It is obvious to the plainest understanding, that vegetable substances alone can be had recourse to by the adulterators of tobacco used for smoking.

"Mr. Bremner is requested to reconcile the following contradictory statements contained in his letter:—

"1. 'It is quite true that the adulteration of tobacco had increased very much.'

"2. 'I do not deny that vegetables can be mixed with tobacco; but I deny that they *were* or *are* so.'

"3. 'The *principal* ingredient in shag, returns, &c., is tobacco, and can be nothing else.' [It has, then, other ingredients?]

"4. 'The fact is, that genuine tobacco is now sold in almost every shop: and I have proved [in what part of the letter?] that all shag has tobacco in it.'

"5. 'I do not deny that adulterated tobacco is still sold, as the Act allows a certain time for its consumption.'

"I maintain that cigars are adulterated, despite of all that Mr. Bremner has said to the contrary: the only proof he has attempted to adduce is, that 'honesty is the best policy.' Were this argument a sound one, then we should be able to procure every thing in a state of purity. I leave your readers to decide whether this is the case or not. I could prove this adulteration, by many facts of which I am in possession, but I fear I should take up too much valuable space.

"Mr. B. says he will give a better reason than any I have mentioned for my not stating the method of analysing tobacco, *viz.*, that I 'can't do it.' To this I will reply only that I have not the slightest objection to giving proof at my laboratory, to him or any other person, but I should prefer doing so to one who would conduct himself as a gentleman on the occasion.

"His assertion that I cannot analyse tobacco, is no proof; and I tell him plainly, that he is in a state of blissful ignorance on the subject.

"It has often been my lot, Mr. Editor, to expose aduterations and frauds, and I have learned to disregard the anger of those, who, deriving benefit from their continuance, feel greatly aggrieved at my endeavors. It is and ever has been my consolation, while they are heaping on me scurrilous abuse and unmerited revilings, that I have been actuated by pure motives—by an anxiety for the public good.

In conclusion, I would advise Mr. Bremner to be more cautious in his statements. He should be informed that assertion is not proof, and that abuse is not argument, and that an injudicious advocate is not likely to obtain the respect of his clients. He has now placed himself in a conspicuous position before the public, and having provoked a more minute exposure of the abuses of the tobacco trade, he has assisted in their overthrow, and unconsciously become a public benefactor, when the tendency of his remarks was to uphold them."

In conclusion we may observe, that Mr. Bremner sought to hide the weakness of his case under the very conspicuous vulgarity of his diction, and that he propped up his assertions with abuse, being unable to bring forward any arguments in support of them.

ON THE PRESENCE OF CYANIDE OF SODIUM IN SOME OF THE SODA OF COMMERCE.

BY CHARLES WATT, JUN.

Some time since, while testing for the per centage of pure alkali in a sample of "soda ash," to which I had added some solution of litmus, in order to ascertain when it was saturated, I was surprised to see the solution retain its blue color, although it was completely neutralised by the acid. A little narrower inspection and further consideration very soon showed me that the color was owing to the presence of a cyanide, and consequent formation of Prussian blue, as the alkali contained rather a considerable quantity of iron.

At the time that I made this observation, I took no heed of it, thinking that it was an accidental contamination, and due to the casks which contained the soda having been placed near some ferrocyanide of potassium prussiate of potassa), and that moisture had

passed from the one to the other, or from some similar cause; but subsequent experience has shown me that this is not the case, but that a portion of cyanide of sodium is sometimes generated during the process of manufacturing the soda; indeed, we have lately seen experiments published, which would render it extremely probable that, without great caution, a portion of this salt would be produced. I allude to some experiments published some time since on the formation of the cyanides from the nitrogen of the air.

All the soda of commerce does not contain the cyanide of sodium; the soda in which I have generally (I think I may say always) detected it, is that manufactured from the sulphuric acid obtained from iron pyrites, (but it is not always contained in this,) and this is a fact worthy of especial notice, as the iron which is always present in the soda manufactured from sulphuric acid obtained from the above named source appears to favor the production of the cyanide.

It is well known that an acid, such as sulphuric, liberates from the cyanides hydrocyanic acid; and it is on this account that I would beg leave to call particular attention to the fact which I have stated, as it may evidently be of much importance in chemico-legal investigations; for it is not going too far to suppose that hydrocyanic acid might be detected in the stomach of a party who had died suddenly, and the contents of whose stomach it had been considered necessary to examine, although, perhaps, merely for the satisfaction of the family of the deceased; and this might be the cause of much distress to them, and, probably, the implication of an innocent party, and might lead to other fallacious results in the course of such investigations.

Some of the soda which I have examined I consider to contain a DANGEROUS quantity of the before mentioned salt, and I would therefore call the attention of medical men and dispensing chemists to the fact.

The best method of testing the purified soda of commerce for the cyanide of sodium is to dissolve it in water, and add a little protoxide, or any of the proto-salts of iron; then stir for a few moments. It is then to be saturated with dilute sulphuric acid, taking care to add only a slight excess of acid. If the alkali contain any cyanide, a beautiful blue color will make its appearance, owing to the production of Prussian blue.

I may here add, in conclusion, that I think that the production of the cyanide is owing to too strong a heat being employed in the manufacture of the soda.

ADULTERATIONS.

As we are desirous of exposing, in THE CHEMIST, all the adulterations to which articles of commerce are subjected,—whether the substances adulterated are used in food, manufactures, or medicine, or for any purpose whatever,—we invite our readers to send us the particulars of every adulteration with which they are acquainted, and to render us any assistance in their power, to the end that they may be laid before the public in our Journal.

We are convinced that by this means much will be effected towards restoring trade to a more healthy condition, at the same time conferring considerable and invaluable benefits on society at large. The fair dealer will thus be protected from undue competition, and will find that honesty—instead of being, as at present, a source of suffering—is "the best policy."

III. PHARMACY, &c.

MEETING CONVENED BY THE JUNIOR DRUG CLUB.

WE call the attention of our readers to the following report (furnished to us by the secretary) of a meeting convened by the Junior Drug Club. Want of space compels us to be very brief in our remarks. It is absurd to ordain that the possession of a shop shall be the qualification for a diploma; the diploma should be the qualification for entering into business.

It is unjust to exclude assistants from obtaining a diploma, if they are found, on examination, to merit one: not that we regard the diploma of the Pharmaceutical Society as of any value, or as conferring any honor on its possessor.

At a public meeting of the Drug Trade, convened by the "Junior Drug Club," and held at the King's Arms Tavern, Snow Hill, on Wednesday, the 7th of September last, the following resolutions and petition were fully discussed, in reference to certain proceedings with a view to the improvement of the Drug Trade, and in expectation of a

Bill being brought into Parliament, by Sir James Graham, on the subject of Medical Reform, which it is presumed will affect Chemists and Druggists.

Mr. Whipple, being elected Chairman, briefly explained the nature and objects of the meeting, and read the resolutions prepared and recommended by the committee appointed at a previous meeting.

The resolutions were as follow:—

1. That this meeting regards the present qualification for a member of the Pharmaceutical Society of Great Britain, as unjust to a great portion of the Drug Trade, and as being based upon illiberal principles, entirely opposed to those views which are usually understood as being characteristic of a zeal for the promotion of science.

As unjust to a great portion of the Drug Trade, because the fact of having the words Chemist and Druggist inscribed on a sign-board, joined to a payment of two guineas, entitles to a voice in the constitution, and a certifying diploma of the said Society; both of which privileges are denied to those who may have passed an apprenticeship of many years in the most eminent establishments in the kingdom, even provided their long sustained and well directed application to the study of Pharmacy may have secured for them the management of the first laboratories of the realm; and in this particular this meeting strongly feel that the public and the profession have not been fairly dealt by, when regulations, sanctioning and pointedly securing such exclusive arrangements, have been made by a Society established, according to their own statement, "because it is considered absolutely necessary to the profession and the public, as well as to the prosperity of Chemists and Druggists, that Pharmaceutical Chemistry should receive that encouragement and assistance which its importance deserves." (See *Transactions*, No. I. page 11, line 6 from the bottom.)

2. That as it is as inexpedient as it is impossible that all persons in the Drug Trade should be masters, so it is as impolitic as the conclusion to which the public mind has been directed by implication is untrue, that the mere fact of being in business or not, as the case may be, which has been recognised by the Pharmaceutical Society to indicate the worthiness or unworthiness of the candidate to possess the Society's diploma, the only visible stamp of currency of which the public will be rendered cognizant.

3. That if the interests of the master and his assistant point to a different goal, a view to which this meeting is decidedly opposed, and the latter is to be sacrificed for the benefit of the former, it is dishonest to take one single subscription from an assistant, merely to build up a Society unscrupulously intent upon forwarding the interests of a fraction only of the class it untruly professes to represent.

4. That this meeting, having no opportunity afforded them of officially laying their case before the Pharmaceutical Society, in consequence of its peculiarity of construction, beg to submit for the consideration of Sir James Graham the following facts:—

That whereas its several members, having duly served their several terms of apprenticeship both faithfully and honestly, and being at this moment assiduously occupied in wholesale and retail Drug houses in the City of London, which have no equal in the world for the amount of capital invested therein, and industry requisite for obtaining honestly a return for that capital, humbly pray that good faith may be kept with them in the maintenance of their several privileges by that Government, to whose revenue the stamp duties on their several indentures have contributed; and would further pray that due caution may be observed in conferring the sanction of the legislature on the fraction of a class who avow the practice of a delusion on the public, when they grant a certificate or diploma to individuals, solely on the grounds that such individuals are established in business, and which diploma is intended to convey to the public that such individuals are also competently instructed to carry on the same,—a question which has never been entertained by the said Society in granting the said diploma, and further a Society which would inflict serious injustice on a great portion of the Drug Trade; for should such diploma carry weight with the public, as it is intended it should, no laboratory man, no wholesale or retail chemist and druggist's assistant, however proficient he may be in pharmacy, can receive the same until he enters into business on his own account, and then he may obtain such diploma without any proof being demanded as to his competency, it being intended to be granted apparently with a view of swelling the numbers of those who may wish to assist in such delusion.

Mr. Mowbray rose to move the adoption of the first resolution, and in temperate language, but with much clearness and energy, explained the position in which the assistants more especially were placed, and strenuously urged on them the necessity of pursuing such a course as, whilst it would give umbrage to none, must inevitably result in their occupying the respectable position which their acquirements, no less than their industry, merited.

Mr. Court fully concurred in the sentiments just expressed, and seconded the re-

solution, which was carried with only one dissentient voice.

Mr. SHEPHERD, in moving the second resolution, considered the branches of the trade could not be fairly treated by the Pharmaceutical Society as at present constituted, from the circumstance of its confining its membership, government, and diploma to those only who happened to keep shops.

Mr. YOUNG, in seconding this resolution, concurred in what had fallen from the previous speakers; and pointed out the necessity of meeting circumstances which pressed hard on the assistants, with firmness and moderation.

The resolution was carried.

Mr. FISHER moved the third resolutiou, and in allusion to some remarks that had been made, did not think the language used too strong, but considered that the assistants should express their sentiments firmly, and be satisfied to let their case rest on its own merits.

Mr. FRANCIS objected to the word dishonest. The intention of a man was dishonest, if he intended to proceed to the till of an adjoining room, and appropriate the property of another; but the individual was not dishonest unless he actually abstracted some of its contents.* Therefore he disapproved of the term.

Mr. MOWBRAY replied, that as he felt the term conveyed plainly the feelings of the assistants, in the humiliating position which the denial of a diploma, unscrupulously granted to others who were in business, had placed the majority of that meeting, so were they justified in giving utterance to those feelings. Indeed, he felt the stigma of dishonesty would also be applicable to the meeting, if in their hearts they responded to the expression, yet with their lips did not avow it. In reference to the views entertained by the previous speaker, in order to prove that every one's ideas were not quite so contracted on the subject of dishonesty, he begged to quote the poet:—

"He who steals my purse, steals trash;

'Twas mine, 'tis his, and has been slave to thousands;
But he who filches from me my good name,
Steals that which not enriches him, but makes me poor indeed."

Now, in granting a diploma to the one, and refusing it to another,—equally competent, as they confessed,—but solely because he was not so fortunate as to possess a shop, would they not, so far as the public were concerned, steal the poor assistant's good name, and make him poorer still?

Mr. MOWBRAY moved the fourth resolution, and adoption of the petition to Sir James Graham, to be signed by the chairman on behalf of the meeting. He (Mr. Mowbray) considered it of the utmost importance to the assistants to prove to the Legislature,—as there was no other channel open to them,—that the present regulations of the Pharmaceutical Society were unjust to them, and to request that due caution may be used that all the branches of the Drug trade be fairly represented, and not that exclusive advantages and power should be granted to any portion of that trade. He thought that the position of any Society proved to be similar to that of the Pharmaceutical Society, as it had been by the discussion of that evening, of which the Pharmaceutical Council had received due notice, and a copy of the resolutions, would be far from beneficial to any who composed it, if they did not immediately modify their constitution, and admit into their councils the complaints and suggestions of the humblest in the trade.

Mr. DONCASTER seconded it.

The resolution and petition were then put, and carried *nem. con.* It was moved by Mr. COURT, seconded by Mr. RISBROCK, and carried,—That the deputation to wait on Sir James Graham with the petition do consist of the following, *viz.* Messrs. Whipple, Mowbray, Young, Shepherd, Fisher, Sutton, and Townsend.

A vote of thanks to the Chairman was then moved, and the meeting separated.

Altogether the above meeting, in their devotion to the purpose for which they met, and the manner in which they exposed the injustice dealt out to them, as well as the fair and temperate manner in which they discussed their views, have proved to us, that the Pharmaceutical Society never made a greater mistake than in contriving their exclusion from their meetings, as we gathered was the case. The assistants have only to pursue their course calmly; all they appeared to us to require, was justice, and public opinion will accord it to them.—EDS.

[* We cannot agree with Mr. Francis in this definition of the term dishonest. A man who conceives the idea of committing a robbery, is a man of dishonest character; and he is a dishonest man, even if he be prevented by circumstances from accomplishing his unlawful purposes. Dishonesty consists in the intention, and is not limited to the successful perpetration of the act, which is merely evidence of the intention. Mr. F. is wrong, in our opinion, both philologically and morally.—EDS. CHEMIST.]

DRUGGISTS AND THEIR ASSISTANTS.

To the Editors of the Chemist.

Gentlemen,—

It would appear, that in administering a little salutary reproof to the "London Assistant" of your July No., I have brought down upon myself the "ruthless ire" of another of the class, whose indignation is so great that, breaking all decent bounds, he does not hesitate to charge me with an utter want of "gentlemanly feeling" and "liberal spirit;" and with not possessing "one thought above that of manual labor." Your correspondent is not chary of significant epithets, and from the vituperative and angry spirit which he evinces, shows plainly enough that the dose administered in my former communication, has been too much for his tender sensibilities, and that his *amour propre* has been deeply wounded. Alas! "the Flesh will quiver when the Pincers tear." He evidently dislikes either the raspatory or the actual cautery; and although yearning after distinction and anxious to become the grave gentlemanly champion, as well as the apologist of his order, a little wholesome correction throws him off his guard, and, forgetting his assumed dignity, he resorts to downright abuse to eke out his "remarks."

But now for the gist of his letter. Not content with attempting to defend his honorable compeers, he launches out into a tirade of reproach against the masters, charging them with wilful dishonesty, unbounded ignorance, and a general "incapacity to fulfil their engagements with the apprentices whom they have undertaken *thoroughly* to initiate into the mysteries of their future calling." I would ask: *who* ever undertook *thoroughly* to initiate any boy, or man either, into the mysteries of a Chemist and Druggist, for any premium that may have been given? Does not the boy "put himself" to a Chemist and Druggist, "to learn his art, and with him after the manner of an apprentice to serve," &c.? And in consideration of such services, does not the master covenant to "teach, or cause to be taught, such apprentice *his art which he useth*, by the best means he can," &c.? But where in any indenture occurs the "thoroughly," put in italics by your gentlemanly correspondent? The advantages are to be reciprocal; and does the master or apprentice most frequently fail in fulfilling his part of the agreement?

Your correspondent states that in addition to the utter ignorance of the masters, they (*mirabile dictu!!*) dislike, and attempt to prevent, any *scientific* or *literary* subject being introduced on the tapis, (he must mean *counter*,) either by their apprentices or assistants. Hear this, ye Master Druggists!! Not only do you rob your servants of their premium, but you actually refuse them the liberty of challenging you to scientific discussions in your shops; and keep them all day long, poor "white slaves" that they are, beating the mortar, compounding medicine, with other low villanous employment, fit only for *porters*, and not for high-spirited, philosophical fellows like themselves.

Your correspondent knows perfectly well *why* apprentices are not allowed to prosecute their scientific experiments; and that this is neither attributable to the masters' ignorance nor want of liberality, but to the reckless manner in which these affairs are generally conducted, and the wasteful extravagance which always characterises such proceedings, the terms of their indentures never being thought of. This, your *gentlemanly* correspondent will say, savors too much of pounds, shillings and pence. Be it so. The observance of a *little* more common honesty in these matters would be to the advantage of all parties, as apprentices—ay, and assistants too—are frequently not over scrupulous as to the source from which they procure the materials for carrying on their *learned labors*.

One word to your *high-minded* correspondent.—Let me advise him, when next he contemplates the perpetration of such dawdling senility as was exhibited in his last production, to lay aside the *petit-maître*, and, by infusing something like common sense into his remarks, as well as a *little* of that courtesy for which he is so great a stickler, make your numerous readers some amends for the trouble they take in endeavouring to understand his unmeaning twaddle.

I am, Gentlemen,
Very respectfully yours,
Pestle.

[We regret to see our correspondents wax wrath with each other: there is no occasion for it; it brings their case into ill odor with the public. In all disputes, the most temperate man stands the best chance. We have to urge on our correspondents the necessity for confining themselves to facts and arguments, and leaving abuse to those who have no better weapon. Let them give as much argument and as many facts, in as few words, as they can. We have inserted "Pestle's" letter, because that of "A Reader of 'The Chemist,'" called it forth; but we must positively, for the future, decline

inserting any communications in which a public grievance is converted into a private quarrel. We do not like to see the main question lost sight of. Whe repeat that we are sorry to see our correspondents more intent on lashing each other, than on suggesting some remedy for the evils of which they complain.

The subject on which our correspondents have been writing is indeed a *quæstio vexata;* much has been said and written about it (especially in The Chemist), but nothing has been done; and much more will be said and written before any thing is attempted. There is a great and grievous fault somewhere: there is a want of activity. The Junior Drug Club will, we hope, effect some good: at present it seems to be nobody's business.]

REVIEW:—

ON DISEASES OF THE BLADDER AND PROSTATE GLAND. With Plates. By William Coulson. Third Edition, Revised and Corrected. London: Longman, Brown, Green, and Longmans. 1842. 8vo. pp. 274.

From the title of this work, our readers may at first sight imagine that a review of it scarcely comes within the province of The Chemist, but an inspection of its contents will soon prove the contrary.

In treating of many of the diseases of which it speaks, it contains so much that is based on chemical principles, in relation to the indications, and means of cure, and is so strictly in accordance with our oft expressed opinions as to the importance of the application of chemistry to physiological and pathological science (hitherto so improperly neglected),—an opinion confirmed by the recent work of the illustrious Liebig,—that we are exceedingly glad of the opportunity thus afforded us of calling the attention of our readers to a work which so coincides with our own views.

Mr. Coulson has long been known as an eminent surgeon, and this work proves that he bases his practice on truly scientific principles. It is lamentable to reflect how few imitate this example. The time, however, is fast approaching, when medical practitioners will be compelled to acquire more knowledge, and have it more thoroughly tested.

On opening the work, the first chapter which meets our eye is that "On Urine," and a very able dissertation it is, and highly creditable to the learning, industry, and research of the author.

In this chapter, after giving the properties of urine in the healthy and diseased states, and the various causes which influence the quantity voided in a certain space of time, the author details the substances which it usually contains, and those which are more rarely found in it; after which he describes the various processes for detecting these substances, and for ascertaining their quantity.

An intimate knowledge of the properties of urine, and of the changes which it undergoes during disease, is of the greatest importance to those in any way connected with the healing art, as through its agency they are often enabled to trace out changes which are taking place internally, and which could be ascertained with certainty by no other means; we therefore earnestly recommend an attentive perusal of this chapter.

To show the lucid style in which this work is written, we quote the following:—

"The art of predicting the crisis in diseases by inspection of the urine, as well as their exact nature, is, in many cases, only to be acquired by long and careful observation. The physicians of antiquity made this a most important part of their study; indeed, Hippocrates seldom narrates the case of a patient without particularly noting the condition of the urine. Galen was not less observant of the various appearances of urine in disease; and the first chapters of Alexander Trallianus are occupied with nothing else.

"In modern times, this essential branch of practical knowledge, founded on an attentive examination of the condition of the urine, has been almost entirely neglected; and the fact ought scarcely to excite surprise. The fraudulent, who are ever ready to take advantage of the credulity of the public, and, by playing upon their weakness, to turn it to their own profit, took up the subject of urine as a fruitful and lucrative source of imposition; they pretended to be able to distinguish a patient's disease by the bare inspection of his urine,—and that, too, without requiring to see him. These were the Uromantes or Urine-casters of a bygone century, who, by their empirical conduct, soon brought into disrepute one of the most important aids, when properly employed, in the detection of disease. Uromancy thus came to be abandoned by regular practitioners, and its employment assumed by illiterate charlatans. It is only of late years, that the obloquy attached to the practice of observing the condition of this excrementitious fluid, has been removed; and ever since the observation of the morbid changes of the urine has been based on scientific principles, this important branch of medical knowledge has regained its proper position in the estimation of every discerning practitioner. A knowledge of the urine in

health, and of the variations to which it is subject in disease, is one of the bases upon which we may safely rely for much valuable information. In fact, the different appearances of the fluid may be considered as indicating, not merely different modifications of the actions of the organs separating it, but also as affording correct information of changes that have taken place in other parts of the organic system, whether in consequence of the influence of some noxious cause, operating upon the whole body, or of the sympathy and reactions, that different parts exercise upon each other."

We pass over the intervening chapters, until we arrive at Chapter 11, as they are purely of a medical and surgical character, and we doubt not, from the elevated position Mr. Coulson holds in the profession, that they will be read by the practitioner with much advantage and pleasure.

Of all complaints which "flesh is heir to," there is none in the treatment of which chemical knowledge is likely to prove of greater advantage, or its absence more dangerous, than calculous disorders; for as it must be admitted that the first step towards the cure of a disease is the knowledge of its cause, so is the knowledge of the chemical composition of the various calculi necessary to guide us in the application of remedies.

In the chapter under consideration our author says,—"It is a popular belief that water impregnated with calcareous matter is apt to produce stone, and the prevalence of this disease in Paris countenances the supposition—a supposition which even the philosophical Hales entertained. But were this a cause, calculous disorders would be far more frequent than they are. The water of the Seine abounds with carbonate of lime, and were the disease attributable to the drinking of it, we should naturally expect that the nature of the calculi would correspond with the character and quality of the water; but we know that this is not the case, and few calculi are more rare than those composed of the carbonate of lime."

Although we cannot say that we hold with Hales, still we must admit that the latter argument against the supposition hardly applies, inasmuch as the carbonate of lime might cause the deposition of such calculi as those composed of the phosphates, without being present itself, as carbonate of lime decomposes the super-salts of lime, such as the super-phosphate, causing a deposition of bone phosphate.

In the same chapter Mr. Coulson also details the appearances and properties of the various calculi, and the methods of ascertaining their composition; indeed, this chapter throughout, like the former we noticed, does much credit to the talents and industry of the author.

We extract the following from this chapter for the benefit of our readers:—

"Up to the time of Scheele(1777) urinary calculi were universally regarded as a species of tartar. The discovery by this chemist of a new substance, the lithic or uric acid, in their composition, dissipated this idea, but gave rise in turn to the erroneous idea that all urinary calculi were composed of it. Dr. Austin (1791) was the first to dispute the correctness of the new views; but he adopted the not less exclusive, and equally unfounded, notion, that calculi were universally composed of induratcd mucus only. The progress of chemistry has now satisfactorily shown that urinary calculi vary considerably in their composition. They have been arranged under the following heads, viz.—1. Lithic acid calculi; 2. Calculi of the oxalate of lime, Mulberry calculi; 3. Phosphate of lime do.; 4. Ammonio-phosphate of magnesia do.; 5. Fusible calculi or mixed phosphates; 6. Carbonate of lime do.; 7. Cystic oxide do.; 8. Xanthic oxide do.; 9. Alternating do.; 10. Fibrinous do."

We must now conclude; but we cannot do so without earnestly recommending an attentive perusal of this valuable work, which has already passed through three editions,—a proof of the value which the public set upon it. It should be placed in the library of, and be read by, every physician, medical man, and chemist and druggist, throughout Europe, not one of whom, we venture to say, will rise from its perusal without having gleaned much valuable knowledge, or will consider his time has been ill bestowed.

REMARKS ON THE NOMENCLATURE AND CONSTITUTION OF CERTAIN PHARMACEUTIC SUBSTANCES.*

BY THOMAS HOLT, M.D. UNIV. EDIN.; M.R.C.S. LOND. AND EDIN., ETC.

THE very same reasons which prompted the London College of Physicians to alter the names of some pharmacopœial compounds, and to adopt in some instances the supposed chemical nomenclature, might also have suggested to them that new names—*sesquipedalia verba*—should be adopted in other cases for the sake of *classical* uniformity! The terms potassa, soda, magnesia, calx, alumina, &c., do not point to the chemical compositions of these substances, but *rather leave it to be understood* that the

* From *The Lancet*.

same are oxides of the metallic bases, potassium, sodium, &c. Nor do such terms as cyanogen, ammonia, &c., give the slightest idea as to how these are composed; who would see in the term cyanogen a bicarburet of nitrogen, or in ammonia a compound of 1 eq. of nitrogen and 3 eq. of hydrogen *ultimately*, or of 1 eq. of anidogen and 1 eq. of hydrogen *proximately ?*

The names of many of the salts, as argenti nitras, plumbi acetas, &c., cannot be regarded *as literally correct*, but are used for their comparative shortness.

Many of the acids, as the nitric (NO_5). oxalic $C_2 O_3$), acetic ($C_4 H_3 O_3$), &c. have not as yet been isolated, or shown to have any separate existence; and even if they could be so isolated, would be entirely different from their hydrated compounds which are received into the pharmocopœias. Anhydrous sulphuric acid *does not appear to be acid at all*, "does not redden litmus, and exhibits a disposition to combine with salts, such as chloride of potassium and sulphate of potash, rather than with bases." "The liquid carbonic acid has little affinity for water, does not combine directly with lime, but dissolves in alcohol, ether, and essential oils, like certain neutral bodies." The white precipitate of mercury of the late London and of the present Edinburgh pharmacopœias, was considered by Hennel to consist of

1 eq. red oxide of mercury,
1 eq. hydrochloric acid,
1 eq. ammonia.

Phillips and the London College name it ammonio-chloride of mercury, and consider it as composed of

1 eq. corrosive sublimate,
1 eq. red oxide of mercury,
2 eq. ammonia;

but Dr. Kane proves that it contains no oxygen, consequently *both the above must be erroneous*. The view proposed by the latter high authority, and in this opinion of its constitution he is supported by Liebig, Graham, Gregory, &c., is, that it is a compound of the anide with the chloride of mercury. Dr. Christison (Ph. Edinb.) considers it as "chloride of mercury (calomel) with ammonia." Another view of its constitution might be taken (which has not been suggested that I am aware of), viz., to consider it as composed of a compound basyle analogous to ammonium* with chlorine, for it has the composition of the chloride of ammonium, only that two atoms of hydrogen are replaced by two corresponding atoms of mercury; and Wohler's white precipitate has a similar composition, one atom only of hydrogen being replaced by mercury: and, according to Mitscherlich, the crystalline form of the latter belongs to the regular system, like that of sal ammoniac (quoted by Graham). This would rather tend to strengthen the eq. of mercury being 101, which has been doubtful, chemists being ignorant of any unobjectionable isomorphous relation of mercury to the magnesian family: in the above, however, we have mercury substituted for hydrogen (which is of the magnesian class), and the form of the crystal remaining the same. The following formulæ may be given; *viz.*

H Ad + H Cl = Kane's formula for sal ammoniac.
Hg Ad + Hg Cl = ditto white precipitate.
NH_4 + Cl = Berzelius's view of sal ammoniac.
(NH_2 Hg_2) + Cl = view proposed of constitution of ordinary white precipitate.
(NH_3 Hg) + Cl = ditto of Wohler's white precipitate.
Hg Cl + NH_3 = formula of Wohler's white precipitate, given by Graham from analysis of Kane.

The constitution of the ammonio-chloride of copper, which appears analogous to that of the white precipitates, also favors the view proposed if the formula be compared.

NH_3 Cu + Cl = chloride of cuprammonium (Graham).
NH_3 Hg + Cl
NH_2 Hg_2 + Cl } = vide supra.

This view of the constitution would apply also to the other precipitates of mercury, formed analogously with salts like the chloride. By-the-by, Dr. Pereira, in his work on Materia Medica, is not satisfied with such terms as potassio-tartrate of soda, potassio-tartrate of antimony, &c., but must coin new ones; and gives for the same, sodio-tartrate of potash, ammonio-tartrate of potash, &c., thus adding to the confusion (which is already quite enough), without there appearing the slightest necessity, or those he proposes having any greater recommendation than the terms given by the College (if we are to have new ones).

Bury, Lancashire, Aug. 3, 1842.

* In medical practice, whichever view may be adopted of the chemical constitution, the term white precipitate of mercury appears as good as any, and not likely to be generally superseded; but if the view proposed be received *chemically*, a term composed of hydrargyrum and ammonium—like cuprammonium — would be objectionable from its length, but some such term as Hermammonium, from Ἑρμης, Mercury, might be suggested.

RHUBARB AS AN APPLICATION TO ULCERATED SURFACES.*

BY ALFRED MARKWICK.

While externe at the Male Venereal Hospital, Paris, under the justly celebrated and much lamented M. Cullerier, who died some few months since of acute hepatitis, a patient was placed under my care, who had already been an inmate of the hospital upwards of two months, for severe and extensive ulceration of the abdomen, occupying the whole of its right side and greater part of its left. The right wrist was also much affected. He had unfortunately inoculated it from the abdomen. Almost every plan of treatment had been adopted; even the actual cautery had been applied three times, but without producing any arrest to the progress of the sloughing. At the time I entered the hospital he was much emaciated; his face was expressive of great suffering and despair; he had no appetite; his spirits were very bad, so much so that he would sometimes cry like a child; and he passed restless nights. The application that was then used was a combination of bark and camphor, which was sprinkled lightly over the parts. I continued this for rather more than two months, and he took at the same time sarsaparilla and iodide of mercury; but he derived no benefit: indeed he became still more emaciated, and he had a perfectly ex-sanguineous appearance. I then suggested to M. Cullerier the employment of the powdered rhubarb. To this he readily consented; and the following morning it was applied, but only in very small quantity, on account of the violent irritation it produced. Over this was placed some linen with holes cut in it about a quarter of an inch apart (*linge troué*, as it is called by the French), which was covered with charpie. On removing the dressings the following morning, a decided amelioration was perceptible. The wound had assumed a much more healthy character. M. Cullerier advised some camphor to be added to the rhubarb, in order, if possible, to mitigate the pain occasioned by the application, This was done; but it had not the desired effect. The powder was then only applied every other day for about six weeks, when the ulcers had nearly healed. His appetite, spirits, and appearance, daily improved, and in about a fortnight afterwards he was able to leave the hospital quite recovered. This patient had remained in the hospital four months without obtaining any relief to his sufferings. Six weeks were sufficient to produce cicatrization of nearly the whole ulcerated surface with the rhubarb—a fact which I think sufficiently proves the efficacy of the drug in sloughing venereal ulcers.

The patient had had no return of the disease when I saw him six months afterwards.

* From *The London Medical Gazette.*

BELLADONNA IN EPILEPSY.*

BY DR. DEBREYNE.

Dr. Debreyne, Lecturer on Practical Medicine at La Grande Trappe, Orne, has published a *resumé* of his experience of the treatment of epilepsy, for the last twenty-five years, in the cases that have occurred under his charge; in which he gives the decided preference to belladonna as a therapeutic agent. He says that, after having tried in vain, valerian, orange leaves, white oxide of zinc, the meadow narcissus, the ammoniaco-sulphate of copper, the nitrate of silver, Méglin's pills, the cyanuret of potassium, the croton-tiglium (not used as a drastic purgative, but as an anti-epileptic), he was induced to have recourse to belladonna, by the perusal of a case published by Stoll, in his *Ratio Medendi*, *pars.* iii., p. 217. Of the other medicines mentioned above, he derived the greatest advantage from the oxide of zinc, the nitrate of silver, and the valerian.

Dr. Debreyne has used the belladonna extract in about two hundred cases of epilepsy, during the last twenty-five years, and he says there has been scarcely one in which its use was not attended with advantageous results. Generally speaking, the fits have been diminished in intensity, and have occurred less frequently, or have been altogether suspended for weeks, months, or even years. Cases have occurred where the fits used to come on every month or week, or several times in the week, but where, under the influence of the belladonna, they have been suspended for six months, and even for one, two or more years. Some patients have asserted that they had not had a relapse for seven, eight or nine years. In all cases where the attack comes on with premonitory aura, Dr. Debreyne furnishes his patients with a small bottle of liquid ammonia, to arrest the coming attack. The more frequent the fits are, the more readily is the complaint influenced by the belladonna; but when they occur only once every four, five, or six months, it is much more difficult to modify them, that is, to diminish their intensity, or to suspend them altogether. In such cases the remedy should be administered some time previous to the presumed epoch of the nearest attack. At the same time it must be observed, that belladonna is

* *Bulletin de Thérapeutique.*

by no means a specific in epilepsy; in fact, cases would occasionally occur, in which the severity of the fits would be for a time much diminished, or the attack altogether suspended; and yet, after the lapse of a certain period, the remedy would become altogether powerless. In such a case, no other medicine was found of service. The cases of epilepsy spoke of throughout by Dr. Debreyne, are of course cases of the disease, when it occurs independent of lesion of structure; but he adds, that in symptomatic epilepsy, if, after the removal of the cause, the fits should still recur, by a sort of habit of the nervous system, the belladonna might be had recourse to, and, should it fail, the quinine alone, or combined with valerian, might be ordered. The mode of administration of the belladonna, is as follows:—Four scruples of the watery extract of belladonna are mixed with two of powdered gum arabic, and a sufficient quantity of any inert powder to make 120 pills, one of which is given the first day, two the second, the dose being gradually increased to six in the twenty-four hours, although it may be raised to eight or nine pills taken daily, if disordered vision, or other symptoms of the injurious effects of belladonna, do not show themselves. In that case, the dose must be diminished, or omitted altogether for several days.

ON A MODE OF TREATING GONORRHŒA.*

BY DR. BERTON.

Dr. Berton had already recommended for the active treatment of discharges, the employment, in large doses, (4 to 8 grammes *per diem*) of the following balsamic opiate:—

Balsam of copaiba } aa . . 40 grammes
Powdered cubebs }
Opium . . . . . 4 decigrammes
Powdered alum . . . . 20 grammes
M. S. A.

This preparation succeeds very well, but there are a host of others, very nearly analogous to it, that may be prescribed with equal advantage. In consideration for those of small pecuniary means, it is not unimportant to cure as well, at a cheaper rate.

The following is what I propose, and several experiments have led me to regard it as calculated to attain the desired object:—

At first, as in the preceding antiphlogistic methods, if there be too much inflammation, from two to four grammes *per diem* of the following mixture are to be immediately administered in boluses or pills—

Tar (goudron) } aa . . equal parts
Alum . . . }

Liquorice or lycopodium } in powder, q. s.

F. S. A., boluses of 3 decigrammes.

Add, according to necessity, a few grains of opium or camphor, to diminish the sensibility of the intestinal canal, and to prevent nocturnal erections.

The dose of these boluses may be even doubled without inconvenience.

* *Journal de Chimie Médicale*, Sept. 1842.

INTERNAL EMPLOYMENT OF NITRATE OF SILVER.*

Dr. Fischer, of Tambach, found fused nitrate of silver very efficacious in cases of gastralgia arising from purely dynamic affections of the nerves of the stomach, especially in females. He gives this salt in the dose of four or five milligrammes, either in pills or draught. He prescribed it without any advantage in the cases of gastralgia caused by hemorrhoïdal diathesis, and become independent of the latter.

* *Hufeland's Journal.*

ON MATTICO AS A STYPTIC.†

BY WILLIAM MUNRO, M.D.

The mattico has been very often used by me in the Dundee Infirmary, as an external styptic; but I have not as yet used it internally. It has been very often used to stop the bleeding from leech-bites, and uniformly with the most decided success. Leeches were applied, by his own desire, inside the nostrils, to our house surgeon, a stout, florid young man, with head symptoms in fever. The bleeding, which was profuse, could not be stopped by cold applications to the head, plugging, &c.; but the mattico, in leaf, applied over the bites, and pressed on with the point of the finger, proved immediately successful. The application of the leaf caused increased heat and throbbing for five minutes, when all unpleasant symptoms went off. The mattico was applied to a man with a wound of the right temple, in whom a considerable branch of the temporal artery had been divided; the wound was dressed, and a compress and bandage applied, but still the blood burst out. Cold water was several times had recourse to, still the bleeding returned after awhile; at last the mattico was used. I stuffed the wound with the powder, but found that it was washed away from the wound in the vessel; I then pressed in several pieces of the leaves, holding them firmly for some time with the point of the finger; and the result was, that we had no more hæmorr-

† Abridged from *Prov. Med. and Surg. Journal*, June 18, 1842.

hage. We had a somewhat similar case of a man with a wound of a branch of the palmer artery, which resisted graduated compression and bandages; and in which the mattico was used with the best success.

USE OF PURGATIVES IN THE TYPHUS FEVER OF PARIS.*

In the course of 1839, M. Audral determined to try the effect of repeated purgation in typhus fever; forty-seven patients were submitted to this treatment, of whom twelve were suffering from a mild form of the disease, twenty-one had a more severe attack, and the remaining fourteen were affected with typhus gravior; of these latter, six died. Of these forty-seven cases, twenty-nine were men, eighteen women, and all were young, with the almost solitary exception of one patient, aged thirty-six, and another forty-two. The attack could not be traced in any case to either hygienic or occasional causes, save in two or three instances, where it was attributed to cold, in one, to a journey on foot from Calais to Paris, and in another to a severe fright. The average day of admission into the hospital was the eighth day of the fever. In every case, symptoms of affection of the abdominal viscera were present, and not one was regarded as typhoid fever, unless ileo-cœcul pain, increased on pressure, was discovered. Rosaceous, lenticular spots showed themselves in twenty-three cases; one in the first list of twelve mild cases, twelve in the second list of the twenty-one more severe, and ten in the third list of fourteen of typhus gravior. Sudamina occurred in only eleven cases; five of the second list, and six of the third. Intestinal hæmorrhage did not show itself in any instance. Pulmonary symptoms appeared in all save thirteen, of which exceptions, seven were of the first list, five of the second, and one of the third. In twelve cases, intense bronchitis formed a complication of the fever; nine of the patients being in the second list, and three in the third. The three last mentioned had pneumonia, ending in acute tubercular degeneration. The heat of the skin, measured in every case daily by a thermometer in the armpit never exceeded 104°, nor fell less than 99° Fahrenheit. Erysipelas of the face occurred in two cases of the third list, and probably hastened the fatal termination; in one case, intestinal perforation took place, and another presented all its symptoms. The typhoid fever was changed in one instance into an intermittent, which was cured by sulphate of quinine. Of the twelve cases of the first list, one was entirely unaccompanied by nervous symptoms; in another, pains of the limbs and joints were complained of; of the second list, one case of pains in the limbs and joints, one of ataxia and delirium, and one attended with adynamic symptoms; in the third list, there were two cases of the ataxic form, one accompanied with violent convulsions, seven of the adynamic form, and three of the ataxio-adynamic variety. The average duration of the disease in the first list was twelve days and a half, in the second seventeen days, and in the third twenty-six days and a half, for the eight cases cured; while, of the six that died, one case terminated in six days, one in nine, one in eleven, one in twenty-eight, one in forty, and one in fifty-eight days. Convalescence was more prolonged, and the patient more liable to relapses in the more severe cases than in those which were slighter.

The mode of treatment was the same in all the forty-seven cases. The day after the patient was admitted into the hospital, whether the attack was serious or slight, an emeto-cathartic of two grains of tartarized antimony was administered, which generally produced both emesis and catharsis; the next and following days, without interval, purgations were ordered, and continued as long as febrile symptoms were present, at least until sixteen, seventeen, or eighteen doses had been taken, when their employment was abandoned. The purgation used was almost exclusively the seidlitz water; one bottle, containing thirty scruples of sulphate of magnesia, being given each day; towards the end of the treatment, when the action of the purgative seemed almost null, forty-five scruples were given. Occasionally sixty scruples of castor oil, or twelve grains of calomel, divided into two or three doses, the last being followed an hour afterwards by a glass of seidlitz water, was had recourse to. A senna draught was sometimes given also. The number of purgatives employed was as follows:—In the first list of twelve cases, one case, three purgatives; six cases, four; three cases, five; two cases, six; the average quantity being four purgatives and a half. The duration of the fever in the hospital, from the day of admission to its removal, was in six cases, four days; in three cases, five days; in one, six days; in one, eight days; in one, nine days; the average being five days; the total average duration of the febrile attack being twelve days and a half. In the second list of twenty-one cases, more severe, the purgatives were employed as follows:—In two cases, four purgatives; in one, five; in two, six; in

* *Bulletin de Thérapeutique.*

seven, seven; in four, eight; in two, ten; in two, eleven; in one, twelve; the average number was eight. The duration of the fever in the hospital, from the day of admission to its removal was, in one case, four days; in one, six; in five, seven; in four, eight; in four, eleven; in three, twelve; in one, thirteen; in one, fifteen; in one, sixteen; the average duration was ten days, the total average duration having been seventeen. In the third list of fourteen serious cases, of which six died, the eight cases of cure will be considered first. In two cases, eleven purgatives were employed; in two, twelve; in two, fourteen; and in two, sixteen. In the cases which terminated fatally, in two cases, three purgatives were ordered; in one, seven; in one, ten; in one, fourteen; and in one, twenty-one; the average number was thirteen purgatives for the cases which were cured, and ten for those which died. The duration of the disease in the hospital was undetermined in one case; in one, thirteen days; in one, fifteen; in one, seventeen; in one, twenty; in one, twenty-four; in one, twenty-seven; and in one, twenty-nine; the average duration was twenty-one days, the total average duration being twenty-six days and a half. In the fatal cases, the duration in the hospital was, in two cases, four days; in one, ten; in one, eleven; in one, thirty-five; and in one, fifty-two; the average being nineteen days. As a general result, we have one fatal case out of every eight of well-marked typhus fever, the diagnosis of which cannot be called in question; a result which, in the opinion of M. Precquerel, the reporter, is favorable to the use of purgatives, more especially as these were not picked cases, but taken at hazard from the wards of La Charité, in the course of six months.

The use of purgatives in cases where constipation existed caused diarrhœa, which continued during a part of the whole disease. The tongue got clean while they were employed, but little advantage resulted when the tongue was dry, reddish, and furred. When that organ was very dry, they were omitted for a time, and resumed afterwards. When diarrhœa existed before purgatives were had recourse to, it was found to be increased the first day only, when it was generally accompanied with bilious vomiting, that being the day for the emeto-cathartic. The stools were rarely increased in quantity or frequency by the aperients, which never caused any intestinal hemorrhage. Pain in the abdomen, and especially in the ileo-cœcal region, did not appear to be much influenced by them, nor were the pulmonary symptoms either. The duration of the complaint did not appear to be shortened by their use, but the fever never increased in intensity, and sometimes it was evidently lessened. In a word, the influence of the purgatives in very serious cases seems to have been null, while, in those less severe, but still very dangerous, and in others still milder, it appears to have lessened the febrile excitement, and the symptoms seem to have diminished in severity as the aperients were repeated. The complaint, therefore, is rendered less serious, and, in the most dangerous cases, no injury did accrue from their use.

A LARGE COMPANY POISONED BY SPOILED MEAT.*

BY DR. SIGG.

A FETE was given in Audelfingen, in the canton of Zurich, in the month of June, 1839, at which about 600 people were present, who all partook, more or less, of the repast provided, which consisted of cold roast veal and ham. As they returned home in the evening, some were seized with vomiting; next day, a greater number complained of general uneasiness, sickness and diarrhœa; and in about eight or ten days, almost all those who were present were confined to bed.

The following is the course the disease ran:—From the first, but chiefly from the fifth to the sixth day, those affected began to complain of uneasiness, lassitude, more or less headache, slight rigors, loaded tongue, metallic taste in the mouth, eructations and vomiting alternating with diarrhœa, general feebleness, want of sleep, excessive thirst, giddiness, and imperfect vision. These symptoms continued from six to eight days, at the end of which period, more than a fourth of those seized had recovered, generally without any treatment, after a copious perspiration. In others, again, the symptoms were more severe, the headache was greater, the pupils were dilated, the skin was hot and dry, and colic, sometimes with constipation, and at others with diarrhœa, supervened. In others, again, the face and sclerotica were highly injected. In those who suffered most, hoarseness, with difficulty of breathing and oppression at the chest, came on, from the fourteenth to the seventeenth day. As the abdominal symptoms abated, the patients got better. In the fatal cases, the intellectual faculties were affected, a petechial eruption appeared in the mouth, the breathing became worse, the skin more hot and dry, the belly tym-

* *Hufeland's Journ. der Pract. Heilkunde;* abridged from *Gazette Méd. de Paris*, July 2, 1842.

panitic, and the stools were frequently bloody. At the post-mortem examinations in four individuals, who sunk under the disease, the mucus membrane of the ileum and cœcum, hut especially of the stomach, was found highly inflamed; and in the latter organ it was softened, and easily detached by the handle of the scalpel; some of the mucus follicles in the ileum and cœcum were ulcerated. From the 10th to the 20th of June, out of the 600 who were present at the fête, 444 were affected; of these 370 were from 20 to 50 years of age; and 74 from 14 to 19 years old. A great many, however, were seized without applying for relief to a medical man, so that we may say that 550 out of 600 suffered more or less from having been at the repast. Nine died, and all those who had severe attacks lost their hair. Several of those who attended the patients, and who slept in the same room, had symptoms similar to those they were attending. The treatment consisted at first in the employment of an emetic, then of chlorated water, of leeches to the abdomen, and mercurial frictions.

The symptoms above described bear a strong analogy to those of typhoid fever, in their *course*, their post-mortem appearances, and in their contagious nature.

DRAUGHT OF TARTARISED SULPHATE OF QUININE.*

℞ Sulphate of quinine . 1 gramm.
Tartaric acid . . 1 *id.* 30 centigr.
Distilled water . . 180 *id.*
Syrup of peppermint 260 *id.*
Mix and dissolve.

M. Lacava speaks highly of the association of sulphate of quinine with tartaric acid, and particularly of the foregoing formula, which has been extolled hy M. Righini.

This draught, which is indicated in all the cases in which sulphate of quinine itself is given as an anti-periodic, is administered in three doses, an hour apart. The first dose should be given at the commencement of the apyrexia, that is to say, as far as possible from the next fit.

INJECTIONS OF INFUSION OF CUBEBS IN VAGINITIS.†

BY M. PIORRY.

A WOMAN, aged 28 years, was admitted to the *Hôpital de la Pitié*, on the 5th of April, 1842, to he treated for intense uretro-vaginitis, which had lasted nine months, accompanied hy violent pain and abundant flooding. This woman had been subjected to several modes of treatment, without success. From the time of her admission, she was suhmitted to injections, repeated six times a day, of 30 grammes of cuhehs in a quart of water. At the end of two days, the flooding had diminished, hut the inflammation of the urethra remaining, cubebs were administered in the dose of three grammes every hour.

On the 17th of April, the patient was completely cured; there was no longer either pain or flooding. Thus, in twelve days, one of the most rehellious of diseases, of nine months' standing, was cured.

M. Piorry states, that he has, in numerous instances, very successfully employed cubehs in inflammation of the canal of the ureter, or of the vagina. Setting out with the principle that cuhehs act only when put in contact with the diseased mucus membrane, hy means of the urine, when the ureter is affected, or hy means of injections, for the vagina, M. Piorry administers this medicine internally, or by way of injection, at very short intervals.

CASE OF POISONING BY SQUILLS.*

BY DR. WOLFRING.

A LABORER, aged fifty-eight, affected with hernia, and weakened as much hy insufficient nourishment as by distress and fatigue, became dropsical, although the disease could not he traced to the existence of any internal cause whatever. The feet, ahdomen, and scrotum, hecame the seat of considerable tumefaction; the thoracic cavity, however, did not present any serous effusion. The physician recommended for ordinary drink a strong diuretic infusion, and also prescrihed, for internal administration, bitter extracts associated with acetate of potassa. Under this treatment the dropsy disappeared.

At the end of some months, during very hard work, in the open air and in damp weather, his feet again hegin to swell. This recurrence of the symptoms rendered the patient sulky, and as he had not ohtained a complete cure by following medical advice, he this time had recourse to an old woman, who promised him entire recovery. This woman directed him to take a vinous tincture of squills, and the poor fellow followed this direction only too closely. He procured some squills cut in small pieces, which he digested for forty-eight hours in about 280 grammes of white wine; he took, at one

* *Journal de Chimie Médicale*, Sept. 1842.
† *Gazette des Hôpitaux*, May, 1842.

* *Medicinische Correspondenz-Blatt bayerischer Aerzte*, No. 5, 1842.

dose, a half of this mixture, and as he very soon after experienced violent griping, he thought it was necessary to increase the dose to produce the expected effect;—he took a few more spoonfuls. Extreme nausea, and more and more violent colics were the immediate result of this increased dose; however, the patient waited for the salutary effect of the medicine, but without taking any more.

At the end of twenty-four hours, during which time the pains and nausea had not ceased for a single moment, without, however, giving rise to vomiting, the physician was called in. He found the face of the patient red and burning, but the feet and hands almost cold; the pulse was small and contracted, the abdomen was so tender that the slightest covering could not be borne.

As the nausea still remained, an emetic was administered, but without the least success. Mucilaginous and oily preparations were administered in a great quantity, and without any interruption, but all these means were ineffectual. The pains had almost entirely ceased on the second day, when he expired. A *post mortem* examination was not permitted.

A chemical examination showed that the vinous tincture of squills, the poisonous agent in question, contained one gramme of extract of squills for every thirty grammes of liquid. The patient had therefore taken five grammes of this extract in the course of one hour.

POMMADE FOR ATONIC ULCERS OF THE LEGS.*

R Dry Tannate of Lead . 30 gram.
Lard . . . . . 8 „
M.

The pommade is spread on pledgets of cotton wadding, and is applied to the ulcers with them.

This medicine has succeeded where others have failed.

ALTERATION OF EXTRACT OF RHUBARB.†

BY. H. REINSCH.

It was long since remarked by Landerer that old mouldy extract of rhubarb has a strong smell of storax. Reinsch has verified this fact with liquid extract of rhubarb covered with a layer of mould, which had been kept close in a flask for 3 years, without being opened. He submitted the liquid extract to distillation; he thus obtained in the receiver a slightly turbid water, with a very large quantity of small drops of oil; the product of the reaction had a strong odor of storax, and a neutral reaction. It was then of a light yellow color, which was removed by agitation with ether; after the evaporation of the ether in a watch-glass, there remained only a few drops of an aromatic oil, with a very strong odor of storax, but which was very volatile, since at the end of an hour it was not possible to perceive any odor on the watch-glass. This odor of storax in mouldy extract of rhubarb arises then from the formation of a peculiar oil. It is probable that many of these oils, whose production is due to the putrefaction of vegetable matters, will be discovered. This oil of rhubarb might then be analogous to the oil with the odor of musk, discovered by M. Rossignon, in spoiled apples, which he called *moloïle*, and which is formed of C H N O.

* *Journal de Chimie Médicale*, July, 1842.

† Buchner's *Repertorium für die Pharmacie.*

EMPLOYMENT OF VERATRINE IN FACIAL NEURALGIA.*

BY DR. LE CALVE.

It is known that veratrine, an organic salifiable base, extracted from the seeds of the *veratrum sabadilla*, and from the bulbs of the *colchicum autumnale*, is a violent poison, which, apparently, should be ranked among acro-narcotic poisons, and which, hitherto, has been of but small therapeutical value.

It appeared to Magendie, who tried it on man, that it acted energetically on intestinal contractility, in such a manner as to provoke the expulsion of fæces, in habitually constipated old men.

Dr. Barsley, who has lately examined the properties of this agent, gave acetate of veratrine, in the dose of a half of a grain (French) at first, then 2 grains, *per diem*, at several times. He obtained marked advantages from its employment in dropsy, chronic rheumatism, and gout. Such were the only cases in which, and still very rarely, veratrine was resorted to, when, quite recently, Dr. Le Calvé, of Montpellier, tried it with remarkable success in many cases of facial neuralgia.

The first case which he relates, is too interesting to be entirely passed over.

After a walk in cold and wet weather, Madame N. suddenly felt violent pains in the right side of the head, accompanied by an insupportable sensation of cold. The locality of these pains varied; sometimes they commenced in the eyebrow, and passed into the temple, and the left side of the head; sometimes they were perceived in the teeth of one or other jaw. The patient at the same time suffered acute pains in the

* *Journal de Montpellier*, 1842.

orbit, which she compared to that which avulsion of the eye would have produced.

The conjunctiva was injected during the attack; there was photophobia and delachrymation. Each attack lasted eight or ten hours, accompanied by fever: calm returned with an abundant perspiration.

Every time that Madame N. was exposed to the slightest cold, a fresh attack supervened, and always with increased intensity. The ordinary treatment—antiphlogistics, antirheumatics and narcotics—completely failed. Sydenham's laudanum, alone, in frictions, at all relieved the patient. At last, the attacks became so frequent, and acquired such intensity, that Madame N., not finding any relief from these excruciating pains, looked on death as a blessing. In October, 1838, one evening at supper, she was seized with so violent an attack that she lost consciousness. I immediately prescribed a pommade of veratrine, to be rubbed on the part from which the pain seemed to arise. On the first friction, which was kept up for five or six minutes, all painful sensation disappeared with rapidity, and the patient slept quietly. Next day she was well, and complained only of a sensation of lassitude in her limbs. The pain did not reappear for some months. Since that time, when she has an attack, the same remedy is resorted to, and with the same success. The lady who forms the subject of this operation, now enjoys good health, and has not suffered from the same complaint for two years; but she is still sensible to sudden changes of temperature, and carefully avoids exposure to cold.

Dr. Le Calvé afterwards relates two analogous cases in which he obtained the same success by similar means. He thinks that, in general, those who have employed veratrine without advantage, have prescribed too powerful a dose; sometimes, accidents have arisen from its employment. An *Officier de Santé* of Montpellier, having heard Dr. Le Calvé eulogize this medicament, prescribed it in a case of facial neuralgia, in the dose of sixteen centigrammes to 30 grammes of lard. At the second friction, there supervened a serious erysipelas of the face and scalp, which compelled him to renounce its use.

I have always commenced with five centigrammes, to four grammes of lard, increasing the dose to six, seven and, ten centigrammes.

TREATMENT OF CHILBLAINS IN CHILDREN.*

BY DR. STÖBER.

To remedy this sometimes very painful inconvenience, Dr. Stöber, Professor to the Faculty of Medicine at Strasburg, employs, when the chilblain is recent, of a deep red, and not ulcerated, cataplasms of linseed meal, or bread and water, surrounded with sugar of lead. The cataplasm is kept on all night; in the morning it is removed, bathing the foot in warm water. Under the influence of this very simple treatment, chilblains are often cured in three or four nights.

Where chilblains are old, and where ulceration is threatened, Dr. Stöber bathes the affected parts with tincture of iodine once in every twenty-four hours, and that for several days in succession; or a similar unction may be made with a mixture of equal parts of dilute nitric acid, and distilled cinnamon water, applied once a day with a feather.

As regards ulcerated chilblains, Dr. Stöber, to hasten cicatrization, has recourse to stimulants, and, in preference, to binoxide of mercury (red precipitate) incorporated with lead.

* *La Clinique des Hôpitaux des Enfants*, June, 1842.

BOOKS RECEIVED.

Is Selenium a True Element? Observations on the Connexion of the Elements by their Atomic Weights, &c., and Theoretical Reasons for considering Selenium a Compound. By SEPTIMUS PIESSE. London: R. Hastings.

TO CORRESPONDENTS.

C. T.—will find his question answered by the review of Mr. PARNELL'S work.

W. M. G., WHITEHAVEN—must furnish us with the means of addressing him privately: we cannot answer such questions in THE CHEMIST.

A JUNIOR OPERATIVE CHEMIST.—Which Oxide?

We wrote to Mr. CHAPMAN, of Leicester, and, not having received an answer, we fear that it may not have come to hand, owing to some deficiency in the address given by him.

Our reply to the letter of Mr. THOMAS BROWN, of Portobello, has been returned through the Post Office.

NOTA BENE.—*All Communications and Books for Review must be addressed, "To the Editors of the Chemist, Care of Mr. Hastings, Publisher, 13, Carey Street, Lincoln's-inn Fields, London." Communications must be prepaid, and sent before the 15th of each Month; Books for Review before the 10th.*

THE CHEMIST.

I. CHEMISTRY.

ON ANIMAL CHEMISTRY.

*On the Chemico-Vital Dynamics of Organization, showing the connection of Chemistry and Anatomy; together with the connection of the Functions of Nutrition, Respiration, and the Chemical Composition of the Tissues.**

BY WILLIAM BITTLESTON.

I. THE objects I have had in view in this paper have been to trace, experimentally, certain chemical relations which appear to exist among a certain class of the animal tissues, and to give an analysis of the osseous system, more extended than, I believe, has been done before. The few paragraphs that precede the experiments, constitute a brief narration of the observations that led me to undertake the investigation; and I have placed them here, believing that few persons try experiments without some motives for so doing, but at the same time without attaching any particular consequence to them. They have served me as an introduction to my researches, and will, I trust, in like manner, enable the reader to understand my object.

II. *On the Chemical Division of the Tissues, founded on the Respiratory Process.*—For the present purpose, I am about to view the tissues as composed of two classes: the one as respiratory, or subject to direct chemical action from the oxygen absorbed by the respiration, the other not. The object of this is, the experiments relate to this function in the former tissues, and the distinction in consequence becomes in some degree necessary. For instance, there are a certain class of the tissues, the gelatinous, that do not exist in the blood already formed as the respiratory ones do, but appear to be the result of a secondary formation, from the organization of the respiratory tissues themselves, these latter being distinguished by their arterial, or red circulation, the former by having little or no circulation of arterial blood at all. I think that the natural division of the tissues will be found to be into those that respire, or are subject to direct chemical action, and those that do not respire. It does not materially differ from Bichat's division, but in placing the distinction on other or higher grounds, being that of the respiration, or non-respiration, instead of mucous membranes, and gelatinous tissues only,* as Bichat has done. See his *Descriptive Anatomy*, or his work on the *Mucous Membranes*.

III. *On the two Respirations of the Vegetable and Animal being the inverse of each other.*—The function of respiration may be distinguished into two kinds; one adapted to the vegetable, the other to the animal; the one respiration being the inverse of the other, as the vegetable inspires carbonic acid and water and expires oxygen; while the animal inspires oxygen, and expires carbonic acid and water.† The question, then, that arises, is, what is the reason for this difference in the respiration of the two classes? It appears to be this: the tissues of the vegetable are composed from the elements it inspires, carbon, oxygen, and hydrogen, and it expires the superfluous oxygen. The respiration, or the elements of it, are, then, its food. It obtains, also, a small quantity from the soil by its roots, but the chief is obtained from the atmosphere by its leaves.‡ A small quantity of nitrogen is also absorbed by the vegetable, and elaborated with its other elements into albumen. It is this latter substance which serves the herbivorous animals that live on the vegetable as food, it being

* This paper is considered by its Author to prove that the formation of the cellule and the crystal are founded on the same laws.

[The reason for the Author claiming date from the 15th June last, for this paper is, that it was then out of his possession, ready for publication, the delay not having been occasioned by himself.]—EDITORS.

* Should this distinction, though I have no reason to doubt it, not prove quite correct, it will not affect the law induced at the end of this paper, as that law is proved from experiments, and not the opinions in the paper.

† Numerous experiments, as is known, have established these facts, from the time of Priestley, Hales, Cavendish, Lavoisier, Davy, Sennebier, De Saussure, Dulong, Despretz, &c. &c.

‡ See De Candolle's *Vegetable Physiology*, Vol. I. p. 129.

the most important and chief part of their tissues, already elaborated for them; and as the carnivorous live on the herbivorous, it is from the vegetable that the food and tissues of the whole are derived. It is this, then, that causes the difference in their respirations: the vegetable derives its food by respiration, while the animal has the greater portion of its food, or tissues, eliminated and given up again by the very same process, their respirations being, in respect to their elements, the inverse of each other.

IV. *That the Process of the Respiration in the Animal, is to remove the Tissues gradually away, to make room for the fresh Food to replace them, while in the Vegetable the Tissues remain, the Respiration in it continually adding to them.*—The vegetable in growing increases indefinitely in size as long as it continues to live;* while the animal arrives at its full development in a small period of its existence; the former grows, by a sort of juxtaposition, but yet the inverse of the mineral, being from within outwards, and only by seasons, while the animal is developed continually at every point, and at every organ, for the whole of its tissues appear to be continually removed and renewed, while the vegetable is periodically increased; and it is these two different processes of the growth of their tissues, that demand the difference in their respirations and nutrition. That is, that the food of the vegetable should be obtained, elaborated, condensed and organized, while that of the animal has only to be organized, (having been condensed by the vegetable already in the process of its respiration,) and has then to be elaborated, expanded and carried away, the double process in either of condensing the gases into the food and tissues, and then expanding them again, appearing as incompatible functions in the same class; particularly as the vegetable is provided with external or a sufficiency of solar warmth, to maintain its periodical growth; while the animal has to furnish its own warmth. For the condensing process of the nutrition in the vegetable elaboration appears, if it may be so said, to fix the heat with the gases and elements of its food in the process of condensing them, while the expansive process of the respiration of the animal furnishes it back again for the purposes of the being. However, it must here be observed in explanation, that the animal appears also to condense gas in absorbing the oxygen for its respiration; but if it does condense the gas, it is not into tissues, as the vegetable does, but to remove them away as carbonic acid and water.

V. We arrive, then, at this conclusion; that it is the nature of the tissues, or rather their mode of nourishment and growth, that determines the nature of the respiration. And as it is in the function of the lungs of the animal that the respiration of the whole of its tissues is expressed, as the section of the pneumo-gastrics proves, so it is in each tissue in particular that its peculiar respiration must be studied. I am, therefore, going to give an explanation, from experiments, of the particular respiration of each of these three tissues of the osseous, muscular, and nervous systems; and by which, I believe, I shall be able to connect together their peculiar respirations with the grand one at the lungs, by facts easy of proof, which are not dependent on experiments on the respiration, but on the peculiar nature of the tissues themselves.

VI. *On the Circulatory Function, viewed in relation to the Respiration of the Tissues.*—As it is well known that the blood is the medium, and the arterial system the vehicle of respiration and nutrition to the tissues, I go to borrow what light I can from the operation of its important function, by simply taking the points of agreement, and the information afforded by anatomists; and showing from them that a general law may, in all probability, be deduced from the circulation, agreeing with the peculiar nature and respiration of the tissues, and thus connecting, as I trust I shall, from both observation and experiment, Animal Chemistry with Anatomy.

VII. *Observations of Anatomists on the Circulation for comparison with my own Observations and Experiments on the Composition of the three Tissues of the Nervous, Osseous, and Muscular Systems.*—M. Magendie, in his *Elements of Physiology*, says, "Some estimate that the brain receives about one-eighth part of the whole of the blood that comes from the heart; though it varies considerably, and can be only taken as an approximation."* Sir C. Bell, in his *Anatomy*, says, that "Those who say the brain is a secreting organ, determine it to be so by asserting it requires five times more blood than any other organ."† And in another part of the same work it is said, "Those four arteries (the carotids and vertebrals) alone convey to the brain a fifth part of the whole mass of the blood. This is the calculation made by Walter, and even those who would settle it at the lowest point still acknowledge that the carotids and vertebral arteries receive at least the tenth part of all the blood of the body. The brain, then, which weighs not a fortieth part

* See also Vol. II. p. 974, of De Candolle.

* See Vol. I. p. 233, Magendie's *Physiology*.
† See Vol. II. p. 348, Bell's *Anatomy*.

of the whole body, receives one-tenth of all the blood, a proportion that must occasion surprise."* One thing is learned here for certain, from these authorities, and that is, that the brain unquestionably receives the most blood, comparatively, of any organ of the body.

VIII. Just the reverse of this, or nearly so, appears to be the case with the osseous system, the difference of opinion here being rather as to how little than as to how much blood it receives. Still I believe it will be found to be generally admitted, that it has, though in a small degree, an arterial circulation. Béclard, in his *General Anatomy* (translated by Dr. Knox), gives a description of the blood-vessels of the bones.† And Dr. Bostock, in his work on *Physiology*, describes the bones as being nourished with vessels. It is also Walter's opinion.‡ Sir C. Bell, in his *Anatomy*, has a long article on the vascular nourishment of bone. I shall cite only a sentence from it, which is in perfect accordance with the rest of the article. He says, "When we find the substance of the oldest bone thus full of vessels, why should we doubt its being able, from its own peculiar vessels, to heal a breach or to repair any loss."§ All I wish to show in quoting these passages, and I could easily cite more, is, that there is authority and proof sufficient to admit the skeleton to be a respiratory tissue; that is, in having an arterial circulation devoted to it, and that the tendons, ligaments, and gelatinous tissues present nothing of the sort, they not requiring it, being formed from the respiratory tissues themselves. The osseous system will receive a further elucidation to confirm this before the paper is concluded.

IX. With respect to the circulation of the muscular system, it does not appear to have occasioned difficulties either way, like the osseous and nervous circulations,—and, consequently, it appears to hold a mean between the two. I have merely to notice, in relation to these facts, that the three symmetrical tissues appear to have each an arterial circulation, or respiration and nutrition in a given order to each other, and the question is, what is it in their peculiar nature that determines that order.

X. From attentively considering and examining, and I can safely say many times, the different properties of these three tissues, of the muscular, osseous, and nervous systems, in relation to the obviously different proportions of nutrition and respiration, conveyed to them by the arterial system, I was led to the following conclusions:—That there was an evident relation, although I could not at first define its amount, between these tissues in this respect; that their respiration and nutrition appeared to be in the inverse order of the amount of their unsaturated elements of carbon and hydrogen in each of these three tissues of the osseous, nervous, and muscular systems, taken comparatively; or, it should be said, rather of the oxygen required to saturate those elements in each tissue, taken comparatively, which is their respiration, for that is the element extracted from the air by the lungs; and that their circulation, or respiration and nutrition, was in the inverse order of their densities, as the brain received the most blood, which is the lightest tissue; and the skeleton, which is the heaviest or densest, received the least blood. And consequently, from these facts, there appeared an evident, if not to say striking, relation between the chemical or natural composition of these three systems of organs, their functions of respiration and nutrition, or circulation; but to any exact degree, the experiments published at the time of making these reflections, did not permit me to determine, although they were sufficient to induce me to undertake the experiments described in this paper, in order to settle the question more approximately myself, or be satisfied of its error.* This brings me now to the experiments, having given the reason that led me to undertake them.

XI. *Osseous System. Proportion of Animal to Earthy Matter.*—The part of the bone used for the experiments was reduced to powder and despoiled of fat by alcohol and ether; it was then desiccated in a chloride of calcium bath at 120° to 130°, F. until it lost no more in weight; the platinum crucible in which it was calcined was placed in a large earthen one to keep out the dust of the charcoal fire, the cover of which was always placed on when the fire

* See Vol. II. p. 121, Bell's *Anatomy*.

† See p. 259, Béclard's *Anatomy by Knox*.

‡ See p. 72, Bostock's *Physiology*.

§ See Vol. I. p. 222, Bell's *Anatomy*.

* The chief cause of the differences in the early experiments on organic substances has been the remarkable tenacity of humidity in those substances, and the want of a settled rule for drying them at (as the boiling point or a little more, as all chemists use now). But in works on Physiology, these experiments, considerably in error, are still quoted as though performed by the most recent and improved methods. The same observations apply to the complete combustion of the carbon so much improved by Liebig's method. Yet the albumen is given, in editions not exhausted, with two or three per cent too little of carbon.

required moving. When completely freed from carbon, the calcined powder was weighed, and analysed to sec the amount of carbonic acid left in it, as this was thought to be better than by replacing it with carbonate of ammonia; in some instances, however, both were done. The carbonic acid in each bone was ascertained (the powdered bone having been first prepared and weighed as described for the calcination), in a mercurial gasometer being expelled with hydrochloric acid, and the volume of gas was reduced to 30 bar. pressure, and 60 Fahr. temperature. By this means, what was lost in calcination, was allowed for by the proportion of carbonic acid that each bone was known to contain. The proportion of lime was ascertained by using the calcined bone. It was dissolved in hydrochloric acid, to ascertain the carbonic acid as stated above, and then the phosphate was precipitated with liquid ammonia, and the remaining lime with the oxalate of ammonia; and this latter precipitate, when reduced by calcination to the carbonate of lime, was found to agree very nearly with the carbonic acid which the bone was known to contain. The phosphates consisted of lime and magnesia, which were calcined, in order to perfectly dry them, and then weighed. The phosphates of lime and magnesia were not separated, as the objects of investigation did not require it. The solution was now evaporated to dryness, the hydrochlorate of ammonia driven off with a red heat, and the residue was found to be chloride of sodium, which by a separate process, in those instances where it was adopted, was found as a carbonate of soda, showing that its conversion into a chloride was by the process of separating the phosphates, &c. The process of separating the salts of the bones was not in every case adopted, as my object was to know exactly the amount of animal matter, and carbonic acid, that each bone contained, as being the chief that were concerned in the question I had undertaken to resolve; in ascertaining those two points in particular, greater pains could scarcely, I think, have been bestowed. The densities were taken from other portions of the powdered bones, which were dried and prepared as before described. After accurately weighing and then being placed in distilled water in a specific gravity bottle, under the receiver of the air pump and then exhausted, and after remaining under it until no more air could be extracted, the bottle and its contents were then brought to 60° Fahr. in a basin of water of that temperature, and then weighed, and the density calculated from the ascertained difference of weights of the pulverized bones in and out of distilled water.

XII.

	Animal to Earthy Matter.				Carbonic Acid.			Phosphates.		Densities.	
	grs.				grs.	cub. inch. grs.		calc.		wt. in wat.	density.
Os frontis, adult skull . .	54·6	18·5	36·1	as 1 : 2	25·0	1·83 = 7·325 in 100		36·1	30·3 = 55·6 in 100	35·6 − 61·3 ÷ 25·7	= 2·382
Ditto fœtal ditto . .	40·0	13·6	26·4	1 : 2	26·0	1·87 = 7·223	,,	. .	. .	32·7 − 56·4 ÷ 23·7	= 2·371
Ditto aged ditto . .	62·1	20·5	46·6	1 : 2	24·4	1·78 = 7·329	,,	41·6	35·0 = 56·5 ,,	38·0 − 65·7 ÷ 27·7	= 2·369
Of aged skull, five bones, the occipital, superior maxillary, temporal, parietal, and malar, in equal portions . . .	50·2	16·8	33·4	1 : 2	30·2	2·24 = 7·419	,,	. .	. . .	37·0 − 63·8 ÷ 26·8	= 2·379
Inferior maxillary of ditto .	32·4	10·5	21·9	1 : 2	24·6	1·77 = 7·219	,,	. .	. . .	31·7 − 59·9 ÷ 23·2	= 2·370
Fibula	50·0	16·1	33·9	1 : 2	26·3	1·93 = 7·342	,,	. .	. . .	32·5 − 56·1 ÷ 23·6	= 2·377
Radius	49·4	16·3	33·1	1 : 2	25·3	1·85 = 7·346	,,	. .	. . .	32·0 − 55·3 ÷ 23·3	= 2·369

Femur	61·3	20·3	41·0	1 : 2	28·2	2·08 = 7·390	,,	41·0	34·2 = 55·8	,,	37·6 − 64·7 ÷ 27·1 = 2·387
Humerus	42·4	14·0	28·4	1 : 2	27·5	1·98 = 7·210	,,	28·4	23·9 = 56·6	,,	28·4 − 48·9 ÷ 20·5 = 2·375
Ulna	30·7	10·6	20·1	1 : 2	23·7	1·73 = 7·322	,,	,,	,,	,,	36·8 − 63·3 ÷ 26·5 = 2·385
Tibia	48·3	16·3	32·0	1 : 2	26·2	1·94 = 7·410	,,	,,	,,	,,	35·7 − 61·1 ÷ 25·7 = 2·382
Clavicle	27·5	9·1	18·4	1 : 2	24·1	1·76 = 7·153	,,	,,	,,	,,	30·4 − 52·5 ÷ 22·1 = 2·375
Scapula	39·8	13·2	26·6	1 : 2	21·3	1·57 = 7·273	,,	,,	,,	,,	33·5 − 58·4 ÷ 24·9 = 2·359
Pelvis	65·3	22·0	43·3	1 : 2	29·1	2·13 = 7·320	,,	43·3	37·0 = 56·8	,,	39·6 − 68·4 ÷ 28·8 = 2·374
Vertebræ, 2 lumbar	44·3	14·9	29·4	1 : 2	18·4	1·33 = 7·271	,,	,,	,,	,,	30·7 − 53·1 ÷ 22·4 = 2·364
—— 2 dorsal	29·7	10·1	19·6	1 : 2	23·5	1·71 = 7·281	,,	,,	,,	,,	22·7 − 39·3 ÷ 16·6 = 2·359
Ribs, 2 upper	24·3	8·2	16·1	1 : 2	21·3	1·56 = 7·361	,,	,,	,,	,,	23·9 − 41·2 ÷ 17·3 = 2·375
—— 2 lower	24·5	8·2	16·3	1 : 2	21·6	1·57 = 7·305	,,	,,	,,	,,	25·1 − 43·4 ÷ 18·3 = 2·362
Femur of the ox	65·4	21·6	43·8	1 : 2	24·7	1·79 = 7·264	,,	,,	,,	,,	28·9 − 49·8 ÷ 20·9 = 2·382
—— of the sheep	47·8	16·1	31·7	1 : 2	26·2	1·96 = 7·482	,,	,,	,,	,,	29·6 − 50·8 ÷ 21·2 = 2·393
						20)146·245			5)281·3		10)23·730
				Average of Carbonic Acid		7·312	,,	Ave. Phos.	56·3	,,	Average density 2·373

	Wt. in Wat.	Density.
Tendon, gelatinous tissue	14·6 − 44·0 ÷ 29·4 = 1.496	
Fibrin, muscular ditto	13·21 − 48·6 ÷ 35·39 = 1·373	
Deduction for earthy salts in fibrin reduced it to	. . . .	1·365
Brain of the ox	10·39 − 61·3 ÷ 50·91 = 1·2039	
Ditto of the sheep	10·53 − 59·3 ÷ 48·77 = 1·216	

XIII. The carbonate of lime, by the process described, obtained from the two frontal bones of the aged and adult skulls, was 8·32 per 100, and from the femur and the pelvis, 7·92, averaging 8·12 per 100, and agreeing very nearly with the quantity required for the carbonic acid of those bones. The density of the brain will of course vary, in different brains, but it is the principle on which the density depends that is the most important to be observed, as on that depends the amount of its respiratory elements, that is, the proportion of its unsaturated carbon and hydrogen, for as it is found to contain more of the fatty principles so will it be less dense in proportion, and its respiratory function augmented accordingly. The proportion of fat in the adult human brain, to the albumen, is about one-half or more, according to Vauquelin* and Denis.†

XIV. To ascertain the amount of unsaturated carbon and hydrogen, to determine the amount of respiratory function in the bone and nervous tissue, the gelatine of the bone and this latter were burned with the oxide of copper, as described in Liebig's organic analysis, and consequently the details will not be necessary here, as they were performed precisely as described, and with the same kind of apparatus. The analysis has not been given in the atomic formulæ, as all that was wanted was to know the amount of unsaturated carbon and hydrogen that each tissue contained. I see in Dr. Liebig's recent work on Animal Chemistry, he takes the tissues, &c. in mass, as the analyses of roast beef and bile are both given and referred to, as I have done here with the nervous tissue, which at present, to obtain a general result such as he wanted, and which is the case here, will be found more satisfactory, as well as more simple and clear. In this case,

* See Berzelius, *System of Chemistry*, Vol. VII. p. 17.

† See *Journal of Physiology*, Magendie, Vol. IX. p. 179.

whether the unsaturated carbon and hydrogen be given in hundredths or in equivalents, it will not make them more or less, and it is on this, their proportion, that the reasoning depends. The elements of fibrine have been taken from Dr. Scherer's analysis; and my own of the gelatine of the bone does not materially differ from his of the tendon, although mine was made long before his was published. I merely mention this, because as more and more exact analyses have been brought out since these investigations were undertaken by me, so has my view or inductive law proved more and more correct than I was able at first to do from the experiments then published. The gelatine of several different bones was examined and found in every respect the same. That of the femur was chosen for these experiments.

Grains. Carb. Acid. Carb. gr.
XV. 8·92 gave 16·60 = 4·510 in 100 = 50·560
of water 5·82 = ·646 of hydrogen in 100 = 7·242
9·46 gave 17·16 = 4·680 = 49·470 in 100
of water 6·00 = ·667 = 7·050 in 100

Grains. Cub. In. Grains. Nitrogen.
3·10 gave 1·80 of nitrogen = ·544 = 17·510 in 100
2·32 ,, 1·29 ,, ·392 = 16·820 in 100

Carbon.	Hydrogen.	Nitrogen.		
50·560	7·242	17·510	Carbon 50·015	Gelatine of the femur. Average of the two experiments.
49·470	7 050	16·827	Hydrogen 7·146	
			Nitrogen 17·168	
2)100·030	)14·292	)34·337	Oxygen . 25·671	
50·015	7·146	17·168	100·000	

XVI. The next experiments are the results of the combustion of the dried brains of the ox and the sheep, made with the view of ascertaining the amount of unsaturated carbon and hydrogen in them.

Ox's Brain.

Grains. Carb. Acid. C. gr.
7·390 gave 17·175 = 4·685 = 63·398 in 100
H.
of water 5·430 = ·604 = 8·173 ,,
8·14 gave 18·760 = 5·116 = 62·868 ,,
of water 6·350 = ·706 = 8·675 ,,

Nitrogen.
Cub. In.
3·76 gave 1·39 = ·4227 = 11·224 ,,
4·13 ,, 1·33 = ·4034 = 9·768 ,,

Carbon.	Hydrogen.	Nitrogen.
63·398	8·173	11·224
62·868	8·675	9·768
2)126·266	)16·848	)20·992
63·133	8·424	10·496

Average of Experiments.

Carbon	63·133
Hydrogen . . .	8·424
Nitrogen	10·496
*Oxygen	17·947
	100·000

Sheep's Brain.

Grains. Carb. Acid. C. gr.
9·23 gave 21·12 = 5·76 = 62·153 in 100
H.
of water 7·31 = ·813 = 8·810 ,,
7·69 gave 17·39 = 4·74 = 61·831 ,,
of water 6·61 = ·734 = 9·562 ,,

Nitrogen.
Cub. In.
3·35 gave 1·12 = ·3391 = 10·131 ,,
3·65 ,, 1·13 = ·3520 = 9·652 ,,

Carbon.	Hydrogen.	Nitrogen.
62·512	8·810	10·131
61·831	9·562	9·652
2)124·343	)18·370	)19·783
62·171	9·185	9·891

Average of Experiments.

Carbon	62·171
Hydrogen	9·185
Nitrogen	9·891
*Oxygen	18·753
	100·000

* The small proportions of sulphur and phosphorus ar e included in the amount given for the oxygen, and the incombustible salts amounting to 1·25 per cent. have been de-

XVII. The first thing that will be given in the table or summary will be the amount of the unsaturated elements of carbon and hydrogen of each tissue, to express the relative amounts of the respiration of each. The next thing will be to show that the densities of the tissues are in a certain ratio to each other, and then that the amount of respiration or oxygen required, is in the inverse of these ratios of the densities. So that in proportion as the tissues occupy a less space, from their increased weight or density, so is less oxygen required to remove them, as their vitality, or use, as tissues, ceases, in the form of respiration, that their place may be re-occupied by the newly vitalized food. So that these two functions of nutrition and respiration, the counterparts of each other, may, not only in every tissue, but with tissue and tissue, organ and organ, as well as member and member, harmonize together and maintain the symmetry of the whole by a general law of organization, characteristic of the wisdom and foresight of the Creator, in a similar manner to that in which we find that He always guards and regulates all His works.

XVIII. Animal matter of bone, $\frac{1}{3}$ of 100.

		Muscular fibrin.	Nervous tissues of Ox and Sheep.	
Carbon	16·671	54·526	63·133	62·171
Hydrogen	2·380	7·007	8·424	9·185
Nitrogen	5·722	15·814	10·496	9·891
Oxygen	8·557	22·653	17·947	18·753

Unsaturated elements in Ox brain.

Carbon	63·133 = 168·352	of oxygen.
Hydrogen	6·181 = 49·448	,,
Total required	217·800	,,

Unsaturated elements of the Bone.		Unsaturated elements of the Muscle.		Unsaturated elements in Sheep's brain.	
Carbon	16·671 = 44·456 of oxy.	54·526 = 145·400 of oxy.		62·171 = 165·784 of oxy.	
Hydrogen	1·311 = 10·488 ,,	4·176 = 33·408 ,,		6·841 = 54·728 ,,	
Total required	54·944 ,,	178·808 ,,		220·512	,,
				217·800	,,
				2)438·312	,,
			Average of both	219·156	,,

RESPIRATION OR RATIOS OF OXYGEN REQUIRED BY TISSUES.

Oxygen required for the osseous tissue to that of		Oxygen.	
the muscular	as 54·944 : 178·808	or 1 : 3	nearly
,, ,, nervous	54·944 : 219·156	or 1 : 4	,,
,, muscular ,, nervous	178·808 : 219·156	or 4 : 5	,,

DENSITIES, OR RATIOS OF SPACE OCCUPIED IN THE TISSUES.

	Densities.			
Density of osseous tissue	2·373	× 2·373	=	5·6311
,, muscular ,,	1·365	× 1·365	=	1·8632
,, nervous ,,	1·216	× 1·216	=	1·4786

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ducted, so as to make a considerable approximation towards knowing, in a piece of tissue of a given character, as to density and chemical composition, what is the actual amount of oxygen of the air or respiration required to decompose it into carbonic acid and water again; the state from which it came, and into which all the tissues as they lose their vitality are decomposed continually, as they are replaced by the fresh, or newly vitalized food. This general law on the respiration as regards the relation of tissue to tissue, in respect to this function, could never be given from direct experiments on the respiration at the lungs themselves, however that organ may be the general expression of the function, and may tell us, from experiment, into what the tissues are decomposed taken together, but their relations to each other on these important functions of respiration and nutrition, taken separately, must be discovered trom an examination of the tissues themselves; at least that is the impression on my mind, and it is with that view that these experiments have been made. I am going now to show the relation, from these experiments, that they appear to hold to each other, and from the table or summary of the experiments to deduce the law.

	Ratios of Densities.					
Osseous to muscular tissue	5·6311	:	1·8632	is as 3	:	1
,, nervous ,,	5·6311	:	1·4786	,, 4	:	1
Muscular to nervous ,,	1·8632	:	1·4786	,, 5	:	4

If the reader will now take the trouble to compare the ratios here of the densities, he will find them just the reverse of the amount of oxygen required for the respiration of the tissues; that is, making considerable approximations, as near as experiments generally approach.

COMPOUND RATIOS OF DENSITIES AND OXYGEN.

	Oxygen.		Densities.	
Osseous to muscular tissue	54·944 : 178·888	::	1·8632 : 5·6311	as musc. is to oss. tissue.
,, nervous ,,	54·944 : 219·156	::	1·4786 : 5·6311	uervous ,,
Muscular ,,	178·808 : 219·156	::	1·4786 : 1.8632	,, to musc. ,,

XIX. *The Induction, or Law, deduced from the foreyoing Experiments, Reasoning, &c.*—Regarding the unsaturated elements of carbon and hydrogen as expressing the amount of the respiration of each tissue, and looking upon their densities as the expression of their dynanisms; and as these when squared are shown to be in ratios in the inverse order of the amount of respiration or oxygen required by each tissue to saturate their unsaturated elements, taken respectively in that order, in a fair degree of approximation; there has been shown, it is considered the relation of tissue to tissue of the nervous, muscular, and osseous systems. And likewise in thus showing the relation of the respiratory and nutritive functions (of those tissues) they being the counterpart of each other, as has been shown (III.), and as the arterial system is formed upon those functions, as the observations of surgeons * show, its regulation, it is presumed, will also be explained by the same law: that is, the areas of the arteries will be found to make a considerable approximation towards being in the inverse ratios of the densities, or dynanisms of the organs, as the dynanisms of the organs are found to be in a grand approximation to the inverse ratios of the respiration or oxygen required by the unsaturated or respiratory elements in them. And, consequently, as the organs determine the amount and the nature of the respiration; and as the respiration of each tissue determines the amount of nutriment required to replace the loss or waste resulting from respiration; these together, as is shown in the variation of the areas of the arteries, during growth, to suit the changes in the organs, determine their areas to be in the inverse ratios aforesaid of the densities, or in the direct ratios of the respiration and nutrition; the observations of anatomists (given in VII., VIII., and IX.) go towards showing such to be the case, as regards the tissues of the osseous, muscular, and nervous systems.*

June 15, 1842.

* See *Guthrie on the Diseases and Injuries of Arteries*, pp. 248, and 354 to 364, on the accommodation that the arteries exhibit to the demands of the nutrition of the organs, during changes in them as respects growth, or amputation.

* The inferences that must naturally flow, supposing the above law to be correct, and I consider it is at least rendered probable, are too numerous, and it would be out of place now to enumerate them here. They will, therefore, furnish matter for further investigation, which, if conducted on the principles laid down in this paper and from those by Dr. Liebig in his recent work on *Animal Chemistry*, by combining the two, that is, the chemical formulæ for showing the possible transposition of the elements, and using the densities to control or prevent the ideas on those changes from being fanciful, which they otherwise must be, for it is the distances or densities of the moleculæ that control the chemical changes of the substances. We shall then have investigations, carrying within themselves the entire rationale of their own process, and controlling and correcting the hypothesis that they naturally give rise to; and a system of chemico-dynamical philosophy applicable alike to organization, and the substances that enter into it, and not as it has been formerly applicable only to the one, those that enter it and not to organization itself. For it never can be the case, till the substances that enter it, and till organization itself, are found to have some relation in common in their operations, that any data or law of any value can be derived that may guide us in our future investigations. To me, admitting that I am right so far as I have gone, the subject begins to unfold itself with a remarkable simplicity, to what I had expected when I first attempted these investigations; and I mention this, that others may be induced to lend their aid in developing views, which in the humble opinion of the author of this paper, must be attended with the most valuable results.

Law on Dynamics of Chemistry.

My object in this paper is to show, that the chemical attraction of bodies, such as the metals, sulphur, nitrogen, &c. &c., is analogous to the general law of attraction or dynamics, which is in the direct ratio of the mass, and in the inverse ratio of the square of the distance. For instance, if density is admitted to express the relative distances of the contained moleculæ of bodies, it will follow that the squares of the densities will be the same, relatively, as the squares of the distances of the moleculæ; such, for instance, as in the isomorphous crystals, where the moleculæ are the most equiangularly arranged, the distances will be the most equal and the most easily comparable—that is, the squares of their densities will be in ratios to the elements that differ in the isomorphous crystals, as they will express the controlling or dynamical masses, and the densities the relative distances.

The following law on the Dynamics of Chemistry (a paper also by the Author of this on the tissues) is cited here, as shewing that the same law of dynanism that presides over the tissues, presides also over chemical molecular attraction generally, as has been shewn to be the case by proving it on the isomorphous crystals. Another circumstance also may be added, and that is, that all the chief microscopical observers agree, that organization commences from a cellule, and its different parts, and tissues, from a succession of cellules, and consequently if this be correct, the tissues may be said to have an isomorphous origin, and to be based on physical principles, the same as the crystals: with this simple distinction that the crystal is formed from without, and the cellular structure from within, while the porosity of this latter and its vascular system in the animal enable it to present all the phenomena of continual change, which in the mineral occur but in such a feeble degree, and are confined to only a few crystals, and which may be ascribed to their difference of structure and growth, but not from a difference in the dynamical law of the moleculæ themselves.

For instance take the nitrate of soda and the nitrate of silver, the crystals of which are isomorphous, it is the silver and the sodium only that differ, the nitric acid and oxygen, except the two metals, being alike in both crystals. It is then the sodium and the silver, that constitute the controlling elements, simply because the crystals of each are in ratios of densities to the proportion in which the silver and sodium are found to exist in those crystals, and which is as 24 is to 108, as 4·742 is to 18·966, and which is also their atomic values, and consequently the law of their dynanism,

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There is some further observation required, on the experiments that have been published, particularly on the osseous system, as they differ more from each other among themselves, as well as from my experiments; for I have found this system, in the skeleton, skulls, and bones of the ox, and sheep, analysed by me, remarkable for the uniformity in its composition. But it must be observed, if the osseous system contained (as represented in some of the published experiments,) two-fifths of animal matter, instead of one-third, that I have found it to contain; should it be so, in those cases it would not disprove my law, because it would, as may be shewn, be less dense with less earthy matter and contain in proportion more unsaturated elements of carbon and hydrogen, so that it would still be in ratios to the other tissues, to prove the law, though not in exactly the same ratios. The same reasoning applies equally to the brain, for the more or less of the fatty principles it should contain, it will be more or less dense, and contain less or more unsaturated carbon and hydrogen in proportion, so that its ratio also, like the osseous system, would still be maintained, and the law would be proved, only not exactly by the same ratios. I must therefore warn the reader of this paper, because he can muster analyses that differ from mine, not to be hasty in consequence, in rejecting the law I have induced in it, as he will find, perhaps, that it exists as a principle beyond the limits of organization, and enters pretty generally into all the phenomena that surround us, as well as into the varieties and variations in organization itself.

It is merely referred to in this note, that if the dynamical molecular law, here attempted to be proved for the tissues, be correct, it at once gives the general law for the dynamical action of all the substances taken into the body, whether in the shape of food, or of medicines, for their action will be, that produce a general effect, precisely in the ratio in which they differ from the composition of the tissues themselves, and in amount of effect according to the time and quantity absorbed, for the amount of effect and its specific nature must not be confounded together. This is as much as can be said in a note on this important point, as it is the author's intention to furnish a second part to this paper on the principal medicines in relation to the laws of the tissues contained in the first part.

or power of attraction in controlling equal elements is in the ratio of their atomic proportions. Or in other words the atomic proportions express the controlling masses, and the densities the relative distances of those masses.

Now, as the densities of bodies are, or can be shown to be, in the direct ratios of the amount of matter contained within them, that is, in the same space, according to the difference in their respective weights, or in other words, in the inverse ratios of their respective bulks, so will the squares of those densities, on the same principle, be also relatively in the inverse ratios of the distances of their contained moleculæ; and, consequently, if it can be shown by squaring the densities of the isomorphous crystals, those being the most simple of comparison from the equable distance of their contained moleculæ, that the variable elements in them are found to be in ratios to the squares of their respective densities, or, which is the same, to the squares of the relative distances of their contained moleculæ, when compared to each other, it is presumed that the general law of dynamics or attraction, as before cited, which is in the ratio of the mass, and in the inverse of the distance, and the dynamical law of Chemistry will be shown to be analogous, without pretending to say they are the same in principle. Our want of knowledge of the essence or principle of dynamism itself, is such as to render the saying of such a thing impossible, though the author of this paper believes, that as all power must have emanated from the Creator himself, however it may be displayed to us in different ways, its source is at least unique, and always the same.

The reader of this, who may think I am retrograding, in not having given this investigation with the atomic formulæ, had better suspend his opinion till he hears the reason for not doing so. In the first place, and chiefly, it is this, the tissues are not enough advanced in their chemical analysis, to do it from their natural composition, and I do not feel at liberty to do it from any thing else, as no artificial way of saturating them in the laboratory can be certain of being right, or of giving a correct formula that will apply to their natural composition. For instance, the formula adopted for the gelatine, will not without an alteration apply to the composition of the bone, though it forms one-third part of the osseous structure. And until there is a complete analysis of the brain, of its different compound elements, after the method of Fremy, and the law shown by which those compounds vary in proportion to each other, which law is not at present known, for Fremy's analysis, good as it is, does not at all give that law; I must say, that no formulæ for those compounds of the brain can be of any value. For the formulæ must not only agree with the atomic proportions, and the compounds of themselves, but likewise in relation to their composition to each other in their varying proportions, as also to the secretions or decomposition of the organ. The formulæ and laws that regulate all these changes, must coincide, and explain each other, which they will do when they are really understood. For these reasons I have not attempted to give what is not for my view actually required, as it can be proved as well without, and would, it is more than probable, have to be altered, as it would be an artificial, instead of a natural formula. This is not written without my having reflected and tried to do it, but, being satisfied that it was premature, it was abandoned.

THE ACTION OF FLUORIDE OF POTASSIUM ON IODINE, &c.

BY REUBEN PHILLIPS.

Iodine, added to a solution of the fluoride of potassium, manifestly causes some chemical action from the depth of color which the solution assumes. In order to study this re-action, I prepared a perfectly neutral fluoride of potassium by the decomposition of the double fluoride of potassium and hydrogen by exposure to a red heat in a platinum crucible; the salt was placed in a glass retort with some iodine, and a quantity of water added sufficient to form a moderately strong solution; the retort was connected with a tube condenser, and the materials in the retort kept in a state of ebullition: as soon as the iodine had passed over into the condensing tube, it was returned into the retort. This was continued about two hours to ensure the completeness of the action. The operation was stopped after all the iodine had been distilled off.

The solution in the retort, when tested, yielded iodine with the hydrochloric acid; and gave a yellowish precipitate with the nitrate of silver. It changed the peroxide of mercury into the periodide—a property which I find belongs to the iodide of potassium. A few grains of the precipitate given by the nitrate of silver was washed and carefully and imperfectly dried; it was then introduced into a glass tube sealed at one end; the end of the tube containing the substance under examination was heated by a spirit lamp; water first distilled, the mass then became highly colored, effervesced, and, finally, sank down in a state of semi-fusion. A minute quantity of some compound of

fluorine was disengaged, forming an irregular dulled zone on the interior surface of the tube; the quantity of fluorine present could not be estimated higher than below the $\frac{1}{100}$ of a grain, and as the mass of materials which afforded this quantity weighed at least 6 grains, the above small quantity of fluorine cannot be considered in any other light than as an accidental impurity due to an imperfection in the washing, as will presently appear.

The precipitate given by the nitrate of silver was partly soluble iu ammonia; that which remained was of a yellowish color, soluble in a concentrated solution of the iodide of potassium, precipitating again unchanged by water, and when heated gave those remarkable changing colors which I have shown to belong to the iodide of silver; it is, therefore, without doubt, iodide of silver.

To determine of what the other compound consisted, some of the materials in the retort were mingled with the nitrate of silver, and the ensuing precipitate after being washed was acted on by a solution of carbonate of potassa assisted by heat. The clear liquid was then decanted and neutrallized by the acetylic acid; nitrate of silver added to this gave a white precipitate soluble in ammonia, which, when washed, dried, and heated, gave a primrose yellow mass, which deepened into an ochre red as the heat increased, exhibiting the well marked characteristics of the iodide of silver. If this residuum contained nothing but iodide of silver it should not be soluble in ammonia, to ascertain which I poured a few drops into the small platinum capsule in which the salt had been ignited, when there appeared some slight signs of solution; to determine this point the liquid was poured into a glass capsule, and the fluid drop carefully evaporated; there was left a small quantity of matter, thus proving that a solution had been effected. The remainder in the platinum capsule was dried and found not to be acted on by hydrochloric acid. But, to account for the solution effected by ammonia, is iodide of silver soluble in ammonia, although stated not to be so? Experiment soon decided:—On some well washed iodide of silver, prepared from a solution of the nitrate by an excess of iodide of potassium, I added a concentrated solution of ammonia; as I could not obtain a clear solution by allowing the insoluble salt to subside, the mass was warmed and filtered, a few drops of the filtered fluid evaporated in a glass capsule left a notable quantity of the iodide of silver; nitric acid turned this solution milky, and a solution of the sulphuret of barium occasioned a blackening. Iodite of silver appears to be more soluble in warm than in cold ammonia, as the warm filtered solution became slightly opaque by cooling, and became perfectly transparent on again regaining its former temperature.

This does not much interfere with the ordinary means of discriminating between the iodide, chloride, &c., of silver, by means of ammonia, as the quantity of iodide dissolved is comparatively insignificant; however, chemists will guard against placing too much reliance on this mode of separation.

But to return: previous to ignition this insoluble silver salt evolved chlorine with hydrochloric acid. The properties taken together belong to no known substance but the iodate of silver. That the iodine in the iodite of potassium contained in the original solution is equal to the oxygen in the iodate, equivalent to equivalent, is evident from the fact that the solution in the retort when mixed with hydrochloric acid precipitates iodine, a very small quantity only remaining in solution; that the solution so treated contains no iodide of potassium will be more evident from the following: A portion was boiled to expel the small quantity of free iodine which it contained, and treated with nitrate of silver the precipitate was totally soluble in ammonia; it therefore contained either no iodide or an exceedingly minute quantity.

This explains the reaction between the fluoride of potassium, iodine, and water; 12 equivalents of the fluoride of potassium, 6 of iodine, and 6 water, arrange themselves into 6 equivalents of the double fluoride of potassium and hydrogen, 5 of the iodide of potassium and 1 of the iodate of potassa, as in the following equation:

$$12\,KF + 6\,H\,O + 6\,I = 6\,KF_2\,H + 5\,KJ + 1\,KJO_6.$$

The above reaction appeared to take place more readily with a moderately dilute solution than with a very concentrated one, but the compounds once obtained may be brought to dryness, as in the following; in a platinum vessel, a solution of fluoride of potassium was mingled with iodine boiled for some time and evaporated; a portion of the saline mass treated with hydrochloric acid gave much iodine. The residue was more strongly heated when iodine was disengaged; this evolution of iodine can be due only to the reproduction of the fluoride of potassium.

The following proves that the transformation of the fluoride of potassium into the double fluoride is essential to this reaction, and that the double fluoride itself has no action on iodine and water:—Hydrofluoric

acid was added to the fluoride of potassium and the solution evaporated by a gentle heat. To a solution of this substance iodine was added, and the whole heated, but no more iodine was dissolved than would have been by as much water, as was proved by the faint color of the solution, therefore no iodide of potassium was formed; but to be more sure the iodine was dissipated by boiling, and the resulting solution tested with nitrate of silver, but no iodide of silver appeared to be formed, but a certain quantity of a light bulky precipitate ensued; this rendered the test fallible. Some of the double fluoride was again boiled with iodine in a platinum vessel, and the whole brought to dryness to expel the iodine; the saline mass was then treated with nitric acid, afterwards diluted with water and starch added, but no iodide of starch was formed. I am unable to account for the appearance of the above flocculent precipitate; it is perhaps a double fluoride of silver with potassium or hydrogen.

THE ACTION OF FLUORIDE OF POTASSIUM, ON SULPHUR AND WATER.

Fluoride of potassium in solution was boiled with sulphur, but judging from the color of the mass no sulphuret was formed, I then added a small quantity of carbonate of potassa, supposing that the double fluoride might act on the sulphuret of potassium, and that if any body was present with which the hydrofluoric acid might combine, maintaining a neutral fluoride, the action might be set up. However, I met with no better success, for the addition of acetate of lead caused no blackening.

THE ACTION OF LIME AND ITS CARBONATE.

Carbonate of lime was added to a solution of the fluoride of potassium, the substances were allowed to remain in contact for a few minutes and then filtered; the filtered solution, which was perfectly clear, effervesced powerfully with dilute hydrochloric acid, thus showing that an alkaline carbonic had been produced.

Hydrate of lime suspended in water was added to a solution of the fluoride of potassium; after a few minutes had elapsed the fluid was passed through a filter, the clear fluid saturated with an excess of hydrochloric acid and treated with the chloride of calcium gave no precipitate, but gave a copius one when no hydrochloric was added, proving the solution contained no fluoride; to verify this the remainder was brought to dryness in a metallic vessel, when the addition of sulphuric acid caused the evolution of no hydrofluoric acid.

THE ACTION OF THE CHLORIDE OF AMMONIUM.

The powerfully alkaline tendency of the fluoride of potassium, rendered it probable that it might displace ammonia from its saline combinations. To prove this, the chloride of ammonium was placed in a test tube with the fluoride of potassium, and a small quantity of water added; the whole, on being boiled, copiously evolved ammonia. This experiment was confirmed by triturating together in a mortar, fluoride of potassium and the chloride of ammonium. This re-action, which has not been further studied, depends on the strong tendency of the simple fluoride of potassium to resolve itself into the double fluoride of potassium and hydrogen.

THE ACTION OF FLUORIDE OF POTASSIUM ON OIL.

To determine this re-action, fluoride of potassium was placed in a metallic vessel, and so much water added as to form a concentrated solution; a small quantity of olive oil was added, and the whole boiled for about half an hour. The oil not being then dissolved, the solution was diluted and filtered through moistened paper to retain the oleaginous particles; the addition of sulphuric acid to the filtered fluid occasioned no appreciable separation of the oily acids. A dilute solution, similarly treated, yielded the same negative results.

THE ACTION OF THE FLUORIDE OF POTASSIUM ON BONE.

A concentrated solution of the fluoride of potassium was allowed to remain for some days in a metallic vessel, containing some fragments of bone previously exhausted by boiling water. The fluid, treated with a solution of the nitrate of silver, gave an abundant yellow precipitate; a small quantity of nitric acid added caused its immediate resolution, with the exception of a slight milkiness. These data are conclusive as to the existence of a soluble phosphate in the solution.

MEMOIR ON A NEW COMPOUND OF ARSENIC ACID AND BITARTRATE OF POTASSA, UREA, AND ALLANTOIN.*

BY M. J. PELOUZE.

NEW COMBINATION OF ARSENIC ACID AND BITARTRATE OF POTASSA.

EMETIC, or double tartrate of potassa and antimony, has of late years become the subject of many interesting observations. Mits-

* *Comptes Rendus*, No. X. Sept. 5, 1842.

cherlich has shown that the oxide of antimony in this salt may be replaced by arsenious acid, and that the crystalline form of both compounds is the same. Soubeiran and Capitaine obtained an emetic with iron; and Hagen substituted ammonia for the potassa. These facts prove the existence of a series of emetics analogous to the series of alums.

The new compound of which I am about to treat does not belong to the class of the preceding emetics: the oxide, or acid containing 3 eq. of oxygen, is here replaced by an acid containing 5 eq. It is obtained by dissolving arsenic acid in 5 or 6 times its weight of water, adding finely powdered cream of tartar. Combination immediately takes place: boiling facilitates it. The limpid liquor, containing an excess of arsenic acid, precipitates, on cooling, the new emetic; but it is better to pour alcohol into the clear liquor. A white powder, which is sometimes amorphous, and sometimes crystalline, is deposited. It is quickly washed in alcohol, then exposed to the air and dried.

Its formula is $C^4 H^2 O^5 K O, C^4 H^2 O^5, A_2 O^5$, SHO. At 130° C., it loses 5 eq. of water.

Dumas and Liebig observed that anhydrous emetic, exposed to a heat of 220° C., loses the elements of 2 eq. of water. I thought it possible, and to a certain extent probable, that in replacing oxide of antimony by a more oxygenous body, as, for example, antimonic or arsenic acid, heat might cause an elimination from the latter compounds of a greater quantity of water than in the preceding case; and this circumstance might become very important in the discussion of the theories, of which the constitution of tartaric acid has been the subject. However, this expectation has not been realized by experiment. Arsenic emetic, exposed to the action of a gradually increasing heat, after having lost its 5 eq. of water of crystallization, soon becomes deeply colored, and gives out an odor of caramel mixed with the odor of alcarsin, and, whatever precautions I have taken to remove more water, without destroying the salt, I could not succeed.

Arsenic emetic is very soluble in water, but the solution is very unstable. Left to itself, it is gradually destroyed, and deposits crystals of cream of tartar, while the arsenic acid remains dissolved.

An excess of arsenic acid prevents this decomposition, and renders the compound much more stable. Alcohol precipitates it from its aqueous solution with an uniform composition, when it is mixed with an excess of arsenic acid.

I have not yet further examined arsenic emetic.

UREA.

M. Regnault's experiments have taught us that urea, like ammonia and the vegetable alkalis, in combining with the oxy-acids, forms salts, into the composition of which there constantly enters 1 eq. of water. The lactate of urea is the only exception to this rule. MM. Cap and O. Henry regard this salt as formed of 1 eq. of urea and 1 eq. of an hydrous lactic acid, without water, of combination. They did not make a direct analysis of it; but they deduced its composition, from the quantities of lactate of lime and oxalate of urea necessary for producing the double decomposition of these two salts. On repeating the calculations which served them as a basis, they appeared to be erroneous; and, consequently, nothing can be concluded from their experiments, as to the composition of lactate of urea. Besides, if these calculations were accurate, their method is not sufficiently precise for the solution of the question.

I therefore tried to prepare pure lactate of urea, in order to analyse it by the ordinary method of the combustion of organic matters.

Urea was dissolved in water, and put in contact with a slight excess of lactic acid. The liquor, evaporated at the ordinary temperature *in vacuo*, deposited white crystals, which, to my great astonishment, I found to be *pure urea:* they had its composition and all its properties.

Although this experiment led me strongly to doubt the existence of lactate of urea, I wished to try to prepare it by double decomposition. I carefully decomposed lactate of lime by oxalate of urea. The liquor, freed from oxalate of lime by filtration, should have contained lactate of urea. It was acid; I evaporated it *in vacuo:* it remained viscous, and as powerfully acid as lactic acid itself. It deposited numerous needles, which were nothing more than urea.

From these two experiments, I should conclude that lactate of urea does not exist, or at least that it is not formed by the above means; and that MM. Cap and Henry have mistaken for lactate of urea nothing more than pure urea, or urea simply impregnated with lactic acid. It is evident, that the double decomposition of lactate of lime and oxalate of urea does not necessarily imply the formation of lactate of urea; and that the acid and base, which would constitute that salt, if it existed, may remain separated in atomic proportions. The same thing is remarked in many other cases; for example, in the decomposition of a salt of alumina by a soluble carbonate, carbonic acid is disengaged instead of combining with the alumina.

MM. Cap and Henry, have announced the existence, in human urine, of lactate of urea in considerable quantity; according to

them, the greater part of the urea is found under this form. This is an error which it is so much the more important to rectify as it has already been adopted, as a well-proved fact, by distinguished chemists and physiologists. It must be said, however, that M. Lecann, in a very interesting Memoir on urine which he has published, has contested the opinion of MM. Cap and Henry relative to the presence of lactic acid and urea in the saline state in that secretion, and that he has demonstrated that each of these two substances exists in it in the free state.

According to MM. Cap and Henry, the urine of ruminating animals should contain urea in the state of hippurate of urea, whilst the excrements of birds and reptiles should contain in combination with uric acid.

These two assertions are also unfounded. I have proved that when hippuric and uric acids are dissolved in water, and when urea, in equivalent proportions, is mixed with them, these two acids separate first, in the state of purity, from the aqueous solution, whilst the urine is concentrated in the superstratant liquor, in which it is found in the free state.

When the atomic mixture of hippuric acid and urea is boiled, a portion of the latter substance is decomposed into carbonate of ammonia, and this circumstance is in some measure confirmative of the non-production of hippurate of urea.

I may add that the double decomposition of hippurate of lime and oxalate of urea, furnished only an atomic mixture of urea and hippuric acid.

Urea, it is evident, acts towards certain acids the part of a base; it is to them a true animal alkali; but this base is exceedingly feeble, and hence it is not surprising that with a marked predilection for certain acids, it does not manifest any affinity for others, especially when these acids are bodies in which acidity is but little developed; and such is precisely the case with hippuric and uric acids.

That which I have just said said of urea applies also to the vegetable alkalis, which are, like it, feeble bases. Perhaps there ought to be a revision of some of these salts with alkaloïd bases, and this would be the more useful, as they sometimes are medicines which are extensively employed, as for example, the *hydroferrocyanate of quinine*. At all events, I have proved that this latter substance, of which I have examined several samples obtained at the shops in Paris, is nothing but quinine, containing a little Prussian blue, arising, doubtless, from the decomposition of hydroferrocyanic acid.

After having shown that M. Regnault's experiments on the necessity of the presence of water in the oxysalts of urea are accurate and do not admit of any exception, it was important to examine whether the analogy of urea with ammonia and the vegetable alkalis, would be maintained with relation to the hydracids, and whether it would form with them anhydrous salts without the inervention of water.

This analogy is indeed maintained as regards dried urea and hydrochloric acid. I obtained a salt formed of equal equivalents of these two substances; its composition is consequently, $C^2\ H^4\ N^2\ O^2$, HCl.

Here again, as with certain oxyacids, weak hydracids, hydrosulphuric acid, for example, cannot combine with urea.

Urea presented to me, in its contact with salts which contain water of crystallisation, a peculiarity on which I will dwell for a moment.

Pulverized and mixed with these salts, it immediately separated the water of crystallisation, and the mass, solid as it was, suddenly became soft or even entirely liquid, when the hydrated salt, as the sulphate of soda, for example, contained much water of crystallisation. Urea is not, however, susceptible of combining with water; put in contact with the air, it does not perceptibly attract moisture. Hence it is curious to see it displace, in order to become dissolved in it, the water of crystallisation of certain salts; that is to say, the water engaged in a combination.

Many anhydrous salts, indeed, remove water from hydrated ones; but it is when they are capable of forming new hydrates, which is not the case with urea.

I have studied the action of heat on nitrate of urea: towards 140° C., this salt was decomposed, and disengaged a large quantity of carbonic acid and protoxide of nitrogen, in the proportion of 2 volumes of the first to 1 volume of the second. The residue was composed of free urea and nitrate of ammonia, which had been previously found under these circumstances. This residue was very soluble in water, and deliquescent. The solution deposits successively crystals of nitrate of ammonia and free urea.

The following equation represents the first period of the decomposition of nitrate of urea:

$$4(C^2\ N^2\ H^4\ O^2,\ NO^5,\ HO) = 4CO^2 + 2NO + 2(C^2\ N^2\ H^4\ O^2) + 3(H3\ N,\ NO^5,\ HO).$$

A second period is soon presented.

The nitrate of ammonia is converted into water and a new quantity of protoxide of nitrogen, whilst the urea in its turn gives carbonic acid and ammonia.

I have observed that urea, in the presence of nitrate of ammonia, does not give

cyanuric acid ; whilst the latter acid, if it is alone, resists a very high temperature, before passing to the state of cyanic acid : it is destroyed with great facility when mixed with nitrate of ammonia. I may add that there are few supporters of combustion so energetic as this salt.

During the decomposition of nitrate of urea, a new acid is formed, of which I shall confine myself to noticing the production, on account of the extremely small quantity on which I operated, for only traces of it were produced.

This acid crystallises in small, brilliant, white, or greyish black, laminæ, of a faint taste, clearly reddening litmus paper, and sparingly soluble in cold water, which enabled me to prove its existence, and to separate it from the urea and nitrate of ammonia. Potassa disengages from it ammonia, but only with heat and very slowly. This acid forms, in tribasic acetate of lead, and in ammoniacal nitrate of silver, an abundant white precipitate. Submitted to dry distillation, it gives acid products, and disappears without leaving any residue. It appears to me to be formed of $C^2\ H^3\ N^2\ O^4$; but I must say I can by no means consider this formula as definitive ; and, in speaking of this acid, it is my wish to induce new investigations concerning the dry distillation of nitrate of urea.

ALLANTOIN.

Allantoïn, discovered by Vauquelin and Buniva, in the *liquor amnii* of the cow, has been artificially obtained by Liebig and Wöhler, by acting with peroxide of lead on uric acid. It is to the latter chemists that the knowledge of its exact composition, and of its principal properties is due.

Its formula is $C^4\ H^3\ N^2\ O^3$.

I have submitted it to some experiments, of which I here give the results.

When gently heated with nitric acid of the density of 1·2 to 1·4, it dissolves in it, and the liquor, on cooling, deposits a considerable quantity of fine crystals of nitrate of urea. I have recognized this salt in all its characters, and found it to have the composition assigned to it by M. Regnault, namely, $C^2\ N^2\ H^4\ O^2\ N^5\ HO$.

With hydrochloric acid, the same reaction ; ready formation of hydrochlorate of urea.

No gas is disengaged in either of these re-actions.

The nitric solution of allantoïn, evaporated and dried at 100°, thus taken up by a little water and ammonia, precipitates with alcohol a white viscous matter, which is redissolved in water, and precipitated a second time by spirit of wine to completely deprive it of nitrate of ammonia and urea, which are the only substances with which it is contaminated. This matter is a new nitrogenous acid, whose formula is $C^{10}\ H^7\ N^4\ O^9$.

It is uric acid, plus 3 equivalents of water. It is white, slightly acid, deliquescent, but almost insoluble in alcohol. It gives by distillation a product containing much hydrocyanic acid, and a bulky residue of carbon. Poured into acetate of lead and nitrate of silver, white, bulky precipitates are formed, which are soluble in an excess of those salts, and also in an excess of acid. The precipitate formed in the ammoniacal nitrate of silver is much more considerable than in the neutral nitrate of silver.

This new acid is produced in several other cases besides that of which I speak. It is always formed when uric acid and allantoïn are decomposed by pure oxide of lead. Chlorine, nitric acid, and, doubtless, many other oxidising bodies, likewise yield it by contact with uric acid. On this occasion, I may add, that when chlorine in excess is passed through a boiling solution of uric acid, there is found in the liquor only quadioxalate of ammonia, which is finally decomposed into gaseous products.

Liebig and Wöhler, in their work on the products of the oxydation of uric acid, have proposed a theory intended to arrange and connect a great number of facts which they have observed. According to this theory, urea should be formed in definite proportion at the same time as the allantoïn and the oxalic acid, by the decomposition of uric acid with the pure oxide. But I have always, on the one hand, observed in this re-action, the formation of the new acid ; and on the other hand, I have sometimes obtained allantoïn mixed with small quantities of urea ; and as, besides, the pure oxide converted, even without heat, allantoïn into this new acid and urea, it might be thought that this last substance is the result of the decomposition of allantoïn, which would be formed without urea in the first phase of the decomposition of uric acid.

The new acid, which may be called *allantoric acid*, because, on one hand, it is derived from allantoïn, and because, on the other hand, it has some analogy of composition with uric acid, is formed alone with urea, by the action of the hydrated acids on allantoïn. Indeed, if the allantoïn be subtracted :

$C^{12}\ H^9\ N^6\ O^9$ = allantoïn.
$C^2\ H^4\ N^2\ O^2$ = urea.

$C^{10}\ H^5\ N^4\ O7$

1 equivalent of urea and if 2 equivalents of water be added to the remainder, we have

1 equivalent of hydrated allantoric acid = $C^{10}H^5N^4O^9$.

Water, at a high temperature, acts in a remarkable manner on allantoïn. It converts it into ammonia and into carbonic and allantoric acids. I expected to find in the liquor only urea and allantoric acid; but I very soon discovered the cause of the absence of urea in this re-action; for, without the intervention of bases or acids, in presence of water only, it is converted at a little above 100° C., into carbonic acid and ammonia.

I have operated these two decompositions in tubes sealed at both ends, which I exposed for a few minutes in an oil bath to a temperature of between 110° and 140° C.

From the preceding, allantoïn appears to be a peculiar kind of salt, in which urea should pre-exist quite formed, and from which it would be easily separated, under the condition of furnishing to the matter to which is found united the elements of a certain quantity of water. Liebig has remarked that allantoïn is, as regards its composition, urate of urea with the equivalent of water, common to this kind of salt:

Allantoïn. Urea. Uric Acid. Wat.
$C^{12}H^9N^6O^9 = C^2H^4N^2O^4, C^{10}H^4N^2O^6, HO.$

But these reactions do not allow of its being ranked among the urates. However, whatever may be the real constitution of allantoïn, the approximation of which I have spoken is far from uninteresting.

M. BIOT'S NOTE TO THE MEMOIR OF M. PELOUZE.

M. Biot remarked that the theory of the different compounds, designated under the name of *emetics*, might be advantageously cleared up by a series of optical experiments on those bodies; for, being all more or less complex tartrates, the tartaric acid in them must necessarily communicate to them rotatory powers, whose direction and intensity will depend on the nature of the elements which compose the combination, as well as on the special mode, according to which they are combined. But this investigation, to be useful, must be made comparatively, with all the soluble emetics. Indeed, an isolated trial would possess little interest, since the existence of the rotatory power in the tartrates has already long since been established by numerous experiments; and the modifications, as singular as various, which they have made known in the composition of the resulting rotatory power, must show beforehand that we should obtain most curious as well as most useful results from a general work thus undertaken.

ON THE SOURCE OF FRICTIONAL ELECTRICITY.

BY REUBEN PHILLIPS.

The source of the electricity developed by friction has long been discussed; and from the apparent impossibility of friction generating that agent, together with the electro-chemical theory, has caused many to ascribe the origin of all such electric phenomena to chemical action. I shall endeavour to show that friction may elicit electricity, and yet that the electricity so developed must be accounted for by the above theory; independent of chemical action.

Atoms are held together by a certain force; that force, by the electro-chemical theory, which I here consider admitted, is electricity. The power necessary to separate any two surfaces is in direct ratio to the surface, therefore a force infinitely small would be capable of detaching an infinitely small atom, although the surface to which it belongs be withheld by an infinitely great force.

Atoms are exceedingly small; so small that for mechanical consideration they may be regarded as infinitely divisible; from this, a very small mechanical force would be capable of removing them from their places. By experiment, we find that the least force constantly applied is capable of removing perceptible quantities of matter. The slightest friction wears out the machine, and travellers say that incessant kisses have wore away the toe of the bronze idol of St. Peter's. The application of these facts to the explanation of frictional electricity is easy. Atoms are united by virtue of their opposite electric energies; if they be forcibly separated, we shall have atoms possessing some of them + and some − electricity. Whether friction does or does not decompose the rubbing bodies into their ultimate elements is immaterial, as the aggregate is maintained by the same power which preserves the atomic combination.

As to the reason why the two electricities so produced do not immediately neutralize each other has nothing to do with the correctness of the above; it evidently presents an anomaly which no one has yet cleared up, and which applies with equal force to either of the above theories.

ON THE COMBINATIONS OF CHLORINE WITH BASES.*

BY M. GAY-LUSSAC.

(*Continued from page* 297.)

A given volume of hypochlorous gas containing an equal volume of chlorine and half

* *Comptes Rendus.*

a volume of oxygen, and these two bodies in the acid having exactly the same decoloring power, it results that the strength of a solution of hypochlorous acid must be attributed one half to the chlorine and the other to the oxygen. Thus, a solution having a strength of 1100°, it will have 550° of chlorine, and 550° of oxygen; and will contain $5\frac{1}{2}$ times its volume of hypochlorous acid, or $5\frac{1}{2}$ times its volume of chlorine, and $2\frac{3}{4}$ of oxygen. If we maintain a solution of hypochlorous acid in a sand-bath at the boiling temperature, it is decomposed the same as by light; chloric acid and a mixture of chlorine and oxygen, in which the chlorine predominates, are produced. On examination, in the course of an experiment, I found the volume of the chlorine five times greater than that of the oxygen. The decomposition of the hypochlorous acid is very rapid when its strength exceeds 900°, or 1000°; below that it is slower. From this it can be distilled without much loss of the strength of 700° to 800°, the operation not being conducted too slowly. An example will be useful to illustrate the process. A known volume of the solution, of the strength of 900° was taken, and the product of the distillation was received in ten nearly equal portions:—

The 1st portion had for strength		2500° *
2nd	,,	1925
3rd	,,	1470
4th	,,	943
5th	,,	624
6th	,,	400
7th	,,	222
8th	,,	106
9th	,,	30
10th remained in the retort		0
		8220

This number 8220°, divided by 10, gives a strength of only 822°, instead of 900°, which the solution employed had; but this difference is explained by a decomposition of one part of the acid during the distillation. But one cannot refrain from being surprised at one thing,—that is, to see a hypochlorous acid solution decomposed in part by distillation, and yet giving products much more concentrated than the solution itself, although under conditions apparently more favorable to decomposition. What is the cause of this difference?

It appears difficult to answer this question, without attributing to the sides of the vessels a very great influence on the decomposition of the hypochlorous acid. In admitting it, the effect is complex, and this is the way in which it may be conceived.

Firstly, The phenomenon takes place only with unstable compounds, already at the point of decomposition.

Secondly, Imagine a liquid mass in space, without contact of solid bodies, it would be more slowly decomposed, all other circumstances being equal, than if it really touched them. This is an assertion, which is but the expression of daily experience, and which it is necessary to admit.

Now, suppose the hypochlorous acid solution contained in a glass matrass, nearly at the boiling point, it will be decomposed at certain points on the surface of the glass with liberation of small bubbles of chlorine and oxygen without distillation, and the hypochlorous acid ultimately would be completely decomposed. The effect, from the cause which produces it, must necessarily be slow and regular.

If the solution is, on the contrary, carried quite to ebullition, the first effect of the heat will be always a decomposition of the hypochlorous acid at certain points in contact with the glass, with production of small bubbles of chlorine and oxygen gases; then another effect will commence; these small bubbles will gradually increase in volume by the vapors of water and acid which will combine with them, and the operation will be converted into a true distillation, which will continue in the same manner. Thus we can conceive, on the one side, why the decomposition of the hypochlorous acid is so slowly gradual, instead of being rapid as it would necessarily be, if it occurred at every point of the solution; and also, why the distillation decomposes one part of the acid, and strongly concentrates another.

The phenomenon of which I have just given the explanation is displayed under many other circumstances; but I shall only observe here, that it bears some analogy to what has been observed with respect to oxygenated water by my learned friend M. Thénard.

If we submit to distillation those which form still more concentrated solutions than the subject of these observations, of which the strength is for example from 1200° to 1500°, the decomposition of the acid will be considerable; on the contrary, with solutions from 600° to 700° the loss will be very little.

It sometimes happens that there are dissolved in the same solution both chlorine

* In distilling some more concentrated solutions of hypochlorous acid, the first portions which came over were of the strength of nearly 5000°.

and hypochlorous acid. They can be separated from one another with sufficient exactitude by keeping the solution for some time in the sand-bath, by which the chlorine will be disengaged. If the solution be very concentrated it will be necessary to reduce it with water to 600° or 700°.

The instability of hypochlorous acid, and the energy of its two elements, will sufficiently explain the powerful action that it exercises on other bodies. Sometimes it is determined by the affinity of the chlorine alone, sometimes by that of the oxygen, but most commonly by both. M. Balard having treated of this in a learned manner in his Memoir, I will not occupy myself with it here; but shall direct attention to the constitution of hypochlorous acid.

M. Balard has observed, and my experiments confirm it, that this acid, for one volume of chlorine contains half a volume of oxygen; and this is the way he establisbes its formula.

By calling R the metallic radical which is combined with one equivalent O, of oxygen, and Cl^2, two atoms or one equivalent of chlorine, we have the following formula:—

$(R + O + 2\ Cl)\ 2 = R\ Cl^2 + Cl^2\ O\ R\ O.$
$(R + O + 2\ Cl)\ 3 = 2\ R\ Cl^2 + Cl^2\ O^2\ R\ O.$
$(R + O + 2\ Cl)\ 4 = 3\ R\ Cl^2 + Cl^2\ O^3\ R\ O.$
$(R + O + 2\ Cl)\ 5 = 4\ R\ Cl^2 + Cl^2\ O^4\ R\ O.$

Of these different formula, says M. Balard, the third has been preferred by chemists, and chlorous acid (his hypochlorous acid) has been compared in its composition to nitrous acid and phosphorous acid. But why not admit the second, certainly the most simple, and in which the chlorous acid is found the equivalent of hyposulphurous acid? Making abstraction of all experimental proof, this supposition was much more natural than the other; for the circumstances in the midst of which chlorous acid is formed, do not in the least resemble those in which are formed the nitrous and phosphorous acids, &c., whilst they are identically the same as those in which the hyposulphurous is produced.

We know, indeed, that by treating the alkaline oxides with sulphur, with the aid of water, we obtain mixtures of 1 atom of hyposulphite and 1 atom of polysulphuret. If in this reaction we substitute chlorine for sulphur, we shall have 1 atom of chlorite and 2 atoms of chloride. The only difference that will exist in the two cases, is that the number which expresses the chemical equivalent of the chlorine being double that which represents its atom, wbile that of the sulphur has its two numbers equal, we shall have for the formula of chlorous acid $Cl^2\,O$, while that of the hyposulphurous acid will be S O.

What denomination ought we now to assign to this compound? It is evident tbat that of chloric acid can scarcely be maintained, and that it would be much more proper to call it *hypochlorous* acid,—a name which reminds us of the analogy of its constitution with the hyposulphurous and hypophosphorous acids, &c., formed like it of one equivalent of radicle, and one equivalent of oxygen. Its combination should be called *hypochlorites*.

It results then from M. Balard's own expressions, that hypochlorous acid is composed like hyposulphurous acid, that it is analogous to it in constitution, that it is produced under identical circumstances, and that it sbould be represented by a similar formula.

However, if we attentively examine this resemblance which M. Balard supposes, we shall not be long in discovering that it rests only on purely theoretical considerations which no direct experiment justifies.

I do not think that it can be correct to represent hyposulphurous acid by S O. Its real formula, its equivalent, is $S^2\,O^2$, which expresses the quantity of acid necessary to saturate one equivalent of base; and if the hypochlorous acid was analogous to it, it ought also to have for formula $Cl^2\,O^2$, Cl here representing one equivalent of chlorine.

Now, a hyposulphite,—that of potassa for example,—composed after the formula $S^2\,O^2\,P\,O$, is perfectly neutral, and a well conducted evaporation does not alter its degree of saturation. But if we take a known quantity of potassa, P O colored blue with turnsole, and pour in little by little some hypochlorous acid, the strength of which is also known, we observe that the blue color remains only until we have added nearly nine twentieths of $Cl^2\,O^2$.* This experiment, without being absolutely decisive, is, however, far from justifying the formula $Cl^2\,O^2$ for hypochlorous acid; and on the contrary, it is far more favorable to the formula Cl O; for it is easy to conceive that near the point of saturation, supposed to take place with Cl O the discoloration of the turnsole commences. Moreover, a solution of hypochlorite of potassa, $Cl^2\,O^2$ P O, placed in a vacuum at the ordinary temperature, beside two vessels, one con-

* It must be remarked, that if it be the carbonate of potassa, the first addition of hypochlorous acid destroys the color of the turnsole.

taining potassa to absorb the vapors of the chlorous acid, and the other sulphuric acid, loses half of its acid and acts not like a hyposulphite, but rather like a neutral salt to which has been added an excess of acid, and which by deficiency of affinity permits it to be disengaged by simple spontaneous evaporation.

We may remark, that if hypochlorous acid had really for its equivalent $Cl^2 O^2$, so long as that quantity of acid has not been employed for saturating one equivalent of potassa, P O, each portion added ought not to produce any other effect than to saturate an equal portion of the base; the hypochlorite would become only less and less basic, and we should not perceive any cause of disturbance, for the neutral or alkaline hypochlorite is extremely soluble. But, as soon as we have added to the potassa a little more hypochlorous acid than the half of $Cl^2 O^2$, or a little more than Cl O, the solution becomes turbid, soon loses strength, and at the same time the hypochlorite begins to be transformed into the neutral chlorate. This effect is especially remarkable when, after having put into the solution of potassa the half of $Cl^2 O^2$, chlorine is added instead of a fresh quantity of hypochlorous acid. The chemical equilibrium is very soon disturbed; the strength of the hypochlorite rapidly decreases; small bubbles of oxygen are disengaged, and chlorate of potassa is speedily formed.

The cause of the greater disturbance produced by the chlorine is here very evident. To add indeed hypochlorous acid to a neutral hypochlorite of potassa, is only in fact forming a more and more acid salt; but in adding the chlorine itself, the base even the salt, the potassa is decomposed; there is formed, as we shall prove further on, chloride of potassium and a fresh quantity of hypochlorous acid, which being added to that given up by the decomposed potassa, tends to render the remaining hypochlorite much more acid. The disturbance caused by the chlorine is therefore much greater than hypochlorous acid can produce, and this sufficiently explains the difference of action of these two bodies on the hypochlorite.

The action of chlorine on hypochlorite of potassa, Cl O K O, appears to me most satisfactorily to demonstrate that this salt is indeed really neutral; for if it were not so, or that this could be the salt $Cl^2 O^2$ K O, it is evident that an addition of chlorine to Cl O K O, of one tenth of an equivalent for example, would only augment the quantity of hypochlorite, and could not produce the great disturbance of which I have just spoken.

So that after the considerations just explained, and which will be strengthened by additional ones arising out of the whole of these labors, it is evident to me, that the combinations formed by hypochlorous acid and bases cannot be compared to the hyposulphites and the hypophosphites, and that their true formula, calling R the metallic radical, is Cl O R O, and not $Cl^2 O^2$ R O. The term *hypochlorous acid* is no longer correct, and I propose to substitute for it that of chlorous acid; and its saline compounds should be chlorites.

That this discussion may be involved in no obscurity I may remark, that the formula which M. Balard has adopted for his hypochlorous acid does not at all arise, as he thinks it, from the second of his four formulæ (given in this Memoir;) for the oxygenous acid which comes from that formula by employing three equivalents of base and three of chlorine, would necessarily be formed of one equivalent of chlorine and two of oxygen.

As regards the analogy in the circumstances, on which M. Balard supports the comparison of his hypochlorous acid, with the hyposulphurous, and hypophosphorous, acids, I do not see that we can draw any other conclusion than this: that it was very probable that the chlorine must form a chloracid in acting on an alkaline base; but as regards foreseeing its proper chemical constitution, there is too great a difference between chlorine, sulphur, and phosphorus, for the analogy to be a certain guide, and not mislead.

Before going further, it will not be uninteresting to indicate the means of measuring chlorous acid. This can be done by measuring its radical, chlorine; and as chlorous acid is formed of equal equivalents of chlorine and oxygen, possessing the same power of destroying color, the strength of the acid divided by two will give the strength of the chlorine.

To arrive at the knowledge of the amount of chlorine contained in a solution, we have taken as unity of strength that which it possesses at the temperature of 32° Fahr., and under the barometrical pressure of 30 in., and the volume of a litre. This strength is divided into 100 equal parts or degrees. Consequently 1° will represent 10 cubic centimetres of chlorine, and 100° will represent $1000^{c.c.}$, or one litre. Nothing more is required than to establish the relation of these degrees with those of the alkalimetre. According to the basis generally adopted since the time of Descroizilles, 5 grammes of concentrated sulphuric acid, dissolved in 50 cubic centimetres of water, are represented by 100°, and 6·1364 grammes equivalent of acid, by 122·728. Each degree will be

equal therefore in volume to half a cubic centimetre.*

Now the weight of a litre of chlorine at 0°, and at 30°, of barometrical pressure weighs 3·1689 gr., and the worth of the equivalent of chlorine 4·4265 gr., as that of the sulphuric acid 122°·728 of the alkalimetric degree, 3·1689 gr. of chlorine will be equal to 87°·8609.

Now the hundredth of 3·1680 gr. or one chlorometrical degree will be equivalent to 0·878609 of the alkalimeter: on this basis the following table has been calculated:—

TABLE SHOWING THE RELATION OF THE CHLOROMETRICAL TO THE ALKALIMETRICAL DEGREES.

Chlorometrical Degrees.	*Alkalimetrical Degrees.*	*Alkalimetrical Degrees.*	*Chlorometrical Degrees.*
In 1 litre.	In $50^{c.c.}$	In $50^{c.c.}$	In one litre of chlorine.
$1^{ch.}$	$0{\cdot}878609^{alk.}$	$1^{alk.}$	$1{\cdot}13816^{ch.}$
2	1·757218	2	2·27632
3	2·635827	3	3·41448
4	3·514436	4	4·55264
5	4·393045	5	5·69080
6	5·271654	6	6·82896
7	6·150263	7	7·96712
8	7·028872	8	9·10528
9	7·907481	9	10·24344
10	8·786090	10	11·38160

For the future we shall represent 1 chlorometrical degree by $1^{ch.}$; and 1 alkalimetrical degree by $1^{al.}$.

Suppose, for example, that we wish to form a neutral chlorite of potassa, Cl O, P O, with $200^{c.c.}$ of chlorous acid, of the strength of $1260^{ch.}$; the potassa being at the strength of $100^{al.}$; what volume will it be necessary to take?

In carrying the volume of the chlorous acid from $200^{c.c.}$ to $1000^{c.c.}$, its strength ought to vary in the inverse order; and we have $1000^{c.c.} : 200^{c.c.} :: 1260^{ch.} : X^{ch} = 1260^{ch.} \times \frac{200}{1000} = 252^{ch.}$.

These chlorometrical, converted into alkalimetrical degrees, give according to the table—

For $200^{ch.}$ . . .	$175{\cdot}7218^{al.}$
50 . . .	43·9304
2 . . .	1·7572
252	221·4094

Thus, to make the neutral chlorite required, it will be necessary to add to the chlorous acid, $221{\cdot}4094^{al.}$ of potassa. If, in place of the strength of $100^{al.}$, we have one of $112^{al.}$, the volume in half cubic centimetres, that must be taken, should be diminished in the proportion of 100 to 112, and would be consequently $\frac{100}{112}$, 221·409 = 197·6 half cubic centimetres.

It would be equally easy to calculate the volume of chlorous acid, of a given strength, necessary for saturating a given quantity of potassa.

* The alkalimetrical degrees are given by means of a graduated tube, divided into half cubic centimetres.

I have not attempted a special study of the chlorites; however, it would not be uninteresting.

Chlorous acid is a very feeble acid, more so perhaps than carbonic acid, although they mutually displace each other. Many oxides, either do not combine with it, or saturate it very imperfectly, and partially abandon it by the simple distillation of their solutions. The chlorites have very little stability; they are decomposed without heat if they are not kept from the light; at the temperature of boiling water, the decomposition is very quick; they are transformed into chlorates, and into chlorides, and at the same time a quantity of oxygen is disengaged, which is generally so much greater as they are more basic. The oxyacids, even carbonic acid, disengage from them chlorous acid, and it can be isolated by distillation.

If the chlorite is mixed with a metallic chloride in sufficient quantity, and if an excess of sulphuric acid be added, the chlorine is immediately disengaged with effervescence. The metal of the chloride takes the oxygen from the chlorous acid, and is dissolved in the sulphuric acid; and the chlorine, of both the chloride and the acid, is disengaged and set at liberty.

But if the sulphuric acid is cautiously and accurately added, and exactly, or no more than enough, to decompose the chlorite, chlorine will not then be disengaged, but, indeed, chlorous acid. This experiment is decisive; we shall return to it in speaking of the chlorides of oxides, with which we are now about to treat.

(To be concluded in our next.)

ACTION OF WATER ON THE COMBINATIONS OF SULPHUR WITH THE METALS OF THE ALKALINE EARTHS.*

BY H. ROSE.

The important Memoir of Berzelius on the sulphurets of the metals of the alkalis, and that of M. Berthier on the combinations of sulphur produced by means of the reduction of the sulphates by charcoal, have so completely enlightened us concerning the nature and composition of these bodies, that the subject may appear entirely exhausted. However, the sulphurets of the metals of the alkaline earths present, with water, phenomena which seem hitherto to have escaped the attention of chemists.

SULPHURET OF BARIUM.

I have made most of my experiments with this metallic sulphuret, because, on account of the complete separation of the baryta in the state of sulphate, the investigations undertaken with this sulphuret give the most striking results, and because the sulphuret of barium, treated with water, forms more numerous products than the sulphurets of the metals of other alkaline earths.

The sulphuret of barium, obtained by heating the sulphate with charcoal at a white heat, was put in contact with cold water for twenty-four hours, and occasionally well stirred during that time. The quantity of water employed was not nearly sufficient for dissolving all the sulphuret of barium. At the end of twenty-four hours the liquor was separated by decantation from the undissolved portion. An equal quantity of cold water was added to the latter, and I proceeded with it in the same way. After this operation had been repeated nine times, the water had dissolved nearly all that it could, and there remained undissolved only the excess of charcoal. In this manner I obtained nine liquors, each of which was immediately examined.

The first liquor was of a faint yellow color; it formed, on the addition of hydrochloric acid, a white cloud of sulphur; mixed with neutral saturated solution of protosulphate of manganese, it immediately evolved sulphuretted hydrogen with effervescence. In order to oxidise it completely, the sulphuretted hydrogen was disengaged by hydrochloric acid, and driven into a mixture of fuming nitric and hydrochloric acids, which converted it into sulphuric acid, without depositing sulphur. By means of a continued current of atmospheric air there was passed into the acid mixture the greatest possible quantity of sulphuretted hydrogen in solution in the liquor, and then chlorine was driven into the latter, until all the sulphuretted hydrogen which it still contained was converted into sulphuric acid. This long process was necessary for rendering the oxidation complete; for it was not possible, by driving chlorine into the solution of the sulphuret of barium, to oxidise all the sulphur by means of that gas; the sulphur was then enveloped in too large a quantity of sulphate of baryta. The oxidised liquors were mixed: the addition of a solution of chloride of barium to the mixture separated from the deposit of sulphate of baryta, instantly gave a very abundant precipitate.

The second liquor, treated like the first, acted in the same manner.

The third, in its mixture with the solution of protosulphate of manganese, presented only a very faint odor of sulphuretted hydrogen; however, it gave rise to a powerful disengagement of that gas by the addition of hydrochloric acid. After oxidation, chloride of barium produced in the solution separated from the sulphate of baryta only a very slight precipitate.

The fourth gave by hydrochloric acid an abundant disengagement of sulphuretted hydrogen; but the solution of protoxide of manganese did not develope the odor of that gas. Chloride of barium did not produce any precipitate in the oxidised liquor after the separation of the sulphate of baryta; but the latter was precipitated by sulphuric acid.

The fifth liquor presented no odor of sulphuretted hydrogen with the solution of protosulphate of manganese; but this addition caused a flesh-red colored precipitate of sulphuret of manganese, although the acids produced only a very slight liberation of sulphuretted hydrogen. Sulphuric acid formed a very abundant precipitate in the oxidised liquor, after the separation of sulphate of baryta.

The sixth liquor presented scarcely any odor of sulphuretted hydrogen when supersaturated with the acids; but sulphuric acid produced in it a very abundant precipitate of sulphate of baryta. The same acid also formed a very copious precipitate in the oxidised liquor, after the sulphate of baryta had been separated.

The seventh liquor exhibited no odor of sulphuretted hydrogen in its supersaturation by the acids; however, a neutral solution of protoxide of iron produced in it a blackish color, although the solution of protosulphate of manganese had not given rise to any perceptible flesh-red precipitate, but had formed only a white precipitate, which

* *Journal für Praktische Chemie.*

became brown in the air. Oxidation produced in this liquor only a very trifling quantity of sulphate of baryta, but sulphuric acid formed in it a very abundant deposit after its filtration.

The eighth liquor gave no blackish coloration with the solution of protoxide of iron; it furnished with that of protoxide of manganese only a white precipitate which turned brown in the air. Oxidation did not produce in it any sulphate of baryta; but sulphuric acid determined an abundant precipitation of it.

The ninth liquor acted in the same manner.

From these experiments, it results that the sulphuret of barium is not dissolved without decomposition in water. When it is gradually treated with that liquid, the latter at first dissolves a combination of sulphuret of barium with sulphuretted hydrogen, then very pure sulphuret of barium, afterwards sulphuret of barium with baryta, and, finally, pure baryta.

The first two obtained liquids contained, as is shown by their action with the reagents, hydrosulphuret of barium; the third, sulphuret of barium with a very small quantity of hydrosulphuret; the fourth, sulphuret of barium with a little baryta; the fifth, a little sulphuret of barium with much baryta; and the remaining liquids only baryta with traces of sulphuret of barium, which I have been able to discover even in the sixth and seventh.

If great quantities of sulphuret of barium be boiled with water, the same products are obtained. The crystals which the cooled liquors deposit are partly hydrate of baryta, partly, in certain circumstances, sulphuret of barium, and partly chemical compounds of hydrate of baryta with sulphuret of barium. The hydrosulphuret of barium remains in solution, because it is the most soluble of all the substances formed in the treatment of sulphuret of barium by water. I will make a few observations on all these products.

HYDRATE OF BARYTA.

If the crystals deposited, as above-mentioned, from the cooled liquors are redissolved once or twice in boiling water, those which are again formed by cooling are pure hydrate of baryta. They may be obtained very free from sulphuret of barium, so that, when supersaturated by the acids, they do not give out the least odor of sulphuretted hydrogen. When promptly and completely expressed between sheets of blotting paper, they contain the quantity of water attributed to the hydrate obtained in another manner; namely, 10 atoms of water to 1 atom of baryta.

Liebig has already remarked that the solution of sulphuret of barium in boiling water may give rise to crystals of hydrate of baryta. However, he attributes the production of baryta accompanying that of sulphuret of barium to the mixture of sulphate of baryta and charcoal not having, in this case, been heated to whiteness, but only to redness, as then only half of the baryta would have been reduced, and which would produce bisulphuret of barium. I will show further on that this last compound may be formed in the solution in contact with the air. It is not, however, an immediate product formed in the treatment of sulphuret of barium by water.

HYDRATE OF BARYTA WITH SULPHURET OF BARIUM.

If the sulphuret of barium resulting from the calcination of sulphate of baryta and charcoal be boiled with a moderate quantity of water, there are deposited from the filtered and cooled liquor, when it is preserved for a long time from the contact of the air, and after the hydrate of baryta, crystals formed of hydrate of baryta and sulphuret of barium. It is sometimes difficult to decide whether a mixture of sulphuret of barium with hydrate of baryta, or a chemical combination of these two bodies, is obtained. This takes place especially when the solution of sulphuret of barium has not been kept very long; for the crystals then form, when they are removed from the liquor and completely freed from the supernatant liquors by rapid expressing between folds of blotting paper, only a coarse, white crystalline powder, and in which it is impossible to distinguish whether it is a homogeneous substance or a mixture.

I have kept, however, for several years, in a cold place, out of contact with the air, a liquor which I had obtained by boiling sulphuret of barium with a moderate quantity of water. The crystals which first separated were in scales; but some months later, very large crystals from 0.009 to 0.014 of a metre in length were formed on these. Many of these crystals, all of which had, as well as could be ascertained, the same crystalline form, were examined. They were white; but afterwards, they became yellowish. They had a tabular aspect, and seemed to be hexagonal dodecahedrons with very obtuse terminal angles. Analysis showed that they were composed of 1 atom of baryta, 3 atoms of monosulphuret of barium, and 28 atoms of water. If we admit in the hydrate of baryta 10 atoms of water, the sulphuret of barium combines with 6 atoms of water, and the chemical compound might be expressed by the formula $BaO\,HO^{10} + 3\,BaO\,HO^{6}$.

Indeed, the sulphuret of barium obtained in the isolated state, ordinarily combines with 6 atoms of water.

The crystals in scales formed in the first place, of which it cannot be said with correctness that they are not mixtures, have also been examined. I found in them the composition approximatively represented by the formula 4 BaO HO^{10} + 3 BaO HO^{6}.

I have also made a third analysis of a compound of sulphuret of barium with hydrate of baryta, which was presented, indeed, after being expressed between sheets of blotting paper, only in the form of a white crystalline powder, but which, however, had a remarkable composition. This compound was obtained by first boiling for several hours in a retort a solution of sulphuret of barium, carefully replacing by fresh boiling water the water distilled over, and with which the sulphuretted hydrogen was disengaged, and afterwards concentrating the liquor by evaporation. The compound was shown by analysis to be composed accordin gto the formula BaO HO^{10} + BaO HO^{10}. The sulphuret is not in this compound combined, as in the others, with 6 atoms of water, but with as many atoms of water as the baryta in the hydrate of baryta.

I have already said that, of the examined compounds of hydrate of baryta with sulphuret of barium, it is only the first of which I can say with much accuracy that it is not a mixture. But it is the composition of that mentioned in the last place which, on account of its simplicity, also renders probable the existence of this compound. Now, if this hypothesis is admitted, hydrate of baryta may combine in several proportions with the sulphuret of barium.

If the compounds of hydrate of baryta with sulphuret of barium be dissolved in hot water, the cooled solution gives crystals of pure hydrate of baryta.

SULPHURET OF BARIUM.

I obtained sulphuret of barium, but never completely free from hydrate, by treating with boiling water the sulphuret of barium produced by the calcination of sulphate of baryta with charcoal, separating from the solution, as far as possible, by concentration in a retort, all the hydrate of baryta and its chemical compounds with the sulphuret of barium. The liquor freed from the crystals contained hydrosulphuret and sulphuret of barium: the latter was obtained by again concentrating the liquor in a retort. The sulphuret of barium was separated by cooling, and the hydrosulphuret remained in solution. During the evaporation, sulphuretted hydrogen gas was evolved with the steam.

The sulphuret of barium obtained was, after being completely dried between folds of blotting paper, in the form of a perfectly white crystalline powder. It became yellowish, not only by exposure to the air, but also spontaneously, when preserved from its contact.

The solution of sulphuret of barium in water remains colorless, when a solution of neutral protosulphate of manganese is added to it. If, however, sulphuret of barium be completely oxidised, the liquor separated from the sulphate of baryta precipitates by sulphuric acid,—a proof that the sulphuret of barium obtained is never free from hydrate of baryta. Sometimes, however, the precipitate has been so slight that it might be concluded that the hydrate of baryta was found only mixed in the state of combination of sulphuret of barium with hydrate of baryta, and not chemically combined with the sulphuret of barium.

I have analysed sulphuret of barium prepared at different periods; I have obtained 7·02, 4·46 and 0·92 of baryta, combined with the sulphuret, in the state of hydrate. Sulphuret of barium contains, according to the first and second analyses, 6 atoms of water, and according to the third rather more than 6 atoms.

Sulphuret of barium does not disengage sulphuretted hydrogen, as I have already observed, when it is mixed with a solution of a protosalt of manganese. However, this is the case only when a certain quantity is treated by sufficient water to dissolve it. But when a much larger quantity of sulphuret of barium, and even that which contains hydrate of baryta, is gradually treated with much less water than is required for its complete solution, it acts just the same as sulphuret of barium immediately obtained from the sulphate by means of charcoal. The first portions of the solution disengage much sulphuretted hydrogen with the solutions of the protosalts of manganese, and contain hydrosulphuret of barium; the latter portions contain sulphuret of barium with hydrate of baryta, and in the last place hydrate of baryta only.

SULPHURET OF BARIUM WITH SULPHURETTED HYDROGEN.

The liquors from which the sulphuret of barium is separated by crystallisation, develope a strong odor of sulphuretted hydrogen when mixed with a neutral solution of protoxide of manganese. However dilute the liquor may be, sulphuretted hydrogen is disengaged in the gaseous form with brisk effervescence. These solutions contain, therefore, hydrosulphuret of sulphuret of barium.

The liquors have, more or less, a yellowish color; but the yellow color does not properly belong to them. It arises from a higher degree of sulphuration of the barium, which is very easily produced by the contact of the smallest quantity of atmospheric air with the solution of the hydrosulphuret. All chemists well know how difficult it is to obtain colorless hydrosulphuret of sulphuret of ammonium. When the hydrogen of the hydrosulphuret is oxidised, forming water, the sulphur separated combines to produce a more elevated degree of sulphuration of the metal.

If hydrosulphuret of barium be concentrated by evaporation in a retort, sulphuretted hydrogen is disengaged with the steam. When the concentration is sufficient, the liquor becomes on cooling a crystalline mass, which, treated by the solution of protoxide of manganese, gives rise to an extremely powerful disengagement of sulphuretted hydrogen.

I have not made the quantitative analysis of this solid hydrosulphuret of barium, because it is difficult to obtain it free from a portion of a higher degree of sulphuration, sulphuret of barium, and even a little hydrate of baryta. It is not soluble in alcohol; that liquid is not capable of separating the different substances.

The higher degrees of sulphuration of barium do not combine with sulphuretted hydrogen, and the hydrosulphuret loses sulphuretted hydrogen in proportion as it absorbs sulphur. If the solution of hydrosulphuret of barium be boiled with powdered sulphur, sulphuretted hydrogen is disengaged with a brisk effervescence. It has then completely lost the property of disengaging sulphuretted hydrogen on the addition of the neutral solution of protoxide of manganese.

The sulphuretted hydrogen which is disengaged from the solution of the hydrosulphuret by boiling with powdered sulphur, has a singularly disagreeable odor, especially that which is disengaged towards the last. It probably contains a higher degree of sulphuration of hydrogen.

If iodine be gradually added to the solution of hydrosulphuret of barium, sulphuretted hydrogen is disengaged and sulphur deposited, by the addition of a small quantity of iodine; but in proportion as more iodine is added, it decomposes the sulphuretted hydrogen, and the solution becomes very acid, owing to the presence of free hydriodic acid. If, on the other hand, water and then iodine be added to the mixture of sulphuret of barium and charcoal obtained by the decomposition of the sulphate of baryta, there results a deposit of sulphur and a neutral solution of iodide of barium.

It is a long time since the observation that a solution of sulphuret of barium may, in certain circumstances, give with iodine a very acid solution, was communicated to me by Wittstock, who prepares considerable quantities of iodide of barium, in order to obtain, by means of its decomposition with sulphate of potassa, a very pure iodide of potassium. It was this observation that suggested to me this work.

Sulphuret of barium is decomposed, therefore, as results from the above-mentioned facts, in the treatment by water, by seizing its elements, into sulphuretted hydrogen and baryta. However, the tendency of sulphuretted hydrogen to form a sulpho-salt with the sulphuret of barium, causes the baryta to separate in the state of hydrate, while the sulpho-salt remains in solution, because the difference in the solubility of these two bodies is great. The hydrate of baryta separates partly in the pure state, and combines partly with the sulphuret of barium to form peculiar compounds, which are more soluble than the pure hydrate of baryta; but in which, however, the constituent parts are combined with so weak an affinity, that the less soluble hydrate of baryta separates in the pure state by aid of several crystallisations, while the sulphuret of barium is again decomposed by water in the manner indicated. A single boiling with water seems ordinarily to decompose the sulphuret of barium into the hydrosulphuret, and into compounds of sulphuret of barium with hydrate of baryta; these last, again, treated with water, deposit crystals of hydrate of baryta, while the sulphuret of barium is decomposed.

It might be considered surprising that, among the products of the decomposition of the sulphuret of barium by water, we should obtain very pure sulphuret of barium in the hydrated state; but in proportion as, on one part, the hydrate of baryta combines with the sulphuret of barium, the hydrosulphuret of barium combines with the sulphuret. However, when the solution of this combination is evaporated, sulphuret of barium becomes free, partly because sulphuretted hydrogen is disengaged with the steam, and partly because, by the concentration and cooling of the solution, the sulphuret of barium is separated from the hydrosulphuret in the crystalline state, and by the presence of too small a quantity of water, and that of the solution of the hydrosulphuret, which has escaped decomposition, which in the absence of this latter may make it bear a larger quantity of water.

To this opinion it might be objected, that pure sulphuret of barium may also be extracted by aid of cold water from the mass

obtained by the decomposition of sulphate of baryta with charcoal; for the experiments undertaken with this mass, and which were mentioned at the commencement of this Memoir, show, that after its gradual exhaustion by cold water, the third and fourth liquors contained sulphuret of barium; in one of them it was combined with only a small quantity of hydrosulphuret of barium, and in the other, with a little hydrate of baryta.

But it may be replied, that these might just as well be in the two solutions of hydrosulphuret of barium and of hydrate of baryta, in such proportions that only sulphate of baryta could be produced by their oxidation, mixed, in one case, with a small excess of sulphuric acid, and in the other, with a slight excess of baryta. A sufficient concentration determines, then, circumstances in which hydrosulphuret of barium and hydrate of baryta may combine to form crystallised sulphuret of barium.

SULPHURET OF STRONTIUM.

Water decomposes sulphuret of strontium in a still more surprising manner than the sulphuret of barium. I employed for my experiments sulphuret of strontium obtained by treating sulphate of strontia by an excess of charcoal at a white heat. The mass turned black by charcoal was boiled with water, and deposited on cooling a considerable quantity of hydrate of strontia, which, after having been freed as much as possible from the supernatant liquor by pressing between folds of blotting paper, appeared perfectly white, and developed by solution in acids only a very slight odor of sulphuretted hydrogen.

The liquor separated from the crystals disengaged, by the addition of a solution of protosulphate of manganese, sulphuretted hydrogen gas, with effervescence.

By continuing to boil the calcined mass, the filtered liquors at last presented scarcely any odor of sulphuretted hydrogen on the addition of the acids, and they contained in solution almost pure strontia.

By evaporating in a retort the liquors separated from the hydrate of strontia, I obtained with the steam a stronger disengagement of sulphuretted hydrogen than is furnished by the solutions of hydrosulphuret of barium. But there was again deposited, by cooling, from the concentrated liquors, only pure hydrate of strontia, and the hydrosulphuret of strontia remained in solution; it was necessary to continue the concentration until it was much reduced in bulk, to have the crystals of hydrate obtained mixed with a small quantity of sulphuret of strontium, or rather with the hydrosulphuret.

I have not succeeded in extracting from the solutions either sulphuret of strontia, or any compound of that body with hydrate of strontia.

I have examined hydrate of strontia obtained from very concentrated solutions by evaporation, and I found it identical in different preparations. It contained 10 atoms of water to 1 atom of strontia.

By concentrating still further by evaporation, the supernatant liquors,—an operation during which the disengagement of sulphuretted hydrogen was so much the more abundant as the volume of the solution became less,—they acquired a deeper yellow color, by the production of a higher degree of sulphuration; and, finally, crystals of hydrosulphuret of strontium were formed in the very concentrated liquor.

According to these investigations, sulphuret of strontium is completely decomposed, by being treated with water, into hydrosulphuret of strontium and hydrate of strontia.

SULPHURET OF CALCIUM.

The sulphuret of calcium was obtained by heating sulphate of lime with an excess of charcoal, at a white heat.

By treating the mass with cold or boiling water, I obtained liquors which disengaged a strong odor of sulphuretted hydrogen by the addition of a solution of protosulphate of manganese. When the mass was afterwards boiled with water, until no more hydrosulphuret could be detected in the solutions, which required very considerable quantities, the water dissolved scarcely anything but lime. The residue consisted chiefly of hydrate of lime.

No crystalline deposit was formed in any of the liquors by cooling, because hydrate of lime is less soluble in warm than in cold water.

The sparing solubility of hydrate of lime in water causes it to decompose the sulphuret of calcium into the hydrosulphuret, which is dissolved, and into hydrate of lime, which remains for the most part undissolved. However, the latter always contains sulphuret of calcium.

If the solutions of the hydrosulphuret be concentrated by evaporation in a retort, there is disengaged with the steam a very large quantity of sulphuretted hydrogen, much more than with the solutions of the hydrosulphurets of barium and strontium, in similar circumstances. This disengagement becomes more abundant as the volume of the liquor diminishes.

The concentrated and cooled liquors deposited small crystals of sulphate of lime, which were contained in the sulphuret of

calcium employed, and had escaped decomposition by the charcoal, and also hydrate of lime containing a little sulphuret of calcium.

By a greater concentration, the liquors became more yellow, and often precipitated a white powder, which was sulphate of lime, and which was produced during the ebullition, by the hyposulphite of lime formed in the liquor.

There were formed, finally, by cooling, in the very concentrated liquors, long golden-colored crystals, which were very small in quantity, however, although their bulk appeared considerable so long as they were not separated from the liquor which had given rise to them.

These same crystals were not presented at a more advanced period of the evaporation, and the disengagement of sulphuretted hydrogen ultimately became so powerful that the liquor formed a considerable scum during the concentration in the retort. If the evaporation is carried very far, so that the liquor might become a crystalline mass on cooling, the latter is essentially formed of the same golden yellow crystals, which are deposited as soon as the liquor begins to cool. They contain only a small quantity of mother liquors, which hold in solution traces of hydrosulphuret.

The hydrosulphuret of calcium cannot exist in the solid form, as Berzelius has already demonstrated.

The crystals do not develope the odor of sulphuretted hydrogen on being treated by the neutral solution of protoxide of manganese; but they do so by the addition of an acid. If they be dissolved in hydrochloric acid, the separation of sulphur renders the liquor very milky; a solution of chloride of barium forms a precipitate in the filtered liquor. If dilute sulphuric acid be poured on these crystals, they present only the odor of sulphuretted hydrogen, and not that of sulphurous acid. Treated, by small portions at a time, with a large quantity of water, they become white, and leave a white residue, which is lime. When heated, they give water and sulphur; they leave a white residue, which, when treated with hydrochloric acid, disengages sulphuretted hydrogen, while the liquor becomes milky, owing to the separation of sulphur; the chloride of barium produces an abundant precipitate in the filtered liquor.

From these investigations it results that these crystals contain neither sulphate, sulphite, nor hyposulphite of lime, nor hydrosulphuret of calcium, but, indeed, a calcium in a higher degree of sulphuration, combined with hydrate of lime.

I have made several analyses of this remarkable salt; I have certainly been able to use only very small quantities for the purpose; but they have given me more uniform results than might have been expected. They have shown, at least, that the crystals which are deposited, by cooling, from very concentrated solutions, have essentially the same composition.

After I had removed the crystals from the liquor, they were dried by pressing between folds of blotting paper.

The results obtained correspond to the composition of a compound, formed of 1 atom of quintisulphuret of calcium, of 5 atoms of lime, and 20 atoms of water; $CaS^5 + 5CaO + 20HO$. They are formed by the disengagement of sulphuretted hydrogen during the boiling of the solutions, and by the transformation of the hydrosulphuret of calcium iuto the sulphuret. Besides, the hyposulphite of lime, gradually formed in the numerous solutions, gave rise by boiling to the sulphite of lime, which was deposited before the production of the salt examined, while the sulphur combined with the sulphuret of calcium, forming a higher degree of sulphuration of the calcium, which produced with the lime in solution the crystals analysed.

MEANS OF DISTINGUISHING ZINC FROM MANGANESE IN SOLUTIONS WHICH CONTAIN AMMONIACAL SALTS.*

BY PROFESSOR OTTO.

If solutions of chloride of zinc and chloride of manganese containing much hydrochlorate of ammonia be rendered alkaline by means of liquid ammonia, there is formed in the solution of zinc, by the addition of small quantities of an aqueous solution of sulphuretted hydrogen, a white hydrated precipitate of sulphuret of zinc, whilst the addition of a small quantity of the same liquor does not form any precipitate in the solution of manganese; much more must be added for the sulphuret of that metal to be precipitated. If concentrated acetic acid is then poured into the liquors, the precipitate of sulphuret of mangauese dissolves very readily, whilst that of the sulphuret of zinc does not dissolve. Professor Otto recommends taking an aqueous solution of sulphuretted hydrogen, and not hydrosulphate of ammonia; because the latter, containing, as it always does, a portion of persulphuret, may occasion errors, since the addition of acetic acid naturally separates sulphur from it. If, for example, it is desired to ascertain whether iron filings contain brass, the filings must be

* *Annalen der Chemie und Pharmacie.*

dissolved in nitro-muriatic acid; the oxide of iron is precipitated by an excess of ammonia, the liquor is then acidulated, the copper is precipitated by sulphuretted hydrogen, and ammonia is added to the filtered liquor, which ordinarily contains a sufficient quantity of sulphuretted hydrogen. If a white precipitate, insoluble in acetic acid, is formed, the presence of zinc is proved.

ON A NEW COMPOUND OF CHLORINE AND OXYGEN.*

BY M. MILLON.

In a Memoir which I had the honor of reading to the Academy, I have demonstrated that the combination of oxygen and chlorine which we have generally called deutoxide of chlorine, and whose formula is $Cl\ O^4$, is a complex acid, incapable of forming salts, and which, by the contact of alkaline bases, is converted into chlorate and chlorite. I had observed this separation, in the action of $Cl\ O^4$ on the potassa, and confirmed its nature by the analysis of the compound of silver $Cl\ O^3\ Ag\ O$, which was obtained by the double decomposition of the chlorite of potassa and the nitrate of silver.

These first observations rendered very probable the existence of a a new compound of chlorine and oxygen of which the formula would be $Cl\ O^3$. I have been so fortunate as to isolate, indeed, this latter compound, and have discovered several simple processes which enabled me to prepare it abundantly; and, as very frequently happens in chemical researches, the new compound being once well determined, I have found that it is formed under circumstances exceedingly numerous, and varied.

I shall simply speak of the frequency of this production, and at the same time I will give a clear and general idea of this compound, which ought according to my idea to be called, *chlorous acid*, it being formed every time that we deoxidise chloric acid. Chlorous acid is the most stable oxygenous compound of chlorine, in the presence of a deoxidising agent, provided it be maintained within the limits of temperature, beyond which this acid or its compounds are destroyed. It is thus that chlorous acid resists the reducing action of nearly all substances both organic and metallic, and that it is a product of the calcination of perchlorate of potassa, which gives chlorite, before it gives chloride.

I shall explain besides, in detail, all the reactions in which chlorous acid is formed.

I shall confine myself in this short Note, to describing two processes of preparation, each of which has its advantages according to the case; I will afterwards extract some principal facts from the study that I have made of chlorous acid, and the chlorites.

Chlorous acid is obtained by introducing into a flask of the capacity of 300 or 400 cubic centimeters, which is filled nearly to the neck, with a mixture of tartaric acid, chlorate of potassa, nitric acid of commerce, sp. gr. of 1·327, and water in the following proportions :—

Tartaric acid	1
Chlorate of potassa . .	4
Nitric acid	6
Water	8

First is introduced the tartaric acid and the chlorate of potassa, roughly mixed without pulverising, and afterwards is added the nitric acid and water previously mixed. The rest of the apparatus is then adapted, and the gas dried over chloride of calcium may pass into dry receivers; or otherwise it may be conducted into a Woulfe's apparatus to be dissolved in water.

The reaction will commence of itself, if we wait a few minutes (at + 25° C.), but we can without fear commence by putting a single piece of lighted charcoal under the flask, and afterwards it may be heated so as not to exceed the temperature of + 45° to + 50° C.

The operation is terminated when the mixture is decolored. In this reaction the chlorous acid is mixed with carbonic acid.

The chlorous acid is a gas of a very deep greenish yellow: its smell powerfully irritates the throat and lungs; it is confounded with hypochloric acid. It decolors the tincture and paper of turnsole, and the sulphate of indigo. It is converted by cold into a red liquid, of a less deep tint than that of hypochloric acid; but requires a considerable lowering of temperature. It is decomposed at + 57° Cent. with production of a slight shock.

Its solution has a caustic taste. It is green when the gas is in small quantity: it is of a very deep golden yellow when the water has absorbed five or six times its volume of the gas, which appears to be the limit of its solubility; at + 20° Cent. the solution stains, and in a few instants the skin is yellow.

A single bubble of gas suffices to color a litre of water. It has a tinctorial power, which can be compared only to that of the soluble chromates.

It is impossible to analyse this gas with the bulbous apparatus which M. Gay-Lussac has so successfully applied to the analysis of the hypochloric acid $Cl\ O^4$. In this apparatus the chlorous acid is transformed

* *Comptes Rendus*, No. 12, September 19, 1842.

into chlorine, oxygen, and perchloric acid, which afterwards resists the heat of a glass tube, heated to redness for 40 centimeters of its length.

But its elementary analysis is made without trouble, by the aid of a small glass tube filled with metallic copper. The gas, well dried, is directed on the metal, which must be heated to an extent of 7 or 8 centimeters. If it is heated only at one point, the perchloric acid will in part escape the decomposition.

The mean of three analyses gave me 60·15 per cent. of chlorine, which leads to the formula $Cl\,O^3$; calculation gives 59·65 per cent. of chlorine.

This formula is confirmed, first by the analysis of the chlorites, which have for a general formula $Cl\,O^3$, M* O; secondly, by the density of the gas, experiment gave 2·646, and calculation 2·733: which indicates a condensation of two volumes of chlorine and three volumes of oxygen, into three volumes of gaseous acid.

Independently of the transformations of the gaseous, and dissolved chlorous acid, in which we find this compound of an extreme sensibility to the light, we observe besides from the contact of humid air, the following phenomenon:—

If we take a flask of the capacity of 8 or 10 litres, into which we pour a little water, and agitate it in such a manner as to saturate the air in its interior with humidity; if afterwards we introduce some drops of an aqueous solution of chlorous acid, holding at the most its volume of gas in solution, —we see, almost immediately, commence from the bottom of the flask, very dense white vapors, which rise insensibly, filling all the capacity of the vessel and finishing by overflowing. We thus obtain by the aid of some bubbles of a foreign gas disseminated in a relatively immense reservoir, the image of a thick fog, notwithstanding that the gas at the moment of its introduction is directly saturated with humidity.

This phenomenon lasts about half an hour: it takes place in an atmosphere of hydrogen, carbonic acid, or oxygen, as well as in common air.

Chlorous acid in the state of gas, is characterised as respects some metals by a remarkable inactivity. Copper, lead, tin, antimony, zinc and even iron, reduced in very fine filings, may remain an hour or more in its atmosphere, without the least sign of alteration. Mercury forms an exception, it absorbs the gas at the ordinary temperature, without leaving a residue.

Chlorous acid in solution in water gives different results, and very various amongst themselves. Thus mercury gives oxychlorides; copper, a mixture of chlorate, and chloride; zinc, and lead, give chlorides, and chlorites; antimony is not in the least attacked, however prolonged may be the contact. In this respect it is similar to gold and platinum and to many metals, which it constantly surpasses, in its general affinity.

The oxides present also numerous peculiarities, and without speaking of the oxides belonging to inferior sections, the alkaline and earthy oxides do not combine without considerable difficulty. The hydrate of lime is without action on the gas; and potassa itself in solution, mixed with gas likewise dissolved, remains more than twenty minutes without forming a chlorite: it is useless to shake the mixture of the two solutions.

Potassa, soda, and baryta form acid chlorites strongly colored with red, but which it is impossible to obtain in the crystalline state. Some, neutral chlorites which exist in solution, are decomposed by the concentration of their liquor. Others again, such as those of manganate of iron, and of mercury, do not appear to exist under ordinary circumstances; but the chlorites of lead, of silver, of baryta, and of strontia, give crystalline salts which are easily analysed.

All these chlorites, independently of the general properties that we foresee, and which consist in their decomposition and combustion, present a sensible character; when we treat them with dilute nitric acid, they appear to disengage a strongly colored yellow gas with a powerful odor, which is no other than chlorous acid itself.

This gas is distinguished from chlorine, in this, that its power of taking away colors is not destroyed by a solution of arsenious acid in hydrochloric acid; it continues to act on the sulphate of indigo whatever may be the addition of arsenious acid. This gas is distinguished again from hypochloric acid, in this, that it does not furnish a chlorate with potassa, and can be expelled from its solution in water by a current of carbonic acid without leaving a trace of chloric acid.

* M is here used to represent a metal.

NEW INSTANCE OF THE FORMATION OF AMMONIA.*

BY M. REIZET.

From Kuhlmann's investigations we learn that, under the influence of spongy platinum, the deutoxide of nitrogen, in contact with an excess of hydrogen, produces ammonia. In

* Abridged from *Comptes Rendus*, No. 4, July 25, 1842.

repeating Kuhlmann's experiments, M. Reiset was induced to substitute various metallic oxides for the spongy platinum. The results which he obtained are very interesting, and are likely to throw great light on the very obscure cause of the catalytic phenomena. M. Reizet says, that with an apparatus composed of two flasks of one litre each, to disengage the hydrogen and deutoxide of nitrogen, and ten grammes of peroxide of iron, heated at the end of a test-tube, he procured sufficient ammonia to saturate completely, in less than an hour, 25 grammes of fuming commercial hydrochloric acid.

ON SOME PRODUCTS OF THE RECIPROCAL ACTION OF ETHAL AND SULPHURET OF CARBON.*

BY MM. DE LA PROVOSTAYE AND DESAINS.

The ethal obtained by M. Chevreul in the saponification of spermaceti, some years since, was made by MM. Dumas and Péligot, the subject of an attentive study. These two chemists fixed by clear and accurate experiments, the nature of this compound, which they definitively ranked in the great class of alcohols. We are, however, far from having a complete series for ethal, and hence it is not uninteresting to add to the combinations which they have studied a new combination, which goes to render still more evident the perfect similitude between these two groups of bodies.

The excellent work of M. Zeize on the xanthates is well known. The authors thought that it would not, perhaps, be impossible to obtain the corresponding combination in the series of cetene. After several trials, it was accomplished in the following manner:—

They dissolved in the sulphuret of carbon as much ethal as to completely saturate it; to the perfectly transparent solution was added finely pulverized potassa: a reaction immediately commenced, and terminated in a few hours. On the pasty and heterogeneous mass was poured three or four times its volume of alcohol at 40°, and it was gently heated in such a way as not to boil. The decanted liquid gave, on cooling, a voluminous substance, of a very little density, that was purified by repeated washings, and crystallisations. The salt thus obtained is white, inodorous, greasy to the touch, sparingly soluble in cold alcohol; it burns, disengaging abundant vapors, in which predominates first the odor of sulphuret of carbon, then that of ethal. The carbonaceous residue was strongly alkaline, and contained sulphuret of potassium.

The analysis of the salt gives for its composition—

$$C^{64}H^{66}OC^2{}^2S.$$
$$KOC^2S^2.$$

It is then incontestable, that the sulphuret of carbon and ethal form, in reacting on one another, a combination of the same nature as that obtained by ordinary alcohol.

The salt of baryta was formed and analysed in the same manner.

The reactions of the sulpho-carbocctate of potassa are very like those of the xanthate; nevertheless it precipitates the salts of zinc, whilst with the xanthate the liquid remains perfectly transparent. Moreover, the hydrochloric acid gives, with the xanthate, xanthic acid, whilst it completely decomposes the new salt, and gives for residue perfectly pure ethal.

* *Comptes Rendus*, No. 12, September 19, 1842.

NEW MODE OF OBTAINING ETHER.

M. Gaultier de Claubry states, that by submitted alcohol, falling drop by drop, to the action of strongly heated organic acids, ether in abundance is immediately obtained. Oxalic, succinic, benzoic, and citric ether have been produced in this way. All chemists know that hitherto these ethers could be formed only with the assistance of the powerful mineral acids, as the sulphuric and hydrochloric: this has been donc away with by M. Gaultier de Glaubry, who heats the acid which he wants to etherify.

NEW INSTANCE OF THE PRODUCTION OF FORMIC ACID.*

BY M. LAURENT.

Having had occasion to analyse some small white granular crystals which adhered to the lids of some zinc vessels in which essence of turpentine was kept, M. Laurent found that they were composed of formiate of zinc.

ABSENCE OF ARSENIC FROM THE ZINC OF COMMERCE.

M. Jacquelain's investigations, undertaken under the direction of M. Dumas, have shown that the zinc of commerce, siliciferous zinc or carbonate of zinc, does not contain any arsenic. Henceforward, when arsenical stains are procured from Marsh's apparatus, it cannot be said that the arsenic arises from the zinc.

* *Comptes Rendus.*

II. CHEMICAL MANUFACTURES.

ON THE MANUFACTURE OF BEAVER AND SILK HATS, AND THE ILL EFFECTS PRODUCED BY WEARING THE LATTER.

Few people are aware of the evil consequences which may arise from constantly wearing a silk hat; they, indeed, may be sensible of the effects, but the cause remains hidden from them. As journalists, it is our duty, and a duty which we always cheerfully perform, to watch over the public welfare, and to point out every thing of an injurious nature; and we now, therefore, call public attention to a subject of vast importance—the ill effects of wearing silk hats. The consequence of this subject is derived from the member which is affected by wearing them, and from the great extent to which these hats are worn, owing to the extremely low price at which they are sold.

It is our intention here to give a brief outline of the method of making beaver and silk hats, from which we shall proceed to detail and trace to their origin the ill effects of the latter on the hair and head, and to show that the same objection does not apply to the former. We have been at very considerable pains to obtain the information which we now submit to the public, and which would have been published much earlier, but that it has occupied much time in collecting.

The manufacture of Beaver Hats is, perhaps, one of the most ingenious and interesting with which we are acquainted.* The various processes which it has to go through, render a beaver hat a very interesting specimen of ingenuity. It passes through a great number of hands; far too great for recapitulation here.

The "stuff body" of the hat, technically termed, "felt," is principally composed of rabbit's fur and wool; it is in one piece, without seam, and is of a conical form. This body is first "stiffened" by means of a liquid called "proofing," prepared by dissolving gums and the water-proofing materials in naphtha, turpentine, alcohol, &c. which is laid on by means of a brush.

The quality of the hat depends on the nature of the fur with which it is covered. The best hats are "ruffed" wholly with beaver: the second quality of fur is what is called newtre;* and the third quality is the fur of the hare, &c. &c. The fur, mixed with cotton,† is introduced upon the body (stiffened as above described), and is worked into it by rolling in woollen and hair cloths, in such a manner that that part of the fur which is next to the skin of the animal strikes into the pores of the "body." This is called "ruffing" or "rolling off."

When the body has been thus covered with fur, it is pulled out by the hand, in boiling "liquor," into the shape of a hat, and is then drawn on a block, and stretched to the

* Dr. Ure, in his *Dictionary of Arts, Manufactures, and Mines*, says that hat-making has been the subject of few improvements, owing to the Union; this, however, is far from being the fact. In what light does he regard the invention (for which a patent was granted in 1813) by Mr. Joseph Ashton, of the water proof hat? Does he mean to infer that this was not an improvement? We regard it as the greatest that has ever been made in this important branch of manufacture, and feel the greatest pleasure in thus giving public testimony to the talents and industry of that gentleman, who is well known to have devoted years and to have spent much money in bringing to perfection his very ingenious idea. However, as Dr. Ure's article on hat-making is in other respects miserably deficient in practical points, and as his information is evidently derived from inefficient sources, we must attribute his assertion to this cause rather than to any injustice on his part. We feel convinced, that if he had investigated the important subject as we have done, he would entirely concur with us.

* The great objection to this fur is, that when dyed, the color does not stand well; the hat turns brown very soon.

† All furs are mixed with a certain portion of cotton, which, as the fur in the "ruffing" process enters into the body, is left on the surface.

proper form in the same hot liquor. This is termed "blocking." While the hat is drying, any cotton that may remain in it is removed with a comb. The fur is now clipped, to meet the requirements of fashion, by which the fine rich tops of the beaver are lost to the public. It is now "dipped" in the "dye kettle," and has as many "suits" given it as are required for producing the necessary color. After this, it is "finished," "picked," "trimmed," and "shaped," or "tipped off."

Silk Hats are manufactured in a totally different manner. A body is made of pasteboard, willow, canvass, cane, Mackintosh, or stuff of the proper shape of the hat. Over the body is laid a strong varnish, principally composed of resin, linseed oil, and the commonest, as being the cheapest, naphtha that can be procured, to which silk hats owe their highly offensive, and, we believe, very unwholesome odor, by means of which the silk plush is made to adhere, and by which they are rendered air tight. The silk cannot be introduced into the body by the same means as the beaver is.

Our object in thus giving the details of the manufacture of each kind of hat, is to call particular attention to the materials of the body used in each, and to the difference between the two methods of covering the body. It will be perceived that the fur is introduced into the very substance of the body in such a manner as to have the appearance of having grown there, while the silk plush is laid on by means of a thick, glutinous varnish. To these differences are to be attributed the ill effects of which we are now about to speak.

It has long been observed, as is well known to the trade, that silk hats keep the head much warmer in summer, and much colder in winter, than beaver hats do, owing to the much greater impermeability of the former, caused by the layer of strong varnish above spoken of. This varnish, stopping ventilation, prevents the passing away of the warm vapor arising from the head while in a state of perspiration, causing it to condense again on the hair, sometimes covering it with large drops. Thus, at a time when ventilation is carried in other things to a prodigious extent, under the superintendence of Dr. Reid, the head is kept most prejudicially free from its beneficial effects.

Medical men are well aware of the importance of getting rid of the excretions, and will concur with us in saying that this effect of silk hats must be a highly injurious one, as, indeed, the facts which we shall adduce sufficiently prove it to be.

Men who constantly wear silk hats lose their hair much sooner than those who adhere to the use of beaver; and this will explain to many, who have not before been able to account for it, the reason of their becoming prematurely bald, and not unfrequently experiencing very unpleasant sensations about the head. The fact that those who wear silk hats become bald earlier than others is well known to every one in the hat trade. A traveller for a large firm has been heard to say that he would eat every hair left on the head of a man who had worn a silk hat for ten years; meaning, of course, that such a person would be completely bald.

It may not be out of place to mention here, that a manufacturer who makes very large quantities of the silk plush for hats, being desirous of patronising an article from which he derived considerable emolument, ordered a silk hat, and wore it for some time. He experienced very great uneasiness about the head, and, at the advice of a friend to whom he mentioned the circumstance, he left off the silk hat, returned to the use of a beaver hat, and suffered no further inconvenience.

We could state many other facts, but that space will not permit; the subject, however, may be resumed at a future period.

OBSERVATIONS ON THE EMPLOYMENT OF MORTARS AND CEMENTS IN BUILDING.

BY CHARLES WATT, JUN.

That this subject has ever been one of much importance, I think all who have ever paid any attention to it will be ready to admit: but of still more moment has it

lately become, in consequence of the great extent to which mortar is used in the construction of the tunnels of railways, &c., and which, from their peculiar situations, are generally exposed to much moisture.

In an article in a recent No. of this Journal, I showed that moisture and carbonic acid exert a very prejudicial effect on stones which are composed of the carbonates of lime and magnesia, and more especially the latter, as carbonic acid dissolves a rather considerable quantity of these compounds. Now, if such is the case with these carbonates in the state of aggregation in which we meet with them in nature, (and who can for a moment doubt it,) to how much greater extent must it be so with those carbonates which are formed by an exposure to the air of their bases, when they are in the most minute state of division, (I allude to mortar prepared by slaking, &c.) offering no resistance to the action of the carbonic acid contained in the air when carried into it by means of moisture.

My attention has for a considerable period been turned to this subject, having frequently observed some mortars in a complete powder, and which might actually be blown away from the bricks or stones between which it was placed.

I stated in the article before alluded to, that the carbonate of magnesia is more easily dissolved by the solvent action of carbonic acid than the carbonate of lime; and as limestones frequently contain this compound, it accounts for the fact of many mortars becoming "rotten," as it is termed, in so short a space of time, namely, because the lime of which they were made contained a large quantity of magnesia, as I have ascertained by direct experiment.

In the building of houses, the selection of mortar, although of some importance, is not of so great consequence as under many other circumstances; for, generally speaking, it will stand as long as the house itself; but it demands serious attention in the construction of fabrics through which moisture is apt to trickle, as in the long tunnels of railways, which, from their position, allow of the collection and retention of much moisture.

Such being the causes of decay, namely, the solvent action of moisture and carbonic acid on the carbonates, it is clear that for mortar, the chief constituent of which is carbonate of lime, some cement should be selected upon which this acid has no action: of such cements there are several; and it is to be borne in mind in such selection, that the more compact and the closer the texture of the cement when dry, the more effectually will it fulfil the required object.

In proof of what I have urged, I quote the following from a reeent Report by General Pasley on the State of "The Box Tunnel." He says,—"I examined also with great attention the brick-work and masonry on the other parts of the Box Tunnel, which are of good materials, and well executed, all the brickwork having been built of blue lias lime mortar of excellent quality: but though this mortar is in common use, and approved by civil engineers, I must confess that I would myself have preferred cement for those small portions of the brickwork which are generally in a damp or wet state, and from which stalactites, or a coating of dissolved lime, are to be seen on the surface."

From the article just quoted, it is evident that the long experience of General Pasley, a man who stands unrivalled on such subjects, brings him to the same conclusions to which my observations lead me, namely, that in damp situations cements should be selected in preference to mortar, of however fine a quality.

Where from necessity mortar is obliged to be used in the construction of underground buildings, I would propose that a layer of carbonate of lime should be placed over the upper or outer surface of brickwork, so that any moisture which might pass through would have all the carbonic acid which it contains completely saturated with lime before it reaches the mortar of the building, and its action to a very considerable extent averted. Or, instead of this, a thin coating of asphalte might be placed; and this, even when cement is used, would be found a great preservative, and as the layer need not be thick, the expensc would not be very great. I would also propose that the internal part of the tunnel be covered with a coating of pitch, to prevent the action of the steam emitted by the engines as they pass on the mortar or cement.

In conclusion, I would remark that, although the adoption of these suggestions may be attended with some difference in the expense, yet the parties concerned may depend on its being the cheapest in the end, as repairs will be less frequently required, which are generally of a very expensive nature, as it is frequently necessary to remove parts which are sound to get at those parts which are dilapidated.

FRAUDS IN THE SPIRIT TRADE.

Gentlemen,—

There are but few trades which offer greater facilities to imposture, or in which there are greater frauds practised, than that of a "Spirit Dealer." In proof of this asser-

tion I shall adduce a few of the many methods adopted by the trade to deceive and cheat the public, and especially the publicans, who, considering the licenses they have to pay out of their profits, are ill able to bear the imposition.

1st. I shall commence with the too frequent practice of "Dashing," as it is sometimes called, *i. e.*, the mixing of British with foreign spirits, and selling it for foreign.

2d. The sending out British brandy or rum in lieu of foreign.

3d. The sending out spirits of all denominations at very reduced strengths.

I shall endeavour to advert a little to each of the above systems; and, as far as I am able, point out the means of detection.

First, then, as regards the mixing. The law entirely forbids even the keeping of British and foreign spirits in the same place, or mixed, under a penalty of 100*l.* or 20*s.* for every gallon of spirits found so mixed. One would think that the penalty would be a sufficient protection to the public; but not so, when you consider how easy it is, when a dealer is going to send out 10 gallons of brandy, to draw 6 or 7 from one cask in one place, and the remainder from another in another place, just as it is ready to start. Again, the system of having permits granted to accompany all spirits sent out, under the penalty of 200*l.*, would appear, to a person unacquainted with the system, as a sufficient protection; but the fact is, that very little spirits are sent to private houses in country places with permits; and the reason is, that the excise generally do not trouble themselves about what is sent to private houses, as their survey is confined to publicans only; but the fact is, that the dealers, consequently, have an opportunity of obtaining just as many permits as may suit their convenience, to cover their frauds; and, at the same time, they so take care to obtain sufficient permits to keep their stock at a fair balance to meet the excise inspections.

Secondly. The sending out British exclusively for foreign is perhaps more practised on publicans than on other persons; and the plan adopted is, to draw a permit for British to accompany it; and, as the publicans generally are unacquainted with the sort of permit required for British and foreign spirits, the British being printed with black ink, while the foreign is printed with red, thus favors the deception; and, if anything is said, or their trickery is discovered, they will say, it was a mere mistake in drawing the permit, but it is singular that they take care never to make a mistake in drawing out the *invoice*.

Thirdly, The sending out spirits at a low rate of strength; this is practised in foreign as well as British, but the British favors the system most, as all spirits being once compounded, as gin, &c., having sugar, &c. mixed with the spirit, effectually prevents the possibility of trying the strength with the hydrometer. The weakness of the foreign is often made up by the addition of British, which gives it a hot, burning taste, and this is the only method of detection.

I am, Gentlemen,
Respectfully yours, M.

[We call the attention of the Excise authorities to the above communication. If proper steps be taken, and full publicity given to them, these frauds must soon cease. Our endeavors shall not be wanting.]

ADULTERATION OF TOBACCO.

Gentlemen,—

In perusing the last number of the Chemist, I observed an article on the adulteration of tobacco, and supposing that the enclosed specimens might be of some service in throwing light on the subject, I have sent them; and will now proceed to state the particulars.

About six months since, my attention as an excise officer was led to the subject of adulterated tobacco, from the startling fact of the manufactured article being sold at a less price than the duty alone.

I consequently purchased 4 oz., for which I paid 9*d.* at a retail shop in a county town. As there was a great deal of small among it, I shook it out, and commenced my experiment with ½ an oz. avoirdupois of the small. I steeped it in 20 oz. of boiling water, and left it until it was cold, when I stirred it up, and poured it through a coarse sieve, and then washed it again with the same quantity of cold water, and mixed both together. I allowed it to stand and settle; the earthy matter fell to the bottom, while the soluble and vegetable matters were either held in solution, or floated in the liquid, which I poured off, adding fresh water, and again allowing it to settle. This I repeated several times, and ultimately filtered it through bibulous paper, on which I dried the sediment. On taking it off, and weighing it, I found there were 32 grains, being, with respect to the weight of tobacco used, as one is to 7·33; besides which, the pores of the paper were completely filled up; so that there must have been several grains more. I next treated 1 oz. of the long tobacco in like manner, from which I obtained only 23 grains of sediment. Now, at this rate, allowing the other 2½ oz. to be in the same proportion, adulterated as this last, and adding the 32 grains from the

first ½ oz., we find the proportion of the sediment to be as 1 is to 16.

The method of making it adhere to the tobacco, is simply this. It is mixed up with molasses, water, &c., and poured, while hot, from a watering pan on the tobacco, when it is "laid down," as it is technically called.

As the enclosed specimens have been inspected and handled by several persons, it is probable there may not be the quantity specified above.

I am, Sir,
Your Obedient Servant,
An Excise Officer.

It is evident that a large portion of molasses must have been used, as the amount of sediment is not sufficiently great to enable the dealer to sell the tobacco at 3*s*. per pound.

We would call the attention of our correspondent, and of the excise generally, to the enormous extent to which rhubarb leaves, "malt combings," treacle, &c. &c., are used in the manufacture of tobacco.

In a recent visit to the manufacturing districts, we carefully examined many samples of the tobacco sold in the shops, and found that the country dealers fully equal their metropolitan brethren in the art of defrauding the government and the public. In fact we may almost go so far as to assert, that the tobacco sold in the provinces is worse than that palmed upon the London consumers.

How, in the face of these damning proofs of the existence of this adulteration, any man can be so hardy as to deny it, puzzles us. Certainly opportunity will occasionally carry men great lengths; but it is somewhat surprising, that a man should state to be false that which every one knows to be true, We wonder how Mr. Bremner enjoys the uncomfortable notoriety into which he has forced himself. High-minded, Quixotic, champion of the aspersed tobacco dealers!

We thank "An Excise Officer;" and we wish that others connected with the Excise would follow his excellent example, and furnish the public with the particulars of every adulteration which comes under their notice, and they must meet with many. If all the officers were to set to work in this righteous cause, fraud would soon diminish in extent, or even altogether cease, and the Excise would be regarded as a great benefit to the public.

We may mention, although it is scarcely worth while, that it has been intimated to us, that some of the immaculate dealers lately defended from our aspersions by Mr. A. B. Bremner, are so wrath, that they have been heard to threaten us with a most dire infliction of vengeance, should we be rash enough to approach Mincing-lane. We assure these parties that we are not in the least affected by the blustering threats of such valorous champions, and that we shall visit Mincing-lane as often as may be required for the purpose we have in view.

Their threats are treated, we assure them, with the utmost derision: we are not to be diverted from our duty by any menaces of personal violence. We have yet much more in store, and which shall be published at a fitting opportunity.

ON THE ELECTROTYPE AND ON CERTAIN PHENOMENA CONNECTED WITH IT.*

BY M. BOQUILLON.

The author commences by noticing a certain number of phenomena which he observed in the course of some galvano-plastic operations, and which seems to him to be of a nature to modify the generally admitted theories, especially as regards the electrical actions which are exerted at the surface of bodies.

In a second part, he devotes himself to the selection to be made, in electrotype experiments, among the different sources of electricity, and describes a new couple of his own invention, the employment of which is very advantageous in an economical point of view, and allows the hydrogen, which is so abundantly developed during the precipitation of the metal, to be collected.

M. Boquillon afterwards explains the circumstances which exert an influence on the molecular state of the metal, or of the alloy deposited, and shows how, by taking into consideration the greater or less stability of the metallic salts employed, and the degree of their solubility, the operation may be conducted in such a manner as to obtain, at will, the deposit of a metal as hard and brittle as steel, or as soft and flexible as lead.

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* *Comptes Rendus*, No. 10, Sept. 5, 1842.

When it is desired to obtain a deposit of a precious, or any other metal, it is important to pay attention to the circumstances just spoken of; since, according to the greater or less rapidity with which the precipitation takes place, and according as the crystallisation of the new metal is more or less confused, the adhesion will be more or less intimate. M. Boquillon says, that if these precautions be neglected, and if the conditions in which the operation is each time performed be not taken into the account, uniformity in the results can never be expected, which is of great importance in the industrial application of a process.

CYANIDE OF SODIUM IN THE SODA OF COMMERCE.

To the Editors of the Chemist.

Gentlemen,—

It is my impression that I am not the only one who has, from its commencement, regarded your valuable miscellany with some degree of interest. Permit me, then, to offer one remark, which is this, that the more frequent contributions of practical men, even involving discussions, would, in the opinion of many, be attended with advantages to your readers generally.

Your conjectures respecting the origin of hydrocyanic acid in the soda of commerce induce me to offer the following observations to your notice :—

Some time since, on examining "rough varieties" which I had made to determine the per centage of alkali, towards the point of saturation, I found I had struck a faint blue color, which, however, increased in intensity with the addition of acid. The color was augmented by the addition of hydrochloric acid, and a very good prussian blue was ultimately separated by filtration.

On repeating the experiment with another portion, I failed to produce the blue precipitate. When searching for the cause of the new product, it appeared to be attributable to the sulphate employed being produced by the double decomposition of sulphate of ammonia and muriate of soda, at a sal ammoniac maker's, from whom I had purchased it. Hence the cyanogen. Where I failed in getting the blue was with the so-called "cylinder saltcake."

Yours obediently,

W. E. Gill.

[I have no doubt that if all the muriate or sulphate of ammonia is not removed, by volatilization, previous to the sulphate of soda being decomposed, a portion of cyanide of sodium might sometimes be formed from the nitrogen obtained from that source; but that this was not the cause of its presence in some of the samples in which I have detected it, I am confident, as I know that the soda was manufactured with the sulphuric acid obtained from iron pyrites.

The presence of iron appears to me to exert much influence over the formation of the cyanide, as I have failed to detect it, even when I knew the soda to have been manufactured from the sulphate obtained by the decomposition of sulphate of ammonia and chloride of sodium, or in soda prepared by any other means, unless that body was present.

Charles Watt, Jun.]

INDIAN PREVENTION OF DRY ROT.

BY JOSEPH SMITH.

At Yaynankhyvin, on the banks of the Irrawaddy, are the celebrated Petroleum Wells, which supply the kingdom of Burmah, and the adjacent countries as far south as Junk-Ceylon, with *lamp-oil*. The priests, whose religious works are all written upon the leaf of the talipot-palm (*Corypha umbraculifera*,) smear the surface with this mineral oil, for the double purpose of filling the interstices with a dark matter which shall render the letters visible, and in order to protect them from the attacks of insects, and the ravages of time. I have seen such books of various ages, from fifty to one hundred and fifty years, whose antiquity could be verified without difficulty—their preservation having been attributed solely to the indestructible properties of the Petroleum, in which they had been soaked. Having mentioned these facts, it only remains for me to draw the attention of scientific persons to them, with the view of testing the qualities of Yaynan, as a *preventive to dry rot in timber*. The experiment may be tried without much expense, by saturating some planks with this liquid, and giving them a fair trial with others not so protected. There cannot be the same objection made to this mineral production as there is to arsenic or corrosive sublimate, inasmuch as the Yaynan is not perfectly innoxious, but has rather an agreeable odor.—*Indian News, 25th July.*

III. PHARMACY, &c.

THE PHARMACEUTICAL SOCIETY.

It having been intimated to us that a report has been circulated to the effect that, at the period of the formation of the Pharmaceutical Society, we made several applications to the council to recognise THE CHEMIST as its organ, and were refused on the ground of our being strangers, whose sincerity was not proved, we deem it necessary to set the minds of our readers and the public right on the subject, not for any importance that attaches to the report, but merely for the cause of truth.

Where, or by whom the falsehood was hatched is to us matter neither of knowledge nor concern; nor would it have excited the slightest notice, could we have imagined it possible that it could have gained currency through the unique specimen of diction so *characteristic* and *peculiarly editorial* as that which appeared in the January Number of the farrago gratuitously furnished to the members of the society, in return for the exorbitant tax of two guineas per annum; in this case it would indeed require only that the quarter whence it proceeded should be known to satisfy the public of its contemptibleness, worthlessness, and utter demerit of credit or notice. Nor would we on any account have exhumed it from the oblivious contempt in which it has been justly suffered to repose, had not the subject been brought to our notice by persons entitled to consideration.

We will place before our readers the real state of facts, and they are now too well known to require minute detail.

We advised the formation of a society,—not the Pharmaceutical Society *as now constituted and managed*—for the maintenance of the rights of, and for the prevention of aggressions upon chemists and druggists by the apothecaries, who, not content with the usurpation of every branch of the sciences of medicine and surgery, wish to secure also all the advantages of those whose privilege to advise a dose of rhubarb, or to assist the poor in slight ailments, over their counter, have excited their envy and cupidity. Our advice as to the formation of such a society preceded any efforts, or even the conviction of the necessity of making any on the part of chemists, more than six months.* Nor could they be induced by us to stir themselves until Mr. Hawes's bill excited their fears. There were meetings convened, and it was found that THE CHEMIST was right, and had stood forward, as its title declares itself to be, the protector of the rights of chemists in the hour of need. For ourselves, thanks and eulogies were lavished upon us from all parts of the kingdom, except from that to which they had been most important—London. Here, it was now made quite clear, that some sinister jobbing was at work in the back ground.

Among those who were convinced of the virtue of our services, Mr. Dinneford, from a sense of justice to us for our struggle in the cause, offered, and exerted himself to do for THE CHEMIST that which it had been the means of doing for the trade; and when his efforts were unsuccessful, felt that the treatment we had received was not such as was due to us, or a just return for the exertions we had so mainly contributed to produce in the formation of that society, which would now, if no stop were put to it, destroy the trade far more effectually than can any machinations of the apothecaries. With him we were in daily communication, devising means to complete that which we had suggested, and urged them to adopt; and he can bear testimony that we never offered ourselves as the organ of the society; he made the overture, and took much trouble to effect the object, advising certain alterations in the title of our journal, which we did not make. So much for the truth of the assertion, let it lie at whose door it may—"palmam qui meruit ferat"—and of

* See *The Chemist*, No. IX., page 281.

the palm which falsehood confers we shal certainly not attempt to deprive its author.

Now as to the ground of the alleged refusal on account of our being " strangers, whose sincerity had not been proved:" Sincerity not proved ! ! Why, we warned them of the danger six months before they even saw it, and advised the formation of an organised body. We who are not members of the trade—who keep no shop—who have therefore no object in, nor opportunity of, inviting physicians, surgeons, and others, to form a junto, and to regale with milk punch, in order to insure the Society's concerns slipping easily into our own hands. Sincerity ! ! ! Let any rational man contrast the situation of ourselves, conductors of an independent journal, and totally free from all connection with chemists and druggists as a body, with that of a man brought up to, and legitimately concerned in, nothing but drug-selling and medicine-dispensing, who wriggles and smuggles himself into every office the society has to bestow, makes rules, (and mark their fairness !) for those who, although his juniors, are able to be his teachers; and it will soon appear on which side presumptive evidence of sincerity preponderates.

Our object, in suggesting the formation of the Society, was to protect the trade, and not to give certain parties the opportunity of truckling to physicians and surgeons for private interests. How it has fulfilled its pledges to the subscribers, the cases of Mr. Kay and the gentleman at Liverpool are best evidences.

Is our sincerity to be questioned by one so incompetent to conduct that which he has assumed to preside over, merely because we are not to be stultified into an acquiescence with the idiotic exhibitions which take place at Bloomsbury Square.

That we are not, by many hundreds, the only parties dissatisfied with and disgusted at the conduct of those entrusted with the management of the Society's affairs, we can amply attest. We have too long conducted our Journal to be in the dark in matters relating to chemists and the conductors of the Pharmaceutical farce. We know of the prevalence of the greatest distrust, and let next time of subscription tell how justly we have stated the general feeling. We certainly agree with them that the intended reduced subscription of one guinea per annum will be a much nearer approach to the real value of the transactions than the present tax. Can it be supposed,—is it probable,—that we should first suggest and advise the formation of a Society, and then seek to injure or destroy it? Why have we withdrawn our support? Because its affairs are confided to jobbers and ignoramuses. What will our readers and the public think, when informed that the oracle and secretary, when discussing the composition of *pilula ferri composita*, agreed that equal quantities of either soda or potassa would be proper, although every babe in chemistry well knows that the atomic weight of soda is 31·3, and that of potassa 47·15. Rather than such a piece of gross and scandalous ignorance should have been exhibited before the public, to the disgrace of a Society we so labored to establish, we would gladly have sent them one of the youngest of the Junior Drug Club, to have shown them the error, and set them right.

It may be urged, that such minuteness is immaterial in Pharmacy. Indeed ! Is this one of the wise maxims of these rulers of the trade. We reply, science is always important for its own sake, and still more so from its application to so interesting a subject as medicine.

Does it follow, because we are at war with its present management we would wish to destroy it? On the contrary, it shows our deep concern for its welfare. Which would most evince his concern for a vessel he had built, the man who confides its navigation to persons of skill, or he who leaves it to the reckless blundering of knaves and blockheads?

The elegant trope of the " Mouse and the rat," is of a kind of which we should indeed be sorry to rob the author of the credit, but we can assure him that the little quadruped vermin are by no means so injurious and

destructive to the Society as the biped ones which continually prey upon it. However, the continual *squeaking* of *one* must be a most insufferable nuisance; why can't some of the officials catch it, or set a trap? We suppose they leave it for the amusement of some more potent animal, as a kitten. We cannot be serious; such trash is too despicable for the consideration of our minds or the record of our pages.

JUNIOR DRUG CLUB.

A MEETING of the "Junior Drug Club" was held at the King's Arms Tavern, Snow Hill, on Wednesday, the 5th October, to receive the Report of the Deputation appointed at a public meeting to prepare the memorial, and to wait upon Sir James Graham with the same. Mr. Whipple in the chair.

The minutes of the last meeting having been read and confirmed, the Chairman read the Report of the Deputation as follows:—

Your deputation having met to deliberate upon the best mode of proceeding, opened a correspondence, by means of their secretary, with the Home Office, requesting an interview with Sir James Graham to deliver the memorial. A courteous reply was received, stating that Sir James Graham was on the point of leaving town, but would be happy to receive the memorial of the Druggists' Assistants, and requested that it might be transmitted to him.

Having duly prepared the said memorial, signed by the chairman on behalf of the meeting, the chairman and secretary were appointed to deliver the same at the Home Office, and accordingly had an interview with the private secretary of Sir James Graham, who received them very politely, listened attentively to their explanations on the subject, and stated that he knew Sir James Graham was very anxious to collect all the information he could respecting the various branches of medical reform. He further promised to transmit the memorial to Sir James immediately.

From the above reception your deputation anticipate much good will result to the trade in general, and to the assistants in particular.

The report having been received and confirmed, a vote of thanks was unanimously given to the deputation, and ordered to be inscribed on the books of the club. Mr. Whipple having acknowledged the compliment, exhorted the members and assistants in general not to relax their efforts in securing their rights and privileges, the meeting then separated.

[We can but reiterate Mr. Whipple's exhortation, and recommend the members of this club to persevere; success will attend their efforts. They have done perfectly right in going to government direct, passing by the Pharmaceutical Society.]

ON THE INFLUENCE OF IMPRESSIONS MADE ON THE MOTHER DURING GESTATION, OVER THE CONFORMATION OF THE CHILD WITH WHICH SHE IS PREGNANT.*

BY M. GUISLAIN.

AMONG the vulgar nothing is better established than the relations which exist between the greater part of deformities which new-born infants present, and the objects which have struck the imagination of their mothers during the course of gestation. But, in proportion as this opinion is accredited by the people, it has been, up to the present time, renounced by men of science. The object of M. Guislain is to guard physicians against seeing things too exclusively, according to their own notions, and to prove that it is impossible, in the present state of science, not to admit, at least in part, the vulgar opinion on this subject. The following are the facts on which the author of this memoir bases his opinions: they are all witnessed by members of the Society of Medicine of Ghent.

Observation 1. Josephine Minnebol was a few weeks advanced in pregnancy; she was in good health; one of her acquaintance came to see her, wearing in her ears earrings, called in Flanders, "petites cloches," (little bells, from their shape). These jewels caught the affections of Josephine—she admired them, coveted them—gazed upon them with eagerness. Forthwith she was beset with an ardent desire of possessing similar earrings. At the period of her confinement she brought into the world a healthy infant, offering the singular phenomenon of two appendages, an inch in length, and shaped like acorns, hanging by a pedicle to the antitragus of each ear.

Observation 2. A woman, aged 29 years, pregnant of her fourth child, and in the third month of her pregnancy, was struck one morning at market, at the sight of the liver of a ray-fish. She experienced an irresistible desire to eat it, but she forbore, from the idea that some inconvenience might result from it, some deformity to her child. At the same time she inadvertently gave

* *Annales de la Société Médicale de Gand.*

herself a knock on the cheek, and right eye. When she was confined (which was at the usual period), it was observed, that her infant, a little girl, was marked on the cheek and right eyelids, with a large stain of a greyish yellow color—exactly analogous to the color of a ray-fish's liver. I have seen this child many times since, and it still preserves its hideous maternus.

Observation 3. M——, an advocate, went with his wife to visit a farmer, in whose house she saw a child having a supernumerary thumb. She had been pregnant a few days. Some time afterwards, she had a dream during the night, in which she saw this same infant, and this same double thumb; —she got up, and uttered a cry of alarm. After this, she was frequently occupied with the idea that her infant would be born deformed. At the eighth month she was delivered of a still-born child, which had a supernumerary thumb.

Observation 4. Dr. ——, member of the Medical Society of Ghent, relates the following concerning his wife. His wife, who is not at all superstitious, was pregnant for the fourth time, and in the fourth month of her pregnancy, when, in the winter of 1832, she missed her footing, and fell at full length, first on her knees, and then upon her elbows; this caused her knees, thighs and elbows to be denuded of skin; she was immediately anxious for the fate of her infant, and she frequently made use of the expression to her husband—"Provided that our child does not suffer from it." Being confined at the usual period, she brought iuto the world a male child, with the knees, thighs and elbows presenting the appearance of torn flesh, denuded of skin.

Observation 5. A woman, in the fifth month of her pregnancy, was engaged in applying a certain number of leeches to the chest of a young man affected with pleurisy. Some time afterwards she brought into the world a healthy child, but presenting on the left side of its chest marks analogous to leech-bites. It is confidently asserted, that there was a large red ecchymosed patch to be seen close by the former. Twenty-four leeches had been applied, and twenty-four spots were found upon the body of the infant.

With regard to two facts, one of which is relative to a woman, who, after having dressed a blister on the back of her husband, was delivered of a child who was marked in the same region with a rising of the skin, caused by serum beneath; the second, relative to a mother, who, frightened at the sight of a camel, was brought to bed soon afterwards of a child affected with spina bifida,—I admit that I do not attach any importance to them; the lesions which the children presented being perfectly explicable, without our bringing to our aid impressions transmitted from the mother to the fœtus.

Let us return to more striking cases.

Observation 6. A child fell down, and made its cheek bleed, in the presence of its mother, who was pregnant, and much frightened. The latter carried her hand to her own face; she was confined late, and her infant presented on its left cheek a *nævus maternus*, resembling a stain of blood.

Observation 7. A woman of Ghent, during the course of her third pregnancy, conceived an insurmountable aversion for a person whom she often saw, and who had her hands bent back at the wrists. She was delivered of an eight months' child, which presented a complete doubling back of the hand upon the fore-arm.

M. Guislain also treats of the influence which certain impressions of different animals, perceived by the mother, are able to transmit to the children, as well as of that which the character proper to each kind of animal exercises during incubation, upon the individuals confided to it. He brings forward the fact of a pregnant cat, which struggled with a terrier with crooked fore-legs. A short time afterwards she brought forth a kitten, the fore paws of which were crooked, and remained crooked, like those of the terrier. Thus, a hen's egg, sat upon by a magpie, was productive of a game cock, surpassing every other animal of the same kind. Pigeons of a lofty flight covered by heavy birds, were no longer able to quit the earth; and *vice versâ*, poulets produced from eggs sat upon by pigeons, have been observed to fly higher, and keep longer on the wing, than other birds of their kind.

The conclusions of M. Guislain are the following:—

1. "Monstrosities can resemble determinate objects, perceived through the sensorium of the mother."

2. "These influences are capable of being communicated to the product of the ovaries, even before its adherence has been effected to the maternal body."

3. "These influences are observed among certain animals, as well as in the human kind."

NEW MODE OF COLLECTING LACTUCARIUM.*

BY M. ARNAUD, OF NANCY.

Of all the preparations and products of the lettuce, lactucarium is, without doubt,

* *Journal de Pharmacie et de Chimie*, Sept. 1842.

the most preferable, on account of its more certain and more uniform action; but the difficulty of procuring considerable quantities of this substance, which is not met with in commerce, its high price, especially on account of the much larger dose (4 to 8 decigr.), as compared with opium, in which it requires to be administered, are causes which have led to the abandonment of this medicine, so valuable in cases wherein the exhibition of calming remedies is indicated; and when, on the other hand, they are contra-indicated, on account of a too great nervous sensibility, or of a tendency to cerebral congestion, which it does not aggravate, as do opium and other narcotics.

Such are the motives which have induced me to have lactucarium collected under my own superintendence, aud to seek some means of obtaining it with the least possible expense. The following is the plan at which I have arrived:—

Cultivate a number of lettuces, particularly of the variety called cabbage, or Batavian lettuce;* when these lettuces are raised, but before the development of the lateral branches, the stem of twelve plants must be cut a little below the commencement of these branches; the twelve plants being cut, and returning to the first, a milky exulation is found on the cut portion, and on that which remains fixed in the earth; this milky exudation must be adroitly collected with the end of the finger, which is afterwards scraped on the edge of a small glass; the same operation is performed on twelve other heads, and so on. On the third day it is repeated on every portion of plant remaining in the ground, a thin slice being first cut off the top: this is done every day until the root is reached.

By means of this process, the minute details of which will be easily found by practice; there is procured with an equal amount of hand labor, and the same number of plants, at least fifteen or twenty times as much lactucarium as could be obtained by simple pressure.

As soon as the lactucarium is collected, it coagulates; the harvest of each day should be divided into small pieces, which should be placed on plates, very near each other, but without touching, and allowed to dry for two days, after which they may be set aside in a bottle.

* This variety, from its acquiring a bitter taste when the temperature is raised, is little cultivated for culinary purposes, but being more than any other kind of lettuce susceptible of being pricked again with success, it is, for these two reasons, very well adapted for the production of lactucarium.

From what has just been said, it will be evident that from the mode of collection proposed, a considerable reduction in the price of lactucarium must result, and that it will cease to be, as hitherto, an object merely of curiosity, and will be ranked among the useful medicines, and which cannot be replaced; as is proved by the observations, certainly not very numerous, of distinguished medical men—observations which will probably increase in number if this medicine becomes more common, and if it is found in the shops of most druggists, at a price rendering it accessible to many.

PREPARATION OF SYRUP OF BALSAM OF TOLU.*

BY EUGENE MARCHAND.

Some time since, M. Deville called the attention of pharmaceutists to the preparation of syrup of balsam of tolu. He was led, in working on balsam of tolu, to suppose that this syrup might be prepared, without injuring its quality, by employing a smaller quantity than that directed in the Pharmacopœia.

M. Deville's ideas were highly interesting, and it was so important for pharmaceutists that their accuracy should be verified, that I decided on making some experiments, the results of which are here given.

To solve the problem set forth by M. Deville, I set out with the idea that water can be charged, if no intermedium be employed, with only very small quantities of volatile oil, at least, unless by distillation, because then it may be completely saturated with it; but that if any intermedium be employed, such as sugar, alcohol, magnesia, &c., a much larger proportion of volatile oil may be combined with it. Hence I concluded that, by triturating balsam of tolu with sugar, and by pouring on it some boiling simple syrup, I should be able to obtain a very powerful syrup. My anticipations were confirmed beyond my hope, and I have no doubt that the formula which I propose will be generally adopted, since it gives a syrup as agreeable and powerful as that of the Pharmacopœia, although the proportion which I employed for the preparation of my syrup is only $\frac{1}{10}$ of that prescribed by the Pharmacopœia.

The following is my formula:—

℞	Balsam of tolu . . .	40
	White sugar	80
	Cold water	150
	Cold simple syrup . .	2500

Triturate the balsam of tolu for a long time with the sugar, so as to make an impal-

* *Journal de Chimie Médicale*, Oct. 1842.

bable powder of it; introduce this powder into a china or tin vessel, with a cover, add the water, and pour on the boiling simple syrup. Stir occasionally, until it is quite cold, and cover the vessel while the mixture is not being stirred. When the syrup is cold, or after twelve hours' contact, filter through paper.

TOXICOLOGICAL INVESTIGATIONS CONCERNING ANTIMONY.

BY MM. FLAUDIN AND DAUGER.

The following are the conclusions at which MM. Flaudin and Dauger have arrived.

1. It is easy to detect antimony combined in small proportions with animal matters; we have been enabled to collect it with the same accuracy as arsenic.

2. The process which has succeeded best is as follows:—Disorganise the animal matters by means of sulphuric acid; at the moment of liquefaction, add nitrate of soda, terminate the carbonisation and take up the dried charcoal by means of water acidulated by tartaric acid: the liquid is submitted to the ulterior investigations proper for characterising antimony.

3. In the case of poisoning by arsenic, complicated by the presence of antimony, the apparatus which we have proposed for the detection of arsenic appeared to us to simplify and facilitate the operations resorted to for separating these two bodies.

4. Contrary to arsenic, antimony is easily eliminated by the urine: in poisoning by antimonial preparations, it is in the liver that antimony is more especially found; it is not detected in the lungs, nor in the nervous, muscular and osseous systems.

5. The fact of the localisation of poisons is a valuable datum for resolving certain little medico-legal questions; those of simulation of poisoning, for example.

6. This fact appears to us to open a new field to physiological and therapeutical investigations.

EFFECTS OF THE SULPHATE OF QUININE ON ANIMALS. CASE OF POISONING BY THIS SALT IN THE HUMAN SUBJECT.*

BY PROFESSOR GIACOMINI.

After having described at length the researches which have preceded his own on the toxical action of sulphate of quinine, Dr. Giacomini passes on to the description of numerous experiments which he has performed on the same subject.

The greatest precautions were taken to avoid all causes of error, and it is principally upon rabbits that he has experimented. On administering 4 grammes of the salt of quinine, no symptom was observed; but after having administered the same dose in 45 grammes of distilled water, with the addition of 22 drops of sulphuric acid, the animal expired after some minutes in a state of perfect calm. This point being established, and the rapidity of the death having proved that this quantity was more than sufficient to kill a large rabbit, about 2 grammes of sulphate of quinine, dissolved in 30 grammes of water, with the addition of a sufficient quantity of the sulphuric acid, were given. Immediately afterwards 5 grammes of double-distilled laurel-water were administered. Death took place almost instantaneously.

In another experiment, a rabbit of the same size took the same quantity of sulphate of quinine, and swallowed immediately afterwards 2 grammes of alcohol, dissolved in 3 grammes of distilled water; it appeared stupefied, then it crawled along, and allowed itself to be taken hold of without attempting to get away. Next day it recovered. —In another rabbit of the same strength a mixture of 3 grammes of sulphate of quinine was given dissolved in water, and a gramme and a quarter of alcohol, mixed with 8 grammes of water. A little lassitude, which passed off at the end of seven hours, and of which no trace was left next day, was the result of this administration. The case turned out differently in another animal of the same kind, to which a mixture of 3 grammes of sulphate of quinine, and 5 grammes of double-distilled laurel-water, were given; in a few minutes the animal died convulsed.

These experiments having been repeated in different ways, the result was, that in almost every instance where the sulphate of quinine was mixed with diluted alcohol, recovery took place; and that when death did occur, it was not until a longer or shorter interval elapsed; that the mixture of laurel-water and sulphate of quinine, far from depriving the latter of its poisonous properties, on the contrary augments them, since all the rabbits to which this mixture was administered, died almost immediately.

After these experiments, Dr. Giacomini reports a case of poisoning by sulphate of quinine, which seems to us worthy of record here.

A man, from 40 to 50 years of age, delicate, of sedentary habits, by mistake put 12 grammes of sulphate of quinine in a glass of sugared water, believing it to be cream of tartar, and swallowed it. An hour afterwards, he experienced extreme pain in the

* *Annali di Medicina*, February, 1841.

stomach, and singing in the ears, as if he were in the first stage of drunkenness. By degrees his strength failed, the noises in the head augmented, and nausea with headache supervened. His feelings of uneasiness soon became insupportable, and he lost all consciousness. Nine hours afterwards he was visited by Dr. Giacomini, who found him lying on his back, motionless, pale, with the fingers already livid and cold, the respiration slow and irregular. During the moments of syncope, the pulse was regular, slow, and scarcely perceptible, the heart's pulsations feeble, pupillary apertures dilated, sight and hearing almost entirely suspended, loss of voice, sighing, and breath cold. Dr. Giacomini prescribed the following mixture:—

℞. Orange flower water . . 30 grammes.
Distilled mint and cinnamon water, aa . . . 25 ditto.
Tincture of Thebaïa . . 30 drops.
Simple syrup, q. s.

Of this mixture the patient took two spoonfuls every hour. Frictions were employed, and he was covered with warm clothes. An alvine evacuation was procured by the administration of an enema; warmth soon returned. On the fifth day the patient was not yet able to stand upon his feet; the weakness and deafness were some time before they disappeared.

The conclusion at which this Memoir arrives is, that the sulphate of quinine, far from being a tonic medicine, has a well marked debilitating action, which it is necessary to combat with diffusible stimulants, and in particular with alcohol.

TANNIN AS AN ANTIDOTE FOR STRYCHNIA.*

BY DR. LÜDICKE.

A VERY delicate woman, aged 40, was for a long time troubled with an acute wandering pain, which manifested itself alternately in the region of the stomach, of the descending colon, in the left intercostal muscles, and in the pelvis, and which Dr. Lüdicke regarded as a rhumatico-spasmodic affection.

After other remedies, nitrate of strychnia was prescribed in the dose of $\frac{1}{24}$ of a grain (German), every three hours, in the form of a powder, associated with sugar.

Instead of strictly following the prescription, the patient ventured to double the dose, and even to take a double dose every hour, although the first dose had been sufficient to cause the commencement of vertigo: at the end of six hours, half a grain had been swallowed. Suddenly the patient, who was walking in the room, was seized with violent vertigo, and fell down backwards, which caused a contusion on the occiput, with a solution of continuity. When raised, she was found to have lost all consciousness.

A quarter of an hour afterwards, the physician arrived; and the patient, who had recovered her senses, related to him, with much effort and frequent interruptions, that, being suddenly overcome by vertigo, she had endeavoured to stoop in order to support herself on one of the marbles of her apartment, but that she was prevented by the sensation which she experienced at the curvature of her back; at the same time, her hands appeared to undergo the same impression. This state of episthotonos had already completely disappeared, but the pains in the back and trembling of the hands remained. The patient complained also of vertigo, which remained notwithstanding her recumbent position, accompanied by nausea. These two symptoms increased every time she wished to rise or to be placed on her seat: at the same time, there were vomitings of a colorless aqueous liquid. The respiration was difficult: the pulse was weak and quick. The motions of the arms, hands and fingers were, besides, perfectly free. The wound on the occiput was the seat of much pain, but an attentive examination showed that it was not serious.

In these circumstances, besides applications of cold water to the head, Dr. Lüdicke prescribed tannin; and, on account of the vomiting which occurred, he associated it with eitric acid and carbonate of soda dissolved in distilled water. The patient took only half a grain of tannin per hour. When the vomiting had subsided, the tannin was continued; but it was then given only in distilled water, with simple syrup. In twenty-four hours from the appearance of the symptoms of poisoning, all those symptoms had eeased. As the pure tannin, of which 60 centigrammes had been administered, might then be replaced by a less powerful astringent medicine, and Dr. Lüdicke, taking into eonsideration the great weakness of the patient, prescribed a decoction composed of 60 grammes of oak bark, to 180 grammes of colature and 30 grammes of syrup of cinnamon and 1 gramme of acetic ether.

By this treatment, the patient was completely cured, and the above-mentioned pain disappeared and did not again return. Neither the poison nor the antidote employed left behind any traces.

Dr. Lüdicke observes, in conclusion, that

* *Medicinische Zeitung*, No. 14, 1842.

the solution of tannin in distilled water, with simple syrup, has the appearance of clear whey ; that its taste is not disagreeable, and that it produces on the tongue a slight sharp sensation resembling that of fresh radishes. The decoction of oak bark with syrup of cinnamon has a very agreeable taste.

POISONING BY CHEESE.

BY DR. POLLIUS.

Nine persons, of both sexes, aged from 12 to 29 years, became ill after having eaten at breakfast, in three different houses, a kind of strong cheese. The symptoms of poisoning began to exhibit themselves two, three, and even four hours after the cheese had been swallowed. They experienced violent pains in the regions of the heart and epigastrium ; in some of the patients these symptoms afterwards extended to the whole abdomen: violent vomitings, in which even blood was ejected, and abundant diarrhœa, supervened. One of the patients experienced excessively painful cramps in the calves of his legs, and in some of them there was a trembling of the whole body. The other symptoms were alternate heat and cold; coldness of the extremities ; small, quick, and rather hard pulse; and distension and tenderness, or, on the contrary, contraction of the abdomen. All the patients complained of vertigo, lassitude, anxiety and thirst.

It was first sought to sustain the vomiting, and at the same time to moderate the too copious alvine evacuations, and the pains, by the administration of mucilaginous and oily preparations associated with extract of hyosciamus, and by the employment of fomentations, cataplasms and anodyne liniments. Wine and other analeptics were given to the weaker sufferers. Under the influence of this treatment, all the patients were restored to health in from eight to twenty-four hours.

The quantity of cheese taken by each person was estimated at from 4 to 15 grammes. The cheeses had been prepared in the ordinary manner, and weighed from 100 to 125 grammes each; they were soft, of an uniform dull white color, bordering on yellow; they presented in the thickness of their substance some portions of a deeper color and harder, of about the size of a pea ; they exhaled a peculiar and disagreeable odor; their taste was bitter and nauseating: however, no mites, mouldiness or other cryptogamic productions were found in them.

By boiling with water, the disagreeable odor of these cheeses became still more manifest; the decoction, filtered and slightly concentrated by evaporation, was milky, and had an acid reaction. By digestion in dilute nitric acid, the substance of the cheese gave a yellowish liquor. The toxical effects can be attributed to no other principles than the acid caseate of ammonia, and the acidified fat which these cheeses contained. These two bodies were isolated, and some pills were made by mixing them with crumb of bread; these pills were endeavored to be administered to mice, but only one of them tasted the pills prepared with the fat acid, and it experienced general trembling and very abundant blackish alvine evacuations.

NITRATE OF SILVER OINTMENT IN ERYSIPELAS.*

BY M. JOBERT.

M. Jobert has been for some months in the habit of employing nitrate of silver ointment in erysipelas; to these caustic applications he attributes the power of diminishing the painful and intolerable tension experienced in parts affected with erysipelas, and especially of limiting the extension of the disease. Three patients lying in a ward at the Hospital of St. Louis experienced remarkable relief in consequence of the application of this topical remedy. At No. 5, in St. Augustin's Ward, a laundress, 16 years of age, who came in to be operated on for strabismus, was seized with an attack of erysipelas in her right arm ; the skin was swollen and painful ; a single application of the ointment changed the appearance of the diseased skin and allayed the pain. At No. 71, in the same ward, was a woman of 60 years of age, in whom ulcerations on the fingers were productive of erysipelas, which spread from the forearm to the arm, and from thence to the trunk ; in this case there were want of sleep, restlessness, and fever. One application of the nitrate of silver ointment to the affected part relieved the pain, and limited the boundaries of the erysipelas. The same thing occurred in a man aged 65 years, who was affected with a severe traumatic erysipelas, which occupied the neck, forehead, ears, and part of the scalp, and which was accompanied with delirium ; here also the erysipelas was prevented from spreading. The ointment that M. Jobert employs for this purpose is the following:—

Lard . . .	30 parts.
Nitrate of silver .	8 parts.

[We have only two observations to make upon this subject: the first is, that in town women will not readily allow the exposed parts of their persons to be rubbed with an ointment, which will give for a long time

* *Journal de Pharmacie*, Sept. 1842.

afterwards a very disagreeable violet hue to their skin. Secondly, if this topical application has the power of quickly putting a stop to the spread of erysipelas, it would be advisable, before using it in all cases indiscriminately, to be assured that this rapid disappearance of a cutaneous disease, which is sometimes very extensive, will not produce some other inconvenience to the rest of the system.—Editors of *Journal de Pharmacie.*]

SIMPLE METHOD OF ARRESTING BLEEDING FROM THE NOSE.

BY DR. NEGRIER, OF ANGERS.

The plan which Dr. Negrier proposes is, he says, simple and certain. It is preferable to the occlusion of the nostrils, as that is difficult to maintain, especially in sleep. During three years that, in numerous cases, he has tried this method, which is simply elevating the patient's arm, he has never found it to fail. After detailing several cases, he thus explains the rationale of the plan. When a person is standing with the arms at the side, the blood which escapes from the upper part of the arch of the aorta takes two directions, viz. towards the head and towards the arms, and that which goes to the head is almost equal in quantity to that which is received by both superior extremities. If, however, the individual, who was formerly hanging his arms, raised them, the blood which was flowing horizontally and without effort from the subclavian into the brachial arteries, must then ascend against the weight of the column of blood contained in the latter; and as there is nothing in the act of raising the arm to increase the force of the circulation, it follows that part of the force formerly expended in sending the blood up the carotids must now be subtracted, and added to that which drives it through the brachial arteries. This explanation may or may not be confirmed by experiments. The subject is worth investigation.—Abridged from *Archives Générales de Médecine*, June, 1842.

CREOSOTE IN PHTHYSIS PULMONALIS.*

BY DR. FRANZ, OF KÖNIGSFELD.

A female, aged 34, was affected with tubercular phthysis, and the emaciation of the patient, the colliquative sweats, the apthæ, fetid expectoration, &c., showed that the disease was already in its last stage. The administration of creosote was then commenced, and after eight grammes of this substance had been given, the improvement obtained was such, that the patient was able to resume her operations.

A result no less favorable was obtained with the same medicine administered to a peasant, aged 28, who labored under phthysis.

But, in three other similar cases, its employment was obliged to be abandoned, because, under the influence of its action, the cough and all the hemoptoïcal symptoms were aggravated by it.

* *Medicinische Annalen.*

EMPLOYMENT OF ALUM IN THE GANGRENOUS SORE-THROAT, WHICH COMPLICATES SCARLATINA.*

BY DR. WINZHEIMER, OF EMERSHEIM.

Dr. Winzheimer has prescribed, with considerable success, in many cases of scarlatina, complicated with gangrenous sore-throat, insufflations of powdered potassa alum, in the dose of 50 centigrammes to 1 gramme. This treatment was repeated three or four times in the 24 hours.

EXTERNAL APPLICATION OF CROTON OIL.

BY M. BOUCHARDAT.

M. B. recommends a plaster, used by Chomel at the Hôtel Dieu; and which is thus prepared:—

Four parts of diachylon plaster are melted at a very gentle heat, and while it is half liquid, one part of croton oil is mixed with it, and the mixture is then spread in a thick layer on calico. Pieces cut from this may be applied to the skin like ordinary sticking-plaster. They quickly produce an active irritation.—*Bul. Gén. de Thérap.*, March, 1842.

DR. FAUCONNEAU-DUFRESNIE'S LAXATIVE SYRUP.†

℞ Powdered jalap . } aa 12 grammes
Muscovy rhubarb . }
Boiling water . . 150 ,,

Infuse in a close vessel until perfectly cool, strain with pressure, filter, and add to the product—

White sugar . . 260 grammes,

dissolve at a gentle heat, and aromatise with—

Tincture of orange peel, 50 grammes.

This syrup is advantageously employed against biliary calculi. It is taken in the dose of a spoonful every morning, either alone, or in a cup of bitter infusion.

* *Medicinisches Correspondenz-Blatt bayerischer Aerzte.*

† *Journal de Chimie Médicale.*

DR. SICHEL'S EMMENAGOGUE PILLS.*

R Gum ammoniac . } aa 4 grammes
Subcarbonate of iron }
Socotrine aloes . 1 ,,

Make fifty pills. From two to six are to be taken twice or three times *per diem*, an hour after meals, rapidly increasing the dose, if it can be borne. It is seldom necessary, in cases of dysmenorrhœa and obstinate constipation, to carry the dose of aloes beyond one or two grammes.

OINTMENT OF TAR AND CAMPHOR: ITS THERAPEUTICAL EMPLOYMENT.†

R Lard . . 30 grammes
Tar . . 4 ,,
Camphor . 50 centigrammes.

M. and F. S. A., a perfectly homogeneous pommade.

The proportions of tar and camphor may be doubled if necessary. There may also, in certain cases, be added to the mixture, to increase its resolutive power, one or two grammes, or even more, of liquid subacetate of lead.

DEATH FROM THE BITE OF A FLY.‡

A TANNER, aged 62, living at Saint Maurice, died in consequence of being bitten by a fly. This insect, which was of the most venomous kind, produced a malignant pustule. The face, which was the part bitten, first swelled up, and afterwards the whole of the body.

DEATH FROM THE STING OF WASPS.

A PERSON having tormented a swarm of these formidable insects, they immediately flew upon a child, aged four years, and stung it so severely as to cause death. This circumstance occurred about a month since, at Cuesmes, in Belgium.

PURIFICATION OF STORAX FOR INTERNAL USE, IN THE FORM OF PILLS, SYRUP OR DRAUGHT.§

BY M. P. R. LEPAGE, OF GISORS.

THE difficulty which is now experienced in procuring in commerce pure balsam of copaiba, and the disgust with which this medicine generally inspires patients, have induced a physician of our locality to try with the same object, liquid storax, which was recommended, some years ago, by M. L'Héritier.

The syrup, whose formula is due to this physician, was first tried and found perfectly inert, as I had foreseen; indeed it would be difficult to conceive how this syrup could participate in the properties of storax, since water removes scarcely anything from this product; it is only with difficulty, by a sufficiently prolonged digestion, that it acquires the odor and taste of storax.

The pills were likewise tried, and although the results obtained were rather more satisfactory than those furnished by the syrup, they still left much to be desired.

For the preparation of his prescriptions, M. L'Héritier directs purified storax to be employed; it is known that by this is meant in commerce, crude storax remelted in sea water, and strained through a cloth to free it from the *débris* of barks, &c. But, according to M. Guibourt, it is now almost impossible to obtain this kind of storax.

The liquid storax of commerce is also liable to be adulterated; now, in order to have preparations always identical with relation to the real quantity of active matter which they should contain, or with respect to the effects which the physician expects, I think that storax intended for internal use should be purified in the laboratory of the druggist. The following is the process which I employ, and which I think should be generally adopted.

Liquid storax, as is known, is insoluble in cold, and soluble in boiling alcohol. Now, if we introduce into a matrass one part of commercial storax, and 2 or 2½ parts of alcohol at 34, and heat the mixture in a sand-bath to the boiling point, a turbid liquor is obtained, which, rapidly filtered, deposits, on cooling, a fluid resin, of a greenish colour, almost transparent and very odorous.

If the operation be quickly performed, the liquor being divided among several filters, (this precaution is necessary only in operating on the large scale,) the filtration is promptly operated, and there remains on the filters, in the shape of residue, only impurities without resin; but when a little turbid liquor is left in the filter, it is sufficient to heat it again with a little alcohol, and to filter while hot;* thus all will pass through.

When the complete separation of the resin from the alcohol is effected, which requires about 12 hours, the supernantant alcohol is decanted, and it is distilled to free it from

* *Journal des Connaissances Médicales Pratiques.*

† *Journal de Chimie Médicale*, Sept. 1842.

‡ *Journal de la Somme.*

§ *Journal de Chimie Médicale*, Oct. 1842.

* It may be filtered hot by means of a funnel heated with steam.

the small quantity of resin which it holds in solution. This alcohol may be used for another operation.

The fluid resin which is deposited by cooling, or the storax, is heated for some time in a sand-bath, in order to volatilise the alcohol which it always contains. Thus purified, the storax is presented under the form of a semi-transparent greenish resin, of an agreeable odor, and of the consistence of turpentine; it is soluble in ether in almost all proportions; alcohol at 40° dissolves it very well, but it is sparingly soluble in alcohol at 33°; it must be kept in well-corked vessels, as full as possible, otherwise it forms a superficial layer of a crystalline appearance, which gradually increases.

The following are the formulæ which I propose to physicians who may wish to try this medicine.

I. PILLS.

℞ Storax purified by means of alcohol . . . . } 8 parts.
Calcined magnesia . . 1 ,,

Mix the magnesia accurately with the storax, and keep the mixture at the boiling temperature in a sand-bath for a quarter of an hour, stirring continually; after which, cool it, still keeping it stirred.

The above proportions give a pilular mass of a good consistence; but it would be well, I think, to divide it into pills of 30 centigrammes, for it hardens considerably by keeping, especially if it be kept in small quantity and in an imperfectly closed vessel. However, it is always easy to restore this mass to its primitive consistence, by melting it in a sand-bath.

By replacing half of the magnesia with two parts of dry hydrated peroxide of iron or by protoxide obtained by the decomposition of pure sulphate of iron by ammonia; and following, in washing that oxide, the rules to be adhered to in washing the carbonate of iron intended for the preparation of Vallet's pills; masses are obtained which may be substituted for the copahivate of iron, or copahine and iron of M. Mége.

II. SYRUP.

℞ Storax purified with alcohol 20
Alcohol at 40 50
Sugar 600
Gum arabic in powder . 30

Dissolve the storax with the aid of heat in the alcohol, pour the boiling and previously filtered solution into the sugar, divided into several portions, dry on a stove, pulverise and dissolve in a matrass, by means of a sand-heat, in 300 grammes of water; add to it the gum arabic, previously moistened with 50 grammes of water; shake the matrass occasionally, in order to facilitate the solution of the sugar, and when it is completely dissolved, strain the syrup through a sieve.

Thus prepared, this syrup is opaque, like that of almonds; its odor and taste are agreeable; part of the resin separates by long keeping, but by stirring it, it again mixes. 30 grammes of syrup represent about 60 centigrammes of purified storax.

Experience has taught me that the resin does not separate so quickly when gum is added.

III. DRAUGHT.

℞ Purified storax . . . 20 gr.
Yolk of 1 egg . . . 0 ,,
Gum syrup . . . . 60 ,,
Orange-flower water . 20 ,,
Peppermint-water . 80 ,,

Nitric ether may be added, if desired.

Purified storax may also be employed in enema, in this case it is necessary to hold it in suspension by means of yolk of egg, in the same manner as copaiba, camphor, &c.

MEANS OF REMEDYING CERTAIN ALTERATIONS OF THE MILK IN NURSING WOMEN.*

BY DR. CHABRELY.

I. A NURSE had the milk too clear and scanty; she did not feel it *rise*. The following means, employed by Dr. Chabrely, succeeded in two days in correcting the bad quality of this milk, and in greatly increasing the quantity of that secretion:—

℞ Magnesia . . . . . 10 grammes.
Powdered orange peel 3 ,,
White sugar . . . . . 65 ,,

M. and F. S. A. a perfectly homogeneous powder.

The nurse took, three times a day, in a cupful of a weak infusion of lime, properly sweetened, a teaspoonful of this mixture. Under the influence of this medicine, her appetite, previously bad, became excellent, and, owing to this circumstance and the direct effect of the medicine, her milk acquired in the space of two or three days all the qualities necessary for affording sufficient nourishment to the child.

II. Madame Armand B. had galactorrhœa for three months, which wasted her. The child fell away. In this case, Dr. Chabrely had recourse to the following medicament:—

℞ Bicarbonate of soda . 4 grammes
Powdered orange peel 4 ,,
White sugar . . . . . 80 ,,

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* *Journal Médicale de Bordeaux*, March, 1842.

M. and F. S. A. a perfectly homogeneous powder.

Madame Armand B. took, three times a day, a teaspoonful of this mixture in a cupful of a weak infusion of edulcorated *aya-pana*. Under the influence of this medicine, she entirely recovered her appetite, which had left her from the commencement of the galactorrhœa. Two such doses were sufficient to correct the milk and to suspend the incessant flowing. From this time, the lady was enabled to continue to suckle her child, to the great benefit of both, for, in a few days, they acquired their former plumpness.

PREPARATION AND THERAPEUTICAL EMPLOYMENT OF ANTHRAKOKALI AND FULIGOKALI.*

BY DR. POLYA.

Dr. Polya states that he has employed anthrokakoli with marked advantage in scrofula, chronic rheumatism, articular rheumatismal tumors, tophaceous arthritic concretions, and hydrarthrosis.

Anthrakokali is prepared by mixing in an iron basin 160 parts of powdered coal with 192 parts of a very concentrated and boiling solution of potassa causticised by lime. When the mixture is made, the vessel is removed from the fire. After removing it from the fire, continue to stir it until it is converted into a homogeneous black powder.

He administers three or four times a day the dose of 0·10 grammes, mixed with 0·25 grammes of powdered liquorice.

Many Parisian practitioners have tried the employment of this medicine, and among others, Dr. Gibert, who, at the *Hôpital Saint-Louis*, has prescribed in several cutaneous disorders. Its internal exhibition not having appeared to him very advantageous, this physician employed it under the form of a pommade according to the following formula:—

Authrakakoli . . .	1	gramme.
Lard	30	,,

M. and F. S. A. a homogeneous pommade.

It is applied morning and evening to the diseased parts.

Tried on 24 individuals affected wth skin diseases, it cured many of them, and its employment in others was signalised by a manifest amelioration.

The action of this medicine appeared resolute, but less exciting than the preparations of iodine or ammonia.

* *Gazette des Hôpitaux*, June, 1842.

FULIGOKALI.

This is a preparation of soot and potassa, which is prepared in the following manner:—

℞ Caustic potassa . .	20	grammes.
Powdered soot . . .	100	,,
Distilled water . . .	2	,,

Boil for an hour, cool, dilute with water to facilitate filtration; filter, evaporate, dry, in order to obtain the fuligokali in a black powder or in scales, and put it in dry and warm flasks.

For sulphuretted fuligokali:—

℞ Fuligokali	60	grammes.
Caustic potassa . .	14	,,
Sulphur	4	,,

Heat the sulphur and the potassa with a little water; after the sulphur is dissolved, add the fuligokali, evaporate, dry, &c.

Fuligokali has been tried by M. Gibert on his patients at the *Hôpital Saint-Louis*, both internally and externally. He made with 30 grammes of lead, and 1 or 2 grammes of fuligokali, a pommade in which he recognised resolutive, detersive and slightly stimulant properties.

ON STORACINE, OR THE CRYSTALLISABLE RESIN OF STORAX.*

BY M. LEPAGE.

It is known that M. Bonastre, who has so attentively studied the resins and balsams, has observed in liquid storax the existence of a crystallisable matter, to which he gave the name of storacine.

This matter cannot be obtained by the simple treatment of the storax of commerce by boiling alcohol, and by cooling the menstruum.

To obtain it readily, it is necessary to treat the storax of commerce directly with ether, without heat, to decant after several days' contact, and to leave the filtered liquor to spontaneous evaporation. The residue, conveniently dried, is redissolved in boiling alcohol of 40°; the filtered liquor is again left to spontaneous evaporation; when the liquid is evaporated to about three fourths, that which remains is decanted, and the crystals which adhere to the sides of the vessel are washed with a little cold alcohol, and dried on filtering paper; and, by dissolving once or twice more in hot alcohol, the crystallisable matter is obtained sufficiently pure.

This matter may also be obtained by treating several times by the alcohol of commerce; allowing each time a portion of

* *Journal de Chimie Médicale*, October, 1842.

the liquid to evaporate spontaneously, and compressing the matter obtained between folds of filtering paper. But in this process it is difficult to get rid of a green coloring matter, which always soils the crystals.

Thus obtained, the crystallisable resin of storax, or storacine, possesses the following properties :—

It is presented under the form of small needles, almost always agglomerated, very white, without perceptible taste, and of a slight, agreeable balsamic odor. It enters into fusion at 38°C.

It is completely insoluble in cold and boiling water; it floats on the latter in the form of oily drops, which slightly increase in consistence when the liquid cools.

It is sparingly soluble in alcohol, at 33°, but it is very soluble in alcohol at 40°, and still more so in ether.

Its alcoholic solution does not redden litmus paper; added to water it becomes milky.

Concentrated ammonia has no action on it.

It does not dissolve in even very concentrated solutions of potassa, and soda.

Sulphuric acid carbonises it when cold; with heat, the action is more energetic.

Hydrochloric acid does not appear to attack it, either with or without heat.

Nitric acid converts it into a very friable yellow matter, without any perceptible taste; it developes a very evident odor of bitter almonds: this, probably, would seem to indicate that storacine contains something analogous to the radical *cynnamyle*.

I should here say, that from an analysis which I have made of purified storax, this product should consist of—

A neutral crystallisable resin (storacine);
A soft uncrystallisable resin;
A green coloring matter;
Benzoïc acid;
Perhaps cynnamic acid.

All these substances are in proportions, which I have not determined.

Benzoïc acid is easily obtained from storax, by treating that substance with water and hydrate of lime, according to Scheele's process, for obtaining it from benzoïn.

The presence of benzoïc acid in storax, explains why its solution reddens litmus; while the well crystallised and pure resin does not do so.

It is worthy of remark, that when purified storax is boiled with water, it does not yield benzoïc acid to the liquid, as do the balsams in the same circumstances; for the water in which this product is boiled does not redden litmus paper.

BOOKS RECEIVED.

Lectures on Electricity, delivered in the Royal Victoria Gallery, Manchester, during the Session of 1841-2. By William Sturgeon. London: Sherwood, Gilbert, and Piper, 1842.

Medical Guide for Mothers in Pregnancy, &c. &c. &c. By J. R. Hancorn, M.R.C.S. Second Edition. London: Smith, Elder, and Co.

An Essay on the Nature and Application of Steam, with an Historical Notice of the Rise and Progressive Improvement of the Steam Engine. By M. A. Alderson, C.E. London: Sherwood, Gilbert, and Co.

[*These Books will be noticed in our next.*]

TO CORRESPONDENTS.

Chemicus, Dublin, will find his question answered by an article in this No.

If H. M. will explain himself at greater length, we shall be better able to answer him.

Those correspondents who are not answered in the present No. of The Chemist, are referred to the next.

Nota Bene.—*All Communications and Books for Review must be addressed, "To the Editors of the Chemist, Care of Mr. Hastings, Publisher, 13, Carey Street, Lincoln's-inn Fields, London." Communications must be prepaid, and sent before the 15th of each Month; Books for Review before the 10th.*

THE CHEMIST.

I. CHEMISTRY.

ON VALERIANIC ACID, THE VALERIANATES, &c. &c.

BY PRINCE LOUIS-LUCIEN BONAPARTE.

To the Editor of the Journal de Chimie Médicale.

SIR,—

In your Number for August, you have just published an article on the lactate and valerianate of quinine, the chemical properties and therapeutic action of which I first described at the last scientific meeting of Florence. I have not the least doubt that the inaccuracies as to what I stated of the therapeutic action of phloridzine, and of certain properties of the valerianate of quinine, have arisen from reports, in themselves inaccurate, and which have been copied in some of the Italian scientific Journals. In fact, I never said that the valerianate of quinine was less disagreeable to the taste, less bitter, less expensive, than the lactate and the sulphate of the same base,—a statement which must be incorrect, considering the disgusting odor and taste of the valerianates in general, and the high price of valerianic acid. All that I said about the therapeutic action of the valerianate was, that having tried it upon two individuals of the *Maremma* of Rome, comparatively with the sulphate, it scarcely deranged the nervous system at all, whereas the sulphate produced that effect even to the extent of complete deafness. I also stated that I hoped to succeed in preparing artificial valerianic acid from substances less expensive even than indigo, (as M. Gerhardt has lately done,) which would diminish the price of the valerianate; and that the salt which I used for my experiments had been prepared with valerianic acid extracted from valerian. As to phloridzine, far from affirming that it has been administered in intermittent fevers with as much advantage as the sulphate or lactate of quinine, I remarked, on the contrary, that it acted energetically only when the salts of quinine failed,—a circumstance of pretty frequent occurrence in some of the quartan fevers; whilst in many cases where the salts of quinine have succeeded, phloridzine has not produced any effect. It is also proper to observe, that in two cases of intermittent fevers, which yielded to the administration of salts of quinine, and which were not affected by phloridzine, the same substance has afterwards been successful, by administering it united with the subcarbonate of magnesia; a fact easily explicable by supposing that in the case where the phloridzine was unsuccessful, it was converted by the acids of the stomach into phloretine and grape sugar, (see the *Memoir on Phloridzine*, by M. Stass, in the *Annales de Chimie et de Physique*, t. 69, p. 180,) which was prevented in part, perhaps altogether, or partly, by the subcarbonate of magnesia. However this may be, in acknowledging that phloridzine is a remedy, not so uniform in its effects as the salts of quinine, I thought I might recommend it to the Italian physicians, among whom it was little known. I have tried phloretine three times, and, as one might have imagined, it has produced no medicinal effect.

I find that the lactate of quinine is possessed of great power; which may be explained from the nature of lactic acid, and from the solubility of the lactate. A fact which has been established by many physicians of the *Maremma* of Rome, and which goes to establish the superior energy of the lactate over the other salts of quinine, is, that quinine or its hydrate acts better than the sulphate, as well on account of the less derangement experienced in the nervous system, as from its more energetic action, even when a smaller quantity of quinine than that which is contained in a dose of the sulphate was administered. Nothing is more likely than that a great part of that quinine should act as the lactate, by combining with the lactic acid of the gastric juice, which always

contains that acid, according to Chevreul, Leuret, and Lassaigne. It is evident, admitting, with Prout and Braconnot, that the gastric juice owes its acidity to hydrochloric acid alone, that what has been said of the lactate of quinine will also apply to the hydrochlorate of that base. I also proposed, in therapeutics, the valerianic acid lemonade, many metallic lactates and valerianates, such as the lactates of mercury, cadmium, &c., and the valerianates of silver and zinc, in epilepsy, that of iron, &c.; in a word, all the chemical compounds in which the negative element exerts a therapeutic power analogous to that of the positive element, or else altogether contrary,—either with the idea of counteracting or diminishing any had effects which this last may exert upon the economy, or as its adjuvant. It is for this purpose that I prepared picrate of quinine, and that I am now occupied with other compounds of this kind, such as the hydriodate and hydrobromate of conicine, as deobstruents, &c.

I embrace this opportunity, Sir, of begging you to insert, as soon as possible, in one of your forthcoming numbers, together with the corrections I have just made with respect to the article that has reference to myself,—a paper on the valerianic acid, the valerianates, the lactate of quinine, and some other new salts of that base, which have not yet been described in any of the works on Chemistry. Although there are many points which I have not been able to clear up, regarding the chemical investigation of valerianic acid, I hope that the details into which I am about to enter, will be acceptable to chemists, to physicians, and to pharmaceutists.

Agréez, &c.

Louis-Lucien Bonaparte.

Florence, 15th Aug. 1842.

ON VALERIANIC ACID, THE VALERIANATES, THE LACTATE OF QUININE, AND CERTAIN OTHER NEW SALTS OF THIS BASE.

I have introduced some modification into the process generally adopted for the preparation of valerianic acid. I distil 20 kilogrammes of the root of valerian well bruised beforehand, with eight times its weight of rain water, which does not contain any of the earthy carbonates; I place in a retort the crude essence of valerian, continuing the distillation until the water acquires a sufficiently marked acidity; (about 120 kilogrammes of water to 20 of root.) In operating, as I always have done, on valerian with very slender roots, which is called the *German* valerian, and which is the richest in valerianic acid, the crude matter must be agitated several times with milk of lime, which will remove the small quantity of valerianic acid that it contains. I say the small quantity, for, contrary to the opinion generally admitted, it is solely the distilled water that furnishes nearly all the valerianic acid. The acid contained in the distilled water is then completely saturated, first with the milk of lime which was used to get away the acid from the crude essence, and then with a fresh portion of milk of lime; finally, the saturation with milk of lime, of which a slight excess is put, is completed. Thus we have in solution valerianate of lime, which must he concentrated until a pellicle appears upon the surface; the very concentrated valerianate is left to cool, and is decomposed by a sufficient quantity of nitric acid in a long and narrow flask, which may be sealed hermetically. The valerianic acid, which is more or less colored, floats on the surface of a very concentrated solution of nitrate of lime; it is decanted and distilled at a moderate temperature, changing the receiver when the drops of acid cease to be oleaginous, or, what is better, when the acid boils at an equahle temperature. Milk of lime has the advantage of being more economical than subcarhonate of magnesia and hydrate of potassa; it has also the advantage over the first of these salts in saturating more promptly the acid contained in the water distilled from the valerian, and an excess of lime does not affect the purity of the product, for it is soon precipitated in the state of carhonate, which does not occur with a solution of hydrate of potassa, which, during the concentration, always terminates by altering the valerianic acid. The carbonate of lime, especially in the state of marble, with difficulty accomplishes the saturation of the acid; and it is absolutely necessary to raise the temperature, which always occasions a loss of acid. Nitric acid is preferable to sulphuric for the decomposition of the valerianate of lime; for with this latter acid there is formed a pulverulent sulphate of lime, from which the valerianic acid is not completely separated without considerable difficulty; nothing is to be feared from an excess of nitric acid, which exerts no action upon the valerianic acid; it is even necessary to add this acid in considerable excess, if it is desired that the oleaginous valerianic acid should not likewise retain in oleaginous chemical combination lime in the state of acid valerianate. I shall hereafter consider the acid valerianates. It is with the valerianic acid extracted from valerian, that I have always obtained the valerianate of quinine for therapeutic purposes. I confess that, although very much disposed

to believe in the identity of the natural acid with the artificial acid obtained by the action of hydrate of potassa in fusion, on indigo, on the oil of potatoes, or on the essence of camomile, I have yet some slight doubt in this respect, in consequence of the disagreement of chemists as to the real boiling point of this acid. MM. Dumas and Stass, in their second Memoir on *Chemical Types*, (in *Annales de Chimie et de Physique*, t. 73, p. 144,) express themselves thus :—"We have confounded natural valerianic acid with that which is prepared from the oil of potatoes and potassa, on account of this latter possessing all the properties of valerianic acid, and because it has exactly the same composition. It differs from it, however, in two points—1st, that its boiling point is 175° ; 2ndly, that it is not congealed at 15°. The valerianic acid, on the contrary, would boil at 132°, and would be congealed at 12° ; but these respective numbers appear to us to be founded upon an error resulting from the employment of trihydrated valerianic acid, or from a mixture which contained it." Having by me some very pure valerianic acid extracted from valerian and containing only one equivalent of water, I observed that its point of ebullition was as high as 76°, whilst it did not become congealed before it reached 15°, like that of M. Dumas. These results very nearly correspond with those of that illustrious chemist ; and I know not to what to attribute the slight difference in the point of ebullition, unless it be to the inaccuracy of my thermometer, or else to the greater purity of my valerianic acid, which had been distilled five times running at a very moderate heat, care having been taken to separate at each distillation the first products. I no longer doubted the identity of the two acids ; and the valerianate of quinine obtained with the artificial acid possessed absolutely the same chemical and physical properties as the valerianate of natural acid. No doubt their therapeutic action is the same as that of the natural acid, although I have not yet made the comparison ; but upon my return to the *Maremma* of Rome, I intend to make a trial of the two acids and of the two valerianates.

VALERIANATES.

The valerianates which have hitherto been examined are those of potassa, soda, ammonia, baryta, strontia, lime, magnesia, glucina, zirconia, of the protoxide of manganese, of zinc, of the sesquioxide of iron, of cobalt, of nickel, of lead, of binoxide of copper, of binoxide of mercury and of silver.*

I have only to describe four new valerianates, those of quinine, cadmium, uranyle,* and uranium, and to make some remarks upon those of baryta and silver.

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* *Vide Traité de Berzelius*, t. ii. p. 347, (last Brussels edition).

* In adopting the ideas of M. Péligot concerning the constitution of the yellow oxide of uranium, I regard this oxide, not as an oxide of uranium, but as an oxide of uranyle, and I call valerianate of oxide of uranyle, or, more simply, valerianate of uranyle, the combination of one eq. of valerianic oxide with one eq. of oxide of uranyle ($U^2 O^2$) O + V. I call the protoxide of uranium simply oxide of uranium, and the salts which are formed, salts of the oxide of uranium, or salts of uranium, because this new metal having only one degree of oxidation well known, like cadmium, lithium, &c., the name of protoxide is not necessary at present. In fact, the yellow oxide is an oxide of uranyle ; the black oxide, saline, or deutoxide, 2 U O + ($U^2 O^2$) O, a bibasate uranylate of uranium, and the saline tritoxide, or the olive oxide U O, + ($U^2 O^2$) O, a neutral uranylate of uranium. As to the two suboxides of which M. Péligot speaks, the one (the brown oxide) would be composed of $U^4 O^3$, and the other (the apple-green oxide) has not been analysed. The first of these sub-oxides might take the name of the triquadriuranic oxide. (See the very interesting Memoir of that eminent chemist, *Ann. de Ch. et de Phy.* 3e série, t. v. p. 5.)

In applying to the oxygenous compounds of uranium, the new nomenclature expressing the relation of equivalents, which I laid before the first Italian scientific meeting of Pisa, and which has not been wholly rejected by the Italian chemists, we should have the following appellations :—

Uranium, U, the metal discovered by M. Péligot ;

Uranic oxide, U + O, the protoxide of M. Péligot, or uranium, the supposed simple body ;

Uranyle, or bibiuranic oxide, $U^2 O^2$ U ;

Uranyle, a new radical of M. Péligot ;

Oxide of uranyle ($U^2 O^2$) + O, oxide of uranyle of M. Péligot, or the yellow oxide of chemists ;

Uranylate of uranium, U O + ($U^2 O^2$) O, the saline tritoxide, or the black oxide of M. Péligot, the protoxide of chemists ;

Triquadriuranic oxide, $U^4 O^3$, a brown suboxide, discovered by M. Péligot ;

Suboxide of uranium, the green suboxide discovered by M. Péligot.

(See *Esposizione d'una Nuova Nomenclatura Esperimente il Rapporto Atomico*, 2d edit. inserted in *Giornale Scientifico Litterario de Pérouse*, 1840.)

VALERIANATE OF QUININE.

I prepare this salt by saturating without heat a solution of valerianic acid in distilled water by a concentrated solution of quinine, in alcohol at 36° C., and I either submit the valerianate which is formed to a spontaneous evaporation, or else I evaporate it by a gentle heat. In either case, valerianate of quinine is obtained in beautiful crystals, having the figure of obliterated rectangular octohedrons, four of whose faces are very large, the other four very small.

When these crystals are formed by evaporation, they are so grouped that the octohedral form is not easily recognised, but by spontaneous evaporation they are procured sufficiently isolated and in a very obvious form. These crystals are very hard to the touch, and do not resemble other salts of quinine, which nearly always crystallise in silky needles. The valerianate may, however, be obtained under this last form, provided the solution is subjected to the continued action of heat, which should never reach the boiling point, because this salt readily assumes the resinous form and separates itself from the heated fluid in the form of brownish drops, the nature of which I have not yet examined. However, it occasionally happens that the octohedral crystallisation cannot be prevented, even though the solution of the valerianate has been heated. It appears, then, that this salt exists under two forms of crystallisation, and perhaps the resinous drops are a substance isomeric with the valerianate of quinine. These drops are dissolved with difficulty in water, and their formation takes place without any sensible disengagement of valerianic acid;—it is proper to mention that their resinous form may explain their difficult solubility in water. The valerianate of quinine once crystallised is not very soluble in water, but it is very soluble in alcohol, and almost insoluble in ether, which, however, swells it up much. It has, like the rest of the valerianates, a slightly disagreeable odor of valerianic acid; like them, it is decomposed by most other organic acids; —no doubt, the acids contained in the gastric juice, would set the valerianic acid of this salt at liberty; however, its effects are uniform in spite of its decomposition, which may be attributed to the uniform proportion itself, in comparison with the quinine, of valerianic acid set at liberty, which is perhaps most favorable to its therapeutic action.

VALERIANATE OF CADMIUM.

The carbonate of cadmium is decomposed very slowly by a solution of valerianic acid; however, after being in contact some days, especially if a considerable excess of the carbonate be employed, the combination takes place even at the ordinary temperature. The solution, evaporated till a pellicle forms, furnishes beautiful plates of a silvery tint, similar to those of boracic acid, but thicker in appearance. This salt is dissolved in water and in alcohol.

VALERIANATE OF URANYLE.

This salt is obtained by decomposing one equivalent of valerianate of silver, by one equivalent of chloride of uranyle. The solution, left to evaporate spontaneously, and excluded from the light, is dried up into a kind of shining varnish of a beautiful yellow color. I have not succeeded in producing crystals from this valerianate, which is very soluble in water, in alcohol, and in ether. Exposed to a moderate heat, it gives off its valerianic acid, leaving behind a residuum of oxide of uranyle.

VALERIANATE OF URANIUM.

When a solution of valerianate of uranyle containing a great excess of valerianic acid, is exposed to the sun's rays, in a stoppered bottle, the oxide of uranyle is reduced and converted into oxide of uranium;—its oxygen goes to the elements of the valerianic acid in combination, which is decomposed, giving off a gas which I have not yet satisfactorily examined, but which I expect to be a mixture of carbonic oxide, carbonic acid, of bicarburet of hydrogen, and of hydrogen. During this decomposition, a violet substance is deposited at the bottom of the vessel, being nothing else than a more or less saturated valerianate of uranium, according as the valerianic acid was in greater or less excess in the solution. It is this excess of acid that has not undergone decomposition, which is combined with the oxide of uranium. It is probable that in order to obtain a neutral valerianate, it would be necessary to add to the solution of the valerianate of uranyle, an excess of two equivalents of valerianic acid. I intend, as soon as I am able, to re-investigate the products of the decomposition of the valerianate of uranyle dissolved in water under the influence of the solar ray. Supposing the decomposition to take place (which, however, I am very far from asserting) with the production of carbonic acid, carbonic oxide, of the bicarburet, of hydrogen and water, then the following statement will be correct:—

$$(U^2\,O^2)\,(O + H^9\,Cl\,O^3) + (2H^9\,C^{10}\,O^3)\,(UO = H^9C^{10}O^3) + C^2\,O^2 + CO^2 + 7H\,C + H^2.$$

The valerianate of uranium is of a somewhat violet color, insoluble, and is con-

verted by the action of air during desiccation into a yellowish powder. It is right, however, to observe, that the valerianate upon which I operated contained the hydrated oxide of uranium. Perhaps the neutral valerianate would not undergo any change when exposed to the action of the atmosphere; and the yellowish compound into which my valerianate containing the hydrate of uranium was converted, owed its color merely to the conversion of this last body into hydrate of uranyle. I have no more of the valerianate of uranyle sufficiently good, in my opinion, to make fresh experiments with; but I am inclined to think that by exposing to the sun's rays a perfectly neutral solution of valerianate of uranyle, we should obtain hydrate of uranium alone, which being well washed, dried, and heated to 300°, ought to furnish oxide of uranyle, anhydrous and pure, identical with that obtained from the oxalate of uranyle by M. Ebelmen, to whom we owe the method of preparing that oxide, which he was the first to separate.* The solution of valerianate of uranyle, from which the violet precipitate is thrown down, has a very characteristic odor, which differs from that of valerianic acid. The small quantity of solution upon which I experimented would not allow me to push my researches as far as I could have wished. It would be interesting to study in detail all the products formed during the decomposition of the other salts of uranyle by the organic acid under the influence of the solar ray.

VALERIANATE OF BARYTA.

I have nothing very interesting to say of this salt, except that I should wish the point to be settled as to whether it is capable of crystallising in prisms, unalterable in the air, as Trommsdorff asserts,† or whether it is uncrystallisable, as Dumas asserts.‡ A concentrated solution of this salt left to spontaneous evaporation during many days in the summer time did not furnish any crystals, although it attained the consistence of syrup; at last it became similar to a kind of colorless jelly.

VALERIANATE OF SILVER.

This salt may be procured in beautiful lamellæ similar to those of boracic acid, by dissolving in boiling water, the precipitate which is formed in a neutral solution of nitrate of silver when a concentrated solution of valerianate of baryta is added. This salt blackens so speedily under the action of light especially when it is crystallised in lamellæ, that it is difficult to obtain it perfectly white, even when the operation is performed in the dark. It is not so soluble in water as one might suppose, especially in boiling water. It is perhaps the most economical way of preparing it, to treat the oxide of silver recently precipitated and still moist with a concentrated solution of valerianic acid until all the oxide is converted into a white crystalline powder of valerianate of silver, which must be dissolved in boiling water, if it is required to have it crystallised in beautiful plates. If monohydrated valerianic acid be poured upon the oxide of silver perfectly dried, great heat is elicited in consequence of the instantaneous formation of valerianate of silver.

ACID VALERIANATES.

If some oleaginous valerianic acid be poured upon a saturated solution of valerianate of quinine, and the liquor be agitated, oleaginous drops are observed in a short time to fall to the bottom of the liquid, whilst the free valerianic acid floats on the surface. When the valerianate of baryta is decomposed by an insufficient quantity of nitric acid, the oleaginous acid retains in a combination itself oleaginous, a considerable portion of valerianate of baryta. These two facts seem to point out the existence of acid valerianates, and that that of quinine is less soluble than the neutral valerianate.

VALERONE.

In subjecting valerianate of baryta to dry distillation, a liquid is produced of an odor altogether peculiar, the vapor of which is very irritating to the eyes and nostrils, and which may contain valerone, the more so as carbonate of baryta is found in the matrass.

NEW SALTS OF QUININE.

LACTATE.—By saturating lactic acid with quinine, and exposing the solution to spontaneous evaporation in a shallow vessel, groups of silky needles are at at length obtained, which are more flattened than those of the sulphate. The lactate of quinine does not crystallise so readily as the sulphate and the valerianate, and is more soluble than these latter salts.

FORMIATE of quinine easily crystallises, in needles similar to those of the sulphate, and it is very soluble in water. The formiate of cinchonine, on the contrary, is crystallised with much greater difficulty: the solution of this last salt, evaporated to the consistence of syrup, takes the form of a mass composed of needles interlacing one another.

I prepared the PICRATE of quinine, with

* *Vide Annales de Ch. et de Phy.* t. 5, p. 198.

† *Ann. der Pharmacie*, t. 1, p. 176.

‡ *Ann. de Ch. et de Phy.* t. 73, p. 135.

the idea of obtaining a salt the action of which would be very energetic, the negative element being very bitter and febrifuge, the same as the positive element;—notwithstanding this, the picrate of quinine is almost insoluble in water, which, however, is colored yellow by it. It is very sparingly soluble in the weak acids. It is one of the least soluble of the salts of quinine. It is less bitter than picric acid and quinine; administered in two cases of intermittent fever, it produced no effect. It is prepared by double decomposition, the sulphate of quinine and picrate of potassa being used;—it appears in the form of a fine yellow powder; alcohol dissolves it, and water being added, precipitates it again from the solution; the alcoholic solution does not furnish crystals on evaporation. Submitted to the action of boiling water, it soon swims upon the surface of the liquid in the form of small oleaginous drops of a brownish yellow color. The picrate of cinchonine resembles that of quinine in every particular.

I shall conclude by observing, that in the preparation of the salts of quinine, it is better, in general, to make saturated solutions cold, as heat very often causes precipitation and separation from the liquid, either of the salt just formed, or of the quinine itself which is added from time to time to saturate the acid. I remedy this inconvenience by using a very conceutrated solution of quinine in alcohol, with which and with the aqueous solution of the acid I make a cold saturated solution. The alcoholic solution coming in contact with the aqueous solution, yields the quinine in the nascent primitive state, which is very favorable for its saturation by the acid, that instantly takes it into solution.

ON THE COMBINATION OF CHLORINE WITH BASES.*

BY M. GAY-LUSSAC.

(*Concluded from page* 340.)

It has been seen that oxide of mercury, moistened with water, in contact with chlorine, produces chloride of mercury, and chlorous acid uncombined with the metallic oxide. This is not surprising, as chlorous acid is a very weak acid. Oxide of mercury is not a very energetic base, and it is known that such bodies are very often forced into combination only by a powerful antagonism. Thus, carbonic acid does not combine with oxide of mercury nor with alumina, and it unites very intimately, on the contrary, with the alkaline bases. It will, therefore, easily be conceived that the circumstances in which chlorine is put in contact with alkaline bases are the same as when it is put in contact with oxide of mercury; that it must form chlorous acid in both cases; and that there is only this difference,—that the chlorous acid formed, instead of remaiuing free in presence of alkaline bases, as takes place with the oxide of mercury, combines with them. The analogy here appears very great; but certainly can be acquired only by experiment.

To proceed with more accuracy, it will be necessary to caleulate in each particular case the quantities of chlorine and base which must be combined together in order to obtain determinate degrees of saturation. Two examples will suffice:—

If it be desired to make a neutral chloride of potassa, Cl PO, with $200^{c.c.}$ of potassa of the strength of 108°, how much chlorine will be required?

This quantity of chlorine represents $108^{\circ} \times \frac{200^{c.c.}}{50^{c.c.}} = 432^{al.}$, which must be reduced to chlorometrical degrees.

According to the table of the solution of alcalimetrical degrees to chlorometrical degrees (page 340), we have

For $400^{al.}$	$455^{ch.}$	264
30..	34..	145
2..	2..	276
$432^{al.}$ =	491..	685

Supposing the oxide of manganese to be of the strength of $85^{ch.}$ for 5 grammes, we should say:—

$85^{ch.} : 5\text{ gr.} :: 491^{al.} : 685 : x = 28^{gr.}92$

Thus it will be necessary to take $28^{gr.}92$ of oxide of manganese, and to receive into the solution of potassa all the chlorine it may give on being treated with an excess of muriatic acid, and neutral chloride of potassa will be obtained. This operation requires a little attention; no chlorine must be lost; and it is necessary to prevent the muriatic acid from getting to the potassa, so that the degree of saturation of the chloride which it is proposed to obtain may not be altered.

For a second example, we will take the preparation of an alkaline chloride of potassa; but we will commence with an important remark.

From the analogy which we have taken as a guide, and which we are endeavoring to confirm, chlorine, at the moment that it comes in contact with potassa, forms chlorate of potassa and chloride of potassium, which must remain passive in the different degrees of saturation which it is desired to obtain with chlorous acid and potassa.

If we take $200^{c.c.}$ of a solution of potassa, of the strength of 108°, with what quantity

* *Comptes Rendus*, No. 25, June 20, 1842.

of chlorine must it be combined to obtain 1 chlorite $\frac{3}{7}$?

As the chloride of potassium and the chlorite of potassa always contain the same quantity of chlorine, we shall have the formula—

$$\frac{3Cl}{3P} + \frac{3ClO}{4PO}$$

Consequently, the potassa will be divided into $3 + 4 = 7$ equivalents; and for this quantity of potassa only six equivalents of chlorine will be required.

Now, we have $200^{c.c.}$ of potassa, of the strength of $108^{al.}$ or $432^{al.}$, which represent our 7 equivalents. By taking $\frac{6}{7}$ of this number, we shall have $370^{al.}3$, which must be converted into chlorine. According to the table we shall have:—

For $300^{al.}0$	. . . .	$341^{ch.}448$
70 ·0	. . . .	79 ·671
0 ·3	. . . .	0 ·341
$370^{al.}3$	—	$421^{ch.}460$

Therefore, $421^{ch.}46$ degrees of chlorine will be required, and they will be obtained with $34^{gr.}79$ of the same manganese as was used in the last experiment. The chlorine, received into the solution of potassa, will give chlorite mixed only with chloride of potassium.

The general characters are the same in each. The decoloring power is in the same degree; tbere is the same instability, the same modification by the action of heat, and above all, the same products when they are treated by the acids.

Thus, we have said that the chlorites, decomposed by sulphuric acid in excess, gave only chlorous acid: but that as soon as they were mixed with metallic chlorides, nothing but chlorine was obtained; that, indeed, notwithstanding tbis mixture, all the chlorous acid was reproduced by adding only sufficient sulphuric acid to decompose the chlorite.

The chlorides of oxide, submitted to the same tests, acted in absolutely the same manner. When an excess of sulphuric acid was added, only chlorine was disengaged. When treated by only sufficient acid to decompose the chlorite which they are supposed to contain, or even rather less, they do not disengage chlorine, but chloric acid. However, if a chloride of oxide were only a direct combination of oxide and chlorine, the smallest quantity of sulphuric acid would immediately displace the chlorine, which is not the case. It therefore follows of necessity, to explain these facts, that chlorine, in passing into an alkaline solution, forms two different products: one very weak, which is first decomposed by the acids; the other more stable, which is afterwards decomposed: these two products can be only a chlorite of oxide and a metallic chloride.

Let us take for an example the neutral chloride of potassa, 2ClO PO, which should be converted into ClP + ClO PO. The quantity of sulphuric acid to be added in order to decompose only the chlorite will be half of that which it would have required to saturate all the potassa employed. With a chloride of potassa $\frac{9}{10}$, for the formation of which it would have been necessary to employ $18Cl + 19PO = \frac{9Cl}{9P} + \frac{9ClO}{10PO}$, the acid would be equal to $\frac{10}{19}$ of the quantity necessary for saturating all the potassa. But while we are unacquainted with the composition of chloride of potassa, we can operate its decomposition, *par tâtonnement*, by adding the acid in small quantities, until the moment at which the chlorine begins to manifest itself by coloring the previously perfectly colorless solution.

The saturation of the chloride of potassa by sulphuric acid is a very easy operation; however, I consider it useful to detail the manner in which I execute it.

I take an end of glass tubing, of about 15 millimetres in length, and draw it to a very fine point over a lamp. This tube, thus drawn out, is intended to perform the office of a funnel, and to allow the sulphuric acid poured into it to flow only very slowly. It passes into a cork fixed into a flask containing the chloride, and must reach almost to the bottom. The cork is likewise sloped to allow a free passage to the motion of the air.

The sulphuric acid destined for the saturation of the chloride is diluted with about twenty times its volume of water. It is poured into the funnel, and while it is flowing into the chlorate, a *rotatory* motion is given to the flask in order to disperse the acid throughout the whole mass of liquid, and to prevent local supersaturation, which would be productive of chlorine. There is thus poured into the chloride the quantity of acid which has been calculated, or indeed the operation is blindly carried on until chlorine is developed. The chlorous acid may be obtained by distillation.

Chloride of lime, which is so easily procured in commerce, may be employed in the preparation of chlorous acid, by decomposing it, with the precautions here indicated, by means of very dilute nitric acid.

Carbonic acid decomposes the chlorides of oxide, but it does so only partially; chlorous acid, which is very soluble, remains in the solution and ultimately arrests the action of the carbonic acid.

Thus $400^{c.c.}$ of a solution of chloride of lime, of the strength of $504^{ch.}$, saturated with carbonic acid and submitted to distillation, gave a first product of $75^{c.c.}$ of the strength of $576^{ch.}$, and another of $105^{c.c.}$ of the strength of $114^{ch.}$ The residue saturated with carbonic acid gave a fresh quantity of chlorous acid.

Chlorine itself decomposes the chlorites, at least partially; and, indeed, the distillation of the chlorate, after the admission of chlorine, always gives chlorous acid. It acts on the base, deoxides it and forms a metallic chloride, and a new quantity of chlorous acid which is added to that abandoned by the portion of base decomposed. But the action of chlorine is not limited to this simple effect. With this tendency to separate the oxygen from the base, it provokes a rupture of equilibrium which is very soon effected, and the greatest part of the oxygen is concentrated into the more stable compound $Cl\,O^5$, $RO,$—that is to say, the chlorite is decomposed into the chlorate. Whether we operate with a chlorite or with a chloride of oxide, the result is the same.*

The action of heat on the chlorites and the chlorides of oxide in solution is absolutely similar. This action is slow, gradual, and, consequently, enigmatical, like many others. It determines in general their conversion into chlorates; but a disengagement of oxygen constantly takes place, and it is so much the more considerable as the chlorous compound is more basic. In the table given below will be found the results of comparative experiments which were made simultaneously with the chlorite and chloride of potassa of different degrees of saturation.

The solutions of each of these salts were heated in the same bath of boiling water, for six or eight hours, in matrasses of capacities varying from 140 to 180 cubic centimetres. The neck was left empty to receive the increase of volume which the liquid experienced by the action of heat, without allowing it to arrive at the cork to which was adapted a tube entering into a bell receiver to collect the gas disengaged. We here give an example of the calculation of an operation with neutral chlorite of potassa.

Volume of chlorite . .	$156^{c.c.}$
Its strength before experiment	$648^{ch.}$
,, after seven hours in a sand-bath .	$5^{ch.}$
Consequent loss of strength .	$643^{ch.}$
Oxygen disengaged, volume corrected	$66^{c.c.}$

Supposing for a moment that all the loss of strength was due to the chlorine, we shall have its volume x by the following relation:—

$$156^{c.c.} \times 643^{ch.} = 100^{ch.} \times x;$$

whence $x = 1003^{c.c.}08$, which represent the decoloring matter. These $1003^{c.c.}08$ are divided into two equal parts: the one $501^{c.c.}54$ for the chlorine, and the other for the oxygen is represented by $\frac{501^{c.c.}54}{2}$ of this gas, while two volumes of the former are equal to only one volume of the latter.

Now, as the potassa of the chlorite contains as much oxygen as the chlorous acid, the whole of this body contained in the chlorite will be equal to $501^{c.c.}54$ of oxygen; that is to say, as a general rule, to the volume of chlorine contained in the chlorous acid.

Thus, in our example, the oxygen of the chlorite being $501^{c.c.}54$, and the oxygen disengaged or lost being $66^{c.c.}$, we shall have $\frac{66}{501^{c.c.}54} = 0{\cdot}13$ for the loss of oxygen, which heat causes in the chlorite by decomposing it and converting it into chlorate.

* I should remark that chlorous acid attacks the chlorides at the temperature of 100°, and even below that, and that it converts them into chlorates, with a disengagement of chlorine and a little oxygen. When this disengagement of chlorine takes place, the chlorite is always with an excess of acid. This is another means of testing its neutrality.

The conversion of a chloride into a chlorate by chlorous acid furnishes a convenient means of preparing the chlorates. It is sufficient to keep in a boiling sand-bath, or in the light, chlorous acid in which the chloride is dissolved. The acid is renewed when too much weakened, and for that purpose the solution is distilled to dryness or nearly so. The portions of acid which are disengaged, concentrated by distillation, may be employed *de novo*, or they may be used for the preparation of chlorous acid. By repeating these operations, the chloride may be completely converted into chlorate.

RESULTS OF THE ACTION OF HEAT ON SOLUTIONS OF CHLORITES AND CHLORIDES OF POTASSA OF DIFFERENT DEGREES OF CONCENTRATION.

Chlorites and Chlorides.	Degree of Saturation.	Loss of Oxygen, that of the Chlorite or of the Chloride = 1.
Acid chloride of potassa	$\frac{11}{10}$	0·03
Acid chlorite of potassa	$\frac{11}{10}$	0·07
Neutral chloride	$\frac{10}{10}$	0·03 to 0·10
Neutral chlorite	$\frac{10}{10}$	0·13
Basic chloride	$\frac{9}{10}$	0·21
Basic chlorite	$\frac{9}{10}$	0·33
Chloride	$\frac{8}{10}$	0.15
Chlorite	$\frac{8}{10}$	0·41 to 0·66
Chloride	$\frac{7}{10}$	0·12
Chlorite	$\frac{7}{10}$	0·13
Chloride	$\frac{5}{10}$	0·37
Chlorite	$\frac{5}{10}$	0·33
Chloride	$\frac{1}{4}$	0·30
Cblorite	$\frac{1}{4}$	0·36
Chloride and oxide of manganese	$\frac{9}{10}$	0·67 to 0·80
Chloride without oxide of manganese	$\frac{9}{10}$	0·25

It will first be remarked that, setting aside some anomalies, the losses of oxygen increase with the excess of base in each kind of salt. The chlorides and the chlorites $\frac{10}{10}$ and $\frac{11}{10}$ suffered losses which amount only from 3 to 13 hundredths, whilst the basic chlorites and chlorides $\frac{6}{10}$ and $\frac{1}{4}$ lost from 30 to 37 per 100.

The losses of oxygen for a chlorite and a chloride, of the same degree of saturation, present some remarkable differences; and most frequently they are greater in the chloride. However, for each strength $\frac{7}{10}$, $\frac{5}{10}$ and $\frac{1}{4}$, the losses are sensibly equal. I know not to what to attribute the discordance of the results. Perhaps the instability of these compounds is the principal cause. By adding peroxide of manganese to chloride of potassa $\frac{9}{10}$, which alone had lost only 25 per cent. of oxygen, tbe loss amounted in one experiment to 67, in another to 80. A very intense red color indicated the presence of hypermanganate of potassa in the solution. Brown oxide of copper acted with the chloride of potassa like the peroxide of manganese. It is remarkable that a solution of chlorous acid alone, and those of the chlorites and chlorides, placed in the same circumstances, are decomposed in an entirely similar manner. The instability of the acid seems to be very little diminished by the presence of a base; the action is also very slow, and the considerations which we have explained with regard to its decomposition by heat might also find their application here.

Whatever may be the cause of the discordances whicb are sometimes manifested in tbe decomposition of the chlorides and chlorites by heat, these discordances being likewise not uniform, and being produced as well among the chlorites as among the chlorides, it is no less probable, from the similitude of the general results of this action, that the chlorites and the chlorides contain a common compound, and that this compound can be only the same chlorite.

One very important consequence which flows from the facts which we have just mentioned, is, that in the preparation of the chlorides it is necessary to raise the heat as little as possible. Two effects are produced by it to the detriment of the chloride, —a disengagement of oxygen and a diminution of strength, which may cause the total loss; whilst by preventing an increase of temperature, the loss remains insignificant.

But independent of heat, there is another cause of loss in the preparation of the chlorides, which deserves very serious attention.

So long as the neutrality is not attacked, the chloride is maintained without alteration at the ordinary temperature; at least, its progress is very slow. But if neutralisation be exceeded, the strength of the chloride soon

rapidly diminishes; oxygen is disengaged in small bubbles in the proportion of two or three hundredths of all that contained in the chloride, and chlorate is formed. These effects take place in the chloride of lime as well as in that of potassa, and, at least within certain limits, they are independent of their degree of concentration: I say within certain limits of concentration, for I have seen neutral chloride of potassa, of the strength of 900° to 1000°, lose in eight hours, at the temperature of 15° to 18° C. and in the diffused light of my laboratory, more than nine tenths of its strength.

Thus, in the preparation of chlorides as bleaching substances, the best result will be obtained by not allowing the temperature to be raised too high, and by not exceeding or even altering the exact term of saturation.

As to the preparation of chlorate of potassa, it is now very evident that Berthollet's theory can no longer be admitted; for this theory, based on the sparing solubility of chlorate of potassa, supposes that so long as sufficient chlorine has not arrived in the solution for the chlorate formed to be held in solution, it is not really formed: consequently, that by taking very dilute potassa or bases forming only very soluble chlorates, as lime, chlorides only, and never chlorates, are obtained.

But it is uniformly the case, on the contrary, that very dilute solutions of potassa, lime and magnesia produce chlorates as soon as the chlorine is in excess, that a little oxygen is disengaged, and that the strength is then considerably diminished; while so long as the chlorine is not in excess, chloride of potassium may be obtained even very concentrated, without chlorate of potassa being formed. To carry into effect the action of an excess of chlorine on a chlorite or chloride,—that of potassa for example,—care must be taken that the chlorine acting on the potassa shall form chloride of potassium and a new quantity of chlorous acid, which should be added to that abandoned by the portion of base decomposed; that the chlorous acid itself should act on the chloride of potassium, converting it into chlorate; and that, consequently, it is much more natural that at the very moment of its nascent state, the equilibrium is broken, and that the chlorite is immediately transformed into chlorate without passing by circuitous routes, which nature always avoids.

However, the conversion of a chlorite into a chlorate may also take place without an excess of chlorine, by the sole agency of heat. The chlorite is decomposed, and the more stable compound, which is capable of offering resistance in these new circumstances, is immediately formed.

This is the general law: whenever there are formed, with the same elements, different compounds of unequal stability, but capable of existing in the same given circumstances, the least stable is formed first. If the circumstances change and can no longer be maintained, the immediately more stable compound succeeds it; and so on until we arrive at a much more stable compound, or until the elements of the compound are separated. We thus see chlorine and potassa produce chlorite of potassa and chloride of potassium at a low temperature; the chlorite is changed into chlorate at a higher temperature, then the chlorate into heptachlorate, and, finally, into oxygen and chloride of potassium. In the same way, sulphur and phosphorus, in contact with an alkaline base, produce at first hyposulphite and hypophosphite, and afterward sulphate and phosphate.

From the foregoing results, the most favorable condition for the preparation of the chlorate of potassa consists in slightly supersaturating the alkaline solution with chlorine. Then, either spontaneously, or by heating above the boiling point, the conversion of the chlorite into chlorate takes place.

When the chlorides of lime and potassium are employed for this preparation, it is likewise advantageous to supersaturate with a slight excess of chlorine. After it has produced its effect, this chlorine may be collected, and may contribute to the formation of a fresh quantity of chloride.

Thus, in the preparation of chlorate of potassa, it is expedient to prevent the temperature from rising too high, before the term of the saturation by chlorine; to slightly supersaturate the solution with chlorine, and to leave it to repose, or else to heat it to 80 or even 100 degrees: there is no longer, then, any inconvenience. By operating even with all these precautions, it will not be possible to avoid a loss of oxygen, but it will not exceed two or three hundredths.

I have obtained gaseous bromous acid by the same process as chlorous acid, but this is a field which M. Balard has reserved to himself.

I confine myself, for the present, to the observations which I have just presented, imperfect as they are. Being induced by some friends to hasten their publication, I must, for a short time, when I hope to be more free to devote myself to working in the laboratory, postpone rendering them more complete, and especially carefully studying the action of light on the bleaching compounds of chlorine. I may add, in conclusion, that M. Bouchardat and particularly M. Larivière have lent me their aid with an

assiduity which makes it a duty, as well as a pleasure, on my part, here to tender my acknowledgments.

ON THE CONVERSION OF BENZOÏC ACID INTO HIPPURIC ACID.*

BY W. KELLER.

Previous to Dr. A. Ure's investigations, Wöhler supposed that digestion converted benzoïc into hippuric acid. The confirmation of this idea by Dr. A. Ure induced him to confide to Mr. Keller a new study of this curious metamorphosis. Mr. Keller experimented on himself; he took, at night, 2 grammes of pure benzoïc acid in simple syrup. Next morning the urine was extraordinarily acid, even after having been evaporated and left to itself for 12 hours. It then deposited only the ordinary sediment of earthy salts; but some time after the addition of hydrochloric acid, it gave rise to a large quantity of long, brown, prismatic crystals, the appearance of which did not allow them to be regarded as benzoïc acid. Another portion of urine, concentrated, by evaporation, to the consistence of a syrup, was converted, by the addition of hydrochloric acid into a mass of crystalline bracteæ. This substance was pressed, dissolved in boiling water, treated with animal charcoal and submitted to several crystallisations. It was thus obtained in colorless prisms, of the length of 0·027 metres.

These crystals were pure hippuric acid. They readily fused by the aid of heat: at a higher temperature, the mass was carbonised, developing the odor of bitter almonds and subliming benzoïc acid. To dissipate all doubt, Mr. Keller determined its proportion of carbon: 0·300 grammes gave, by combustion, 64 per cent. of carbon. According to the formula $C^{18} H^{16} N^2 O^5 + H O$, crystallised hippuric acid contains 60·97 per cent. of carbon; crystallised benzoïc acid, on the contrary, contains 69·10 per cent.

He was thus able with facility to extract from urine a large quantity of hippuric acid, by swallowing benzoïc acid.

The author ascertained that the urine never ceased to contain urea and uric acid, and the proportions of these two bodies appeared to him to be the same as in normal urine. The recommendation of Mr. Ure to employ benzoïc acid in cases of uric acid concretions, gout and urinary calculi, seems, therefore, too precipitate. This chemist appears, indeed, to think that uric acid is employed in the conversion of benzoïc acid into hippuric. As his observation was made on a gouty woman, it may be admitted that this urine would not have contained uric acid but for the internal use of benzoïc acid. Besides, it is evident that hippuric acid, being separated only by the addition of an acid, is combined with a base in the urine.

* *Annalen der Chemie und Pharmacie.*

ON ANIMAL ELECTRICITY.*

BY M. MATTEUCCI.

M. Dumas communicated to the Royal Academy of Sciences, Paris, at its sitting of the 24th of October, some experiments which, in his opinion, would open a new era to the most delicate physiological investigations.

These experiments were detailed in a *paquet cacheté* deposited by M. Dumas, in the name of M. Matteucci, and which the author, who was present, wished to be opened.

The learned philosopher of Pisa, in the note contained in the *paquet cacheté*, thus expresses himself:—

"Prepare, quickly, the thigh of a frog, leaving the nerve attached to it; place this nerve on the thighs of another frog prepared in the ordinary manner. If you then compel the second frog to contract its muscles, either by electrical excitation, or by any other means, at the moment when the muscular contraction takes place, the muscles of the leg of the first frog are also seen to contract."

"If I am not mistaken, added M. Dumas, it is the first time that we have seen the contraction of the muscles of one animal exert any influence whatever on the muscles of another, and determine contraction; and if it be added that this influence is transmitted through a sheet of unsized paper placed between two animals, while it is stopped by a very thin sheet of gold, we shall comprehend all that is new in this class of phenomena discovered by M. Matteucci.

"This experiment, and some others no less clear, have been repeated by M. Matteucci in my laboratory, before MM. De Humboldt, Kupfer, Valenciennes, &c. Among the new experiments, I shall confine myself to citing the following.

"M. Matteucci very quickly prepared the leg of a frog to which he left the nerve attached, and he introduced this leg into a well varnished glass tube which he held in his hand. By afterwards making a wound in the muscle of any animal, living or recently killed, and by introducing into the wound the nerve of the leg which is in the glass tube, in such a manner that it may touch at one point the interior of the wound and at another the

* *Comptes Rendus*, No. 17, October 24, 1842.

edge of the wound or the surface of the muscle, the leg was seen to contract. By holding the leg thus isolated, it is not the current from the frog which is discharged by the body of the observer that produces the contraction: since it is necessary for the nervous filament and the muscular mass to touch at two different points; it is, indeed, an electric current to which the contraction must be attributed.

"This experiment must not be confounded with those of Aldini and others; for these, —the leg of the frog the nerve of which was passed into the wound not being isolated,— were not protected from contractions produced by the proper current; indeed, Aldini, in relating an experiment in wbich it was isolated on a small shelf (*banc*),—and it is the only one in which he seems to have taken precaution,—says (see his *Essay*, vol. 1, p. 17), that the frog did not contract in this case, which evidently proves that the contractions which he observed were due to the current circulating in the body of the observer. In the same way it was not established, that, to operate contractions by this method, it was necessary to make a connection with the nerve in two points of the muscular flesh, so that the experiment succeeded only by accident. However, these facts, established by so simple an experiment, are proved by M. Matteucci still more accurately by other experiments made with the galvanometer, which give the direction of this current, and which prove its independence of all chemical or other action, which might be supposed to be introduced by the experiment."

ON A NEW PROCESS FOR OBTAINING INDIGOTINE.*

EXTRACT OF A LETTER FROM M. FRITZSCHE TO M. CHEVREUL.

Being always occupied with indigo, although with the view of destroying its color, my experiments have this time led to a method of preparing pure indigotine, which I consider worthy of the attention of chemists, as it yields it in the crystalline form, easily and quickly.

I had long observed that indigo, treated by an alcoholic solution of potassa, gave, in certain conditions, small quantities of indigotine in bracteæ, and, by repeating my process, I have been enabled to produce the same result without fail. It is a simple reduction of indigo, in which alcohol is employed instead of water, and, because the substances ordinarily employed for effecting this reduction are not soluble in alcohol, grape sugar is used instead of them; for the same reason, potassa or soda must be substituted for lime; but as all these substances, with the exception of alcohol, have already been employed, the method essentially consists in the employment of alcohol.

I proceed thus:—I take one part of commercial indigo and one part of grape sugar; I put them in a bottle capable of containing 40 parts of liquid, and pour in alcohol until the bottle is half full, and I add to it a solution of $1\frac{1}{2}$ part of a very concentrated solution of caustic soda in the other half of the alcohol. The bottle, thus filled and well shaken, is left for some time in repose, and after the liquid has become clear it is removed by means of a syphon into another bottle. The liquid obtained, so long as the atmospheric air cannot get to it, is of so deep a yellowish red, that it is transparent only in very small quantities; but as soon as it comes in contact with oxygen, it acquires a purple color, and rapidly passes, small quantities being operated on, through all the shades of red, violet and blue, while the whole quantity of indigotine is deposited in bracteæ which are smaller or larger according to the quantities of liquid and the patience exerted in carrying on the oxidation very slowly. Although the crystals are always microscopical, a single glance is sufficient to show that the fine and very light powder which they form is really crystalline; and, as all the other substances remain undissolved from the commencement, or are dissolved after the precipitation of the indigotine, the latter is as pure as can be desired. After having washed it on a filter with a little alcohol, nothing more is required but to wash it with warm water, which is very quickly done. There are always deposited on the crystals small drops of a substance insoluble in alcohol, but very soluble in water, arising from the action of the soda on the grape sugar; this renders the washing indispensable.

It still remains for me to say a few words relative to the gain in the quantity of indigotine, and I am happy to be able to give you perfectly satisfactory numbers. Four ounces of very indifferent commercial indigo yielded from the first infusion 2 ounces of pure indigotine; a second infusion of the residue gave only 1 drachm, and the residue of this second infusion contained only very little of the coloring principle. This proves, in my opinion, that this method will be preferable to all others for testing the value of the different kinds of indigo in commerce,—a point which I have been compelled for the present to neglect, but to which I shall at another time recur.

* *Comptes Rendus*, No. 15, October 10, 1842.

ON LITHOFELLIC ACID AND THE PRODUCTS OF THE ACTION OF NITRIC ACID ON THIS SUBSTANCE.

BY MM. MALAGUTI AND SARZEAU.

In examining one of tbose calculous concretions known under the name of Eastern bezoars, we found, as did Mr. Hartmann, that it was almost entirely composed of lithofellic acid—the substance recently discovered by Dr. Göbel, and to which Wöbler has attracted the attention of cbemists through the opinions which be bas published concerning amorphism.

Being in possession of a very considerable quantity of this substance, we have undertaken a lengthened investigation, and we have already recognised that tbe yellow acid matter produced by the action of nitric acid differs from lithofellic acid, $C^{40}\,H^{72}\,O^{8}$, from wbich it is derived, by 8 atoms of hydrogen minus and 6 atoms of oxygen and 2 of bypoazotide plus, $C^{40}\,H^{56}\,O^{14}\,(NO^{4})^{2}$ = nitro-lithofellic acid.

We bave also ascertained that the acid product of the dry dlstillation of lithofellic acid represents the composition of that acid, minus tbe elements of 2 equivalents of water, $C^{40}\,H^{68}\,O^{6}$ = pyrolitbofellic acid.

We could also speak of new transformations of these new products under tbe action of the alkalis and tbe halogen bodies; but we defer tbis until we bave formed some precise idea as to tbeir chemical nature, and until we have completed the examination of the three series of salts arising from the primitive acid and tbe two acids derived from it.

A NEW INSTANCE OF THE FORMATION OF THE PERMANGANIC ACID AND OF THE PEROXIDE OF COBALT.

BY REUBEN PHILLIPS.

Having had occasion, during some experiments, to add the native peroxide of manganese to a solution of potassa into which chlorine had previously been passed, I observed, at the end of some hours, that tbe solution had assumed the magnificent tint of a solution of the permanganate of potassa. I also added a solution of the chloride of cobalt to the above chlorous compound, and observed the instantaneous precipitation of the peroxide of cobalt; in this instance none of tbe precipitated oxide gave any evidences of solution and of the formation of a cobaltate of potassa analogous to the ferrate.

REVIEWS:—

TRAITÉ DE CHIMIE PATHOLOGIQUE: *Ou Recherches Chimiques sur les Solides et les Liquides du Corps Humain, dans leur Rapports avec la Physiologie et la Pathologie.* Par S. D. L'Heritier, Docteur en Médecine, &c. &c. Paris: J. B. Bailière. London: H. Bailière, 219, Regent Street. pp. 744. 1842.

Varied, indeed, are the applications of which the science of chemistry is susceptible. The ingenuity of man has turned this wonderful science to account in most of the important arts of life. It is indispensable to manufactures and pharmacy, and is becoming a most valuable adjunct to medicine. Its relations to pbysiology and pathology have acquired vast importance from the discoveries of tbe illustrious Liebig, and from the ingenious tbeories which he has based on tbem. The application of cbemistry to the elucidation of tbe beretofore bidden mysteries of physiology and pathology is perhaps the highest object to which the chemical philosopher can devote his efforts.

Liebig's work bas, among otber merits, peculiarly tbat of calling attention to tbe subject; it opens a new path,—one, indced, in which many will follow, albeit as glcaners in tbe field, collecting facts which may have escaped the notice of their leader, or which, as not subserving bis immediate purpose, he may have passed over unhecdcd for the moment, tbougb not unobserved, but destined for future study.

We have been led almost unconsciously into these remarks, from tbe nature of the work before us. Tbe subject, indeed, is one which has for us many charms, and one whicb we dilate on witb pleasure and dismiss with regret.

M. L'Héritier's work is one of no ordinary interest. It is well arranged, concise, explicit, and full; it contains all that was previously known, and much original observation; the analyses are well described, and seem to have been carefully performed; in sbort, it is an excellent book.

This work is divided into eleven chapters. Chap. I. treats of Chyle, Lymph, and the Blood: II. Of the Products of Secretion which assist in Digestion: III. Of Urine and the Kidneys: IV. Of the Cellular and Adipose Tissues, of Serosity, and of Fat: V. Of the Serous Membranes and their Serosity: VI. Of the Mucous Membranes and their Secretions: VII. Of the Nervous System (Brain, Nerves and Spine): VIII. Of the Organs of External Senses: IX. Of the Organs of Generation: X. Of the Locomotive Apparatus: XI. Of Morbid Products.

In looking for extractable matter, we find so much that is of interest that we are puzzled to make a selection; we have, however, alighted on the following, which we translate for the benefit of the scientific reader.

"NON-EXISTENCE OF FREE SODA IN THE BLOOD."

"One of the arguments on which most reliance is placed in denying the possibility of the existence of free carbonic acid in the blood, is that of Dr. John Davy. That chemist maintains that blood contains an alkali likewise free, and that, consequently, an alkali and an acid cannot exist together and at liberty in the same fluid. But this is more apparent than real, and, notwithstanding its plausibility, it is easy to prove that it is based on an error.

"'All those who have examined the blood,' says Davy, 'agree in stating that it contains a considerable proportion of free alkali; this fact once proved, is it not contrary to all the rules of chemical science to believe in the presence of carbonic acid free and circulating with the blood?' M. Denis concurs in the opinion of the English chemist. 'If the blood contains carbonic acid, its soda,' says he, 'would be more or less carbonated; now, it is positively under the caustic form that it exists in this fluid.'

"Nothing is less proved, whatever Davy and Denis may say, than the existence of free soda in the blood in circulation. I do not deny that a free alkali may be obtained from the serum, when it has undergone the action of a strong heat; but does that prove that it existed in it before the analysis? I know very well that serum, especially after it has remained some time exposed to the air, gives an alkaline reaction with test paper, but I do not think that this effect is produced by a free alkali; it results rather from the action of the alkaline carbonates which constitute a considerable part of the saline matters contained in the blood.*

"The carbonates, as well as the free alkalis, change the color of litmus paper; and if carbonic acid cannot remain free in a liquid with an alkali, nothing prevents it from preserving that state in a liquid containing alkaline carbonates.

"Although the alkalis and the carbonates have the same action on litmus paper, they differ from each other not only in many properties which do not belong to each of them, but likewise in the effects which they produce on the blood; for the carbonates give a fine arterial color to venous blood, while the alkalis powerfully blacken blood.

"In support of these facts, we will quote a familiar example:—

"The saline water of some countries, that of Saratoga, among others, contains, like the blood, a large proportion of chloride of sodium and alkaline carbonates, without the least atom of free alkali. This water, on coming out of the spring, is supersaturated with free carbonic acid; but, notwithstanding this excess of acid and carbonates, it neither reddens litmus paper nor turns turmeric paper brown; when left for some time exposed to the air, or when slightly heated, the acid is disengaged, and it then exhibits a powerfully alkaline reaction on the employment of test papers.

"If free carbonic acid did not exist in the blood, and if serum owed its property of colouring test paper to the presence of a free alkali, it should possess this property as well when it begins to separate from the crassamentum as when it has undergone the action of the air. This is not the case; for recent serum very rarely changes the color of litmus paper; it is only after being left for a short time exposed to the air that the carbonic acid is disengaged, and that the carbonates give a decided test with this paper."

We conclude our notice of this work by recommending it to all interested in the great subjects of which it treats; it will amply repay the labor of an attentive study.

* All the experiments which we have made on blood prove the existence of a carbonated alkali in that fluid. We have tested blood just drawn and quite warm, and have found it to give a faint alkaline reaction.—EDS.

THE SECOND SUPPLEMENT; *Completing the Seventh Edition of Dr. Turner's Chemistry.* By JUSTUS LIEBIG, M.D., Professor of Chemistry in the University of Giessen, and WILLIAM GREGORY, M.D., Professor of Chemistry, King's College, Aberdeen. London: Taylor and Walton, Upper Gower Street.

IT is with great pleasure that we have to notice the completion of this most excellent work. We have been, we candidly confess, anxiously awaiting its publication, as, we doubt not, have many of our readers. The delay which has occurred, we are informed in the preface, has been caused by the preparation and publication of Professor Liebig's Animal Chemistry.

Could the late much lamented and talented Dr. Turner rise from his grave and behold the valuable, interesting, and important improvements and additions which have been made by Professor Liebig in that portion of the

work which is devoted to organic and exorganic chemistry, what feelings of delight would animate his bosom! We take upon us to aver that Liebig cannot better hand down his name to posterity in England, than in conjunction with that of Turner.—But we must not forget the British editor, Dr. Gregory: laborious, indeed, has been the task he has had to perform; and the manner in which he has executed it in this Supplement is deserving of much praise. We are not of that number who think that blame attaches to Dr. Gregory for having condensed as much as practicable the accounts of the various substances of which the work before us treats, so long as he has made no omissions;—had he not done so, the book would have required a "porter's knot" for its transmission from place to place;—besides, most of the practical details connected with the various processes of preparation have been published in *this* Journal in an unmutilated form, which would be impracticable in such a book as the one under consideration. He has done right in reducing the size as much as possible.

We congratulate the Editors on the completion of their labors, in which, doubtless, they have been encouraged by the opinions of the press and of the public, with regard to the former portions. To give our readers an analysis or merely a brief outline of the contents of this work would take up too much space; but we may state that, among other things, it contains accounts of the various products of the animal and vegetable kingdoms, with a good chapter on the blood and on the animal secretions, with the latest information on every subject.

Part IV. treats of "Analytical Chemistry," for which Dr. Gregory states that he is indebted to Mr. E. A. Parnell. At the end of the book there are several tables taken from various works, which will be highly serviceable to many.

We are unwilling hastily to leave a work which we have so long desired to see completed; still less are we willing to discharge it with merely a word of praise: we therefore turn to page 1175, and quote the following:—

"When wood is decomposed by the action of water in the absence of air, the process is properly termed mouldering, to distinguish it from eremacausis or decay, in which oxygen is the active agent. In mouldering, oxygen is not, it is true, entirely excluded; but its access is limited, and the results are different. The elements of water, along with some oxygen, are taken up, and carbonic acid escapes. Thus, when oak wood was decomposed by lying under water, a white mouldered matter was formed, containing $C_{33}\ H_{29}\ O_{34}$. This is derived from oak wood, $C_{36}\ H_{22}\ O_{22}$, by $5\ H\ O + O_3$, and the substraction of $3\ C\ O_2$. Mouldered beech yielded $C_{33}\ H_{25}\ O_{24}$, which may be accounted for in a different way.

"Wood coal, or brown coal, has been produced by a similar action. A pure specimen of wood coal from Laubach, contained $C_{33}\ H_{21}\ O_{16}$; that is, a greater proportion of carbon and hydrogen, and a smaller proportion of oxygen, than wood.

"If wood were to lose 3 atoms of carbonic acid and 1 atom of hydrogen, such a wood coal would be the result; for if from wood, $= C_{36}\ H_{22}\ O_{22}$, we take 1 atom of hydrogen, and 1 atom of carbonic acid, $= C_3\ H\ O_6$, there will be left wood coal, $= C_{33}\ H_{21}\ O_{16}$.

"Another specimen, from Ringkühl, contained $O_{33}\ H_{15}\ O_9$, and differs from wood, by 4 atoms carbonic acid, 5 atoms water, and 2 atoms hydrogen.

"A specimen of wood, which had been long kept in the boiler of a steam engine, acquired the appearance of wood coal, and had the same composition. In the formation of wood coal, the essential change seems to be the separation of carbonic acid from its elements, while a portion of hydrogen is removed by oxidation, owing to the limited access of air. The separation of carbonic acid seems still to go on even in the deepest beds of brown coal, and is the source of the acidulous springs found near such beds, and also of choke damp in mines. When near the surface, the proportion of wood coal is always less owing to the action of the air, by the oxygen of which the hydrogen is removed.

"Mineral coal appears to be produced by a long continued decomposition of wood or wood coal, by which carbonic acid, water, and carburetted hydrogen are separated. Splint coal, and canal-coal, according to Richardson and Regnault, are both $C_{24}\ H_{13}\ O$, which may be thus derived from wood:—

3 atoms carburetted hydrogen .	$C_3\ H\ C$	$C_{36}\ H_{22}\ O_{22}$ = wood.
3 atoms water	$H_3\ O_3$	
9 atoms carbonic acid . . .	$C_9\ O_{10}$	$C_{12}\ H_9\ O_{21}$
Mineral coal	$C_{24}\ H_{13}\ O$	

"Caking coal is $C_{20}\ H_9\ O$, or canal-coal minus olefiant gas, $C_{14}\ H_{14}$ (Liebig).

"This explains the occurrence of fire damp, or carburetted hydrogen, in coal mines: whereas in mines of wood coal, carbonic acid, or choke damp, alone occurs. The occurrence of fire damp proves that changes are constantly occurring in the beds of coal. When the whole of the hydrogen is removed in the form of carburetted hydrogen, the residue must be anthracite, which is nearly pure carbon.

"Besides carbonic acid and light carburetted hydrogen, fire damp, according to Bischoff, contains olefiant gas and hydrogen; the latter probably derived from the atmospheric air, although most specimens of coal contain nitrogen originally derived from the juices of the wood.

"Some varieties of coal contain oily and resinous matters; and in some cases an oil, like oil of turpentine, may be obtained by distilling the coal with water (Reichenbach.) It is possible that these oils and naphtha may be formed out of the elements of the carbonic acid and carburetted hydrogen separated from the wood during its conversion into coal; but if the wood had been originally pines, then the resin and oil of turpentine may have been originally present in it.

"On the whole, the production of wood coal and mineral coal may be considered, not as a case of eremacausis, but as a process to which oxygen is not essential, and in which the elements present group themselves into new arrangements, in a manner somewhat analogous to what occurs in fermentation.

"The brown acids discovered by Berzelius in certain mineral waters, and called by him *Crenic* and *Apocrenic* acids, appear to be modifications of humus, and are no doubt derived from the decay of vegetable matter."

We add the following on the

"BRAIN AND NERVOUS MATTER."

"*Nervous matter* is distinct from all other animal tissues, and is produced by the animal system exclusively. In composition it is intermediate between fat and the compounds of proteine; containing nitrogen, which is absent in fats, but in far smaller quantity than proteine does; and being, on the other hand, much richer in carbon than proteine, or its compounds. It appears likewise to contain phosphorus as an essential ingredient.

"From the recent researches of Frémy, brain appears to contain a peculiar acid, analogous to the fatty acids, which he calls cerebric acid, and which contains nitrogen and phosphorus; this is mixed with an albuminous substance, with an oily acid—the *oleophosphoric acid*, with *cholesterine*, and finally with small quantities of oleine and margarine, and of oleic and margaric acids.

"The two acids, peculiar to the brain and nervous matter, occur sometimes free, but generally combined with soda or with phosphate of lime.

"*Cerebric acid* is extracted by ether from the brain after it has been exposed to the action of boiling alcohol, which coagulates the albumen. The matter deposited on cooling by the ether is a mixture of cerebric acid, generally combined with soda or bone-earth, oleophosphate of soda and a little albumen.

"This mixture is acted on by alcohol, acidulated with sulphuric acid, which precipitates sulphates of lime and soda, and albumen. The filtered solution contains cerebric and oleophosphoric acids; cold ether removes the latter, and the former is purified by solution in hot ether and crystallisation.

"When pure, it is white, crystalline, and pulverisable. In hot water it swells up like starch, but does not dissolve. It contains phosphorus, but no sulphur if purified from albumen; the phosphorus amounts to barely 1 per cent.: and it contains 2·3 per cent. of nitrogen. It has the character of a fatty acid, but its acid properties are feebly marked.

"*Oleophosphoric acid.*—This acid has not yet been obtained quite pure. With the alkalis it forms soaps, and its compound with soda appears to exist in the brain. When it is long boiled with water or alcohol, it is resolved into oleine and phosphoric acid. This change is accelerated by acids, but it takes place also spontaneously at the ordinary temperature, only more slowly; and the presence of animal matter in a state of decomposition seems to cause it to be resolved into oleine and phosphoric acid. Thus, when brain has been allowed to undergo partial putrefaction, it no longer yields oleophosphoric acid, but oleine and phosphoric acid. It contains 2 per cent. of phosphorus. The oleine of this acid is identical with that of human fat.

"*Cholesterine.*—This fat, as extracted from the brain, in which it occurs in considerable quantity, has the same composition and properties as the fat of biliary calculi. (Couerbe; Frémy.) Frémy has also succeeded in detecting in the liver traces of the characteristic fat acids of the brain.

"The grey portions of the brain appear to be chiefly albuminous; while the white portions consist of an albuminous tissue, similar to the grey, but loaded with the fats above described.

"The softening of the brain in diseases of that organ seems to be the result of putrefaction, and is accompanied by the separation of the oleine from the phosphoric acid. The oleine itself also is decomposed, yielding free oleic acid.

"There can be no doubt that the brain and nervous matter (which is quite similar to brain) are formed in the body from compounds of proteine, either by the loss of some azotised compounds, or by the addition of highly carbonised products, such as fat. But we are ignorant in what part of the body, or by what organs, nervous matter is prepared. This point requires minute investigation. In the meantime, according to Chevreul, the fatty matters, which occur in small quantity in the blood, are similar to those of the brain."

We recommend, nay, we seriously advise, the medical student, the medical practitioner, the manufacturer, the chemist and druggist, the student of chemical philosophy, the agriculturist, in a word, every one whose profession, trade, or art, has any connection with the science of Chemistry, to obtain and attentively peruse this valuable portion of the work.

As a book of reference it has many claims; on organic and oxorganic Chemistry, the work, taken as a whole, is a model. The Editors have conferred a boon on the British public, for which, from what we know of that public, having labored for it for some years, they will not go unrewarded.

LECTURES ON ELECTRICITY; *delivered in the Royal Victoria Gallery, Manchester, during the Session of* 1841-42. By WILLIAM STURGEON. London: Sherwood, Gilbert and Piper. 1842. pp. 240.

THE science of electricity is now becoming so popular a study, that a knowledge of it may be regarded and considered as an essential feature in the education of every one who wishes to be an agreeable and profitable member of society.

We look forward with pleasure to that period when the sciences will be universally included among the studies of youth; when those valuable years which are lost in scraping up miserable smatterings of classical lore will be more usefully devoted;* not that we do not regard Latin and Greek as necessary qualifications for every gentleman; but we would commence education with that which is instructive and amusing, and by this means create a love of learning, when the scholar should take to the dryer studies; and we are convinced that by this plan no time would be lost; for it is well known that a boy who is fond of learning, will, at the age of fourteen or fifteen years, acquire as much in a few months as it would take another at an earlier, or even the same age, who has had tuition made irksome to him, years to attain. Love of learning can only be created by making the early studies of youth *agreeable*.

With these views we recommend the work before us to the proprietors of schools, and parents who agree with us in our remarks: it is well adapted for the purposes for which we recommend it.

We have room for the following from the commencement:—

"In introducing these lectures to the members of the Royal Victoria Gallery, as they are intended for instruction rather than display, it will be necessary to avoid, as much as possible, all those phrases and technicalities which not only puzzle, but absolutely mislead, even those who have, in their own estimation, much higher pretensions to a knowledge of such fashionable appendages to scientific literature, than the persons for whose instruction these lectures are intended; and to whom, therefore, I shall address myself with freedom, and in the plainest language that the present state of these subjects appears to me to be capable of admitting. I do not, however. wish to enter into any engagement that would limit my labors to the humble task of a mere detail and exhibition of facts, without linking them together in some theoretical system or systems of physical laws; because one of my objects is to trace to the same operations of nature, those facts, and those only, which are easily and not otherwise explained, by that code of laws which governs the display of one peculiar class of phenomena: and not to encumber any theoretical system with those phenomena to which they do not appear to belong; but to explain each class of phenomena by its own peculiar code of laws; or, if you please, by its own peculiar theory. Hence it is, that I shall be expected to be explicit on every point on which I touch, both experimental and theoretical, and either undertake to explain all those experimental facts which I may consider necessary to bring forward, or candidly acknowledge that they are inexplicable upon the theoretical principles which I advance.

"It will here be necessary to enter into

* We may mention that it is rapidly becoming the practice in large scholastic establishments to teach the sciences as well as the ordinary branches of a liberal education, and cannot too highly applaud those who are introducing so signal an improvement.

certain conditions with my readers respecting some of those theoretical points, which, to many philosophers, even of the present day, appear to be somewhat doubtful; though I believe the opinions of many others are favourable to those theoretical views by which I propose to be guided.

"I wish to be understood, then, before I proceed any farther, that besides those recognised portions of matter which appear to be the principal part of the materials which constitute the earth and its atmosphere, such as the various kinds of solids and fluids which usually receive these general appellations, there are, at least, three others, whose reciprocal actions on each other, and whose peculiar operations on the former classes of bodies, are productive of the most surprising, and, in our present state of physical knowledge, the most interesting phenomena that nature has revealed to man. These are the *electric* matter; — the *magnetic* matter;—and the *calorific* matter;—each of which I shall consider as a distinct element, possessing peculiarities of force and modes of action, and exhibiting phenomena which no other kind of matter has the power of displaying. They, however, operate on one another in a very remarkable manner, by their peculiar reciprocal excitations, and are thus productive of phenomena which have led some philosophers to the belief of their complete identity.

"The fineness and subtilty of the *electric*, the *magnetic*, and the *calorific* particles, lead us to infer that they insinuate themselves into the pores of all other kinds of terrestrial matter; and their inactivity, when unmolested in these their natural habitations, is obviously a consequence of the equilibrium of their respective forces when in an undisturbed state. So long, therefore, as these natural equilibriums remain unmolested, all of these material agents are perfectly inactive,* and exhibit no phenomena whatever. Hence it is, that some exciting process becomes absolutely necessary before any of their respective phenomena can be produced. The processes of excitation which may be employed for bringing these agencies into a state of activity are exceedingly various, as I shall have occasion to show in many parts of these lectures; but, for the present, it will be sufficient that I describe one simple mode only, of exciting each individual agent, by means of which certain phenomena, of each class, may very easily be brought into existence."

From a careful consideration of this book, we freely state that it is just the one that we would put into the hands of youth; it has been arranged with much care and industry, and in it the author evinces a thorough knowledge of the subject on which he writes. Every one desirous of becoming acquainted with the science of electricity should possess himself of a copy, and study it with attention.

* I am aware that, to the mathematician, the term *inactive* may appear to convey a doubtful meaning; because forces holding one another *in equilibrio* do so by means of their *activity*, though perfectly *inactive* on other forces, and on surrounding matter. It is the *inactivity* on surrounding matter that is here to be understood.

LONDON ELECTRICAL SOCIETY.

At the meeting in November, the Secretary, Mr. Walker, read the conclusion of his translation of M. Becquerel's paper "On the Electro-Chemical properties of Gold," containing some interesting applications of theoretical deductions to actual practice. The author proposes to solve the following question: Any solution whatever, whether acid or alkaline, being given, containing gold and *other* metals, to extract the gold in a great state of purity. He takes, for instance, a solution of gold and copper in aqua regia: he then makes a solution of the same specific gravity of copper alone in aqua regia. He uses these two in the liquids for a single cell diaphragm apparatus, and places a plate of copper in the single solution in connection with a plate of platinum in the other: in due course all the gold, and the gold alone, is extracted; for the current thus generated is just enough to decompose the chloride of gold, and that alone. He pursues similar methods with solutions of several metals, extracting the higher metals first. He applies similar principles to electro-gilding with the single cell, so as to obtain the mat-gold so much required in the arts. He takes a dilute solution of double cyanuret of gold and potassium, and a solution of yellow ferro-cyanuret of potassium containing a little common salt, and with this excites a single pair, consisting of zinc and the object to be gilded. He seems to prefer for gilding the single cell to the battery process, and adopts this method in order to obtain that firm and characteristic deposit, without which electro gilding is of little avail.

II. CHEMICAL MANUFACTURES.

ON THE MANUFACTURE OF THE CHROMATE OF POTASSA, AND ITS ADULTERATION.

BY CHARLES WATT, JUN.

THE extensive use which has lately been made of the chromates in the arts, and the still more extended application of which, doubtless, they are capable, must render any improvement in their manufacture that will reduce the expense, of much importance and value.

Previous to going into this subject in detail, I cannot help remarking, that it is a great pity that our large manufacturers do not more energetically exert their endeavors to improve their various processes of manufacture, and that they do not to a much greater extent cultivate and encourage this important branch of Chemistry; if they did, they would reap considerable benefit. There are some, to their credit be it spoken, who do employ their intellect for this purpose, and I doubt not that they derive great advantage from so doing; for there are but few chemical arts which do not admit of improvement of a very extensive nature.

These *practical* men, if they would but pay a little attention to the *theoretical*, combining, as they would then do, the two essentials, would, I feel confident, be able to effect many important and, to themselves and the public generally, valuable improvements.

The chromate of potassa is the salt from which the other chromates are prepared; it is manufactured in the following manner:—The substance called chromate of iron,—an ore, composed of the oxides of chromium and iron,—is heated to a strong red, with a sufficient quantity of nitrate of potassa to oxidise the oxide of chromium, and convert it into chromic acid, and to yield a base for that acid, when formed, to combine with.

In order that too much nitrate of potassa may not be employed, about one fifth of the weight of the ore is added first; the chromate formed from this is then removed, and a fresh portion of nitre used, and so on, until all the oxide of chromium is further oxidised and converted into chromate of potassa.

Why nitrate of *potassa* has been so long employed in this manufacture, I am at a loss to discover; for it must be obvious that chromate of soda would answer all the purposes of chromate of potassa, the base being of little consequence, so long as it forms a soluble salt with the chromic acid, as it is merely useful as a vehicle for the chromic acid. Why not make the chromate of soda? There would be many advantages attendant on doing so; one of which would be less cost for carriage, as 84 parts of the chromate of soda contain as much chromic acid as 100 parts of chromate of potassa; and, besides, the salts of soda, from which the chromate may be formed, are cheaper than those of potassa. Having been long practically acquainted with this manufacture, I can freely state that I know of not the least objection to the use of soda. The bichromate of soda is as readily formed as that salt of potassa. All the required changes take place quite as readily when the chromate of soda is formed as in the formation of chromate of potassa; and it would be no difficult matter to prove to the purchaser, even if it were necessary to charge a rather higher price for the chromate of soda, that he would be no loser, as it contains more of the constituent for which he purchases it,—the chromic acid.

The composition of chromic acid is $Cr\,O^3$, that of the oxide of chromium, $Cr^2\,O^3$. To convert the latter into an alkaline chromate, two equivalents of an alkaline nitrate are required, *i. e.* the alkali contained by the 2 equivalents; the nitric acid of these 2 equivalents of nitrate contains 10 equivalents of oxygen, whereas the oxide of chromium requires only 3 equivalents of oxygen to convert it into chromic acid, consequently 7 equivalents of oxygen are by this process wasted.

In some experiments which I tried some time since, I found that carbonate of potassa or soda might be in a great measure employed instead of the nitrate. I used a sufficient quantity of the nitrate to supply oxygen, the alkaline carbonate being merely employed to supply a base for the chromic acid. I used for every half equivalent of oxide of chromium one third of an equivalent of nitrate of potassa or soda, and two thirds of an equivalent of carbonate of potassa or soda, from which was obtained about an equivalent of the chromate. I mixed the carbonate and nitrate intimately before using them, and found by a little care that but very slight preponderance of alkali remained when the operation was completed.

I have now to speak of the adulteration of the chromate of potassa. I have lately seen several samples of this article highly discreditable to the parties who send it out. I do not charge the manufacturers with the adulteration; I believe the parties who are guilty of it are those who purchase it for the sake of selling it again. A short time ago I

was supplied with a sample which was nothing but sulphate of soda and chloride of sodium colored with a strong solution of the chromate, and which caused a white precipitate in any of the soluble salts of lead. For the benefit of the purchaser, I subjoin the following method of examining the chromates of potassa and soda.

First ascertain the quantity of moisture contained in the sample, by weighing out a certain portion, drying it on a sand-bath, and again weighing; the loss of weight will give the quantity of water: then dissolve it in distilled water, and add any soluble salt of lead until it ceases to give a precipitate. The mass is then to be boiled, and more distilled water added; the supernatant liquor is then to be poured off, and if the sample under examination contain any chloride of sodium, small shining crystalline needles of chloride of lead will form in the liquor as it cools. The remaining precipitate is then to be treated with strong nitric acid, which will decompose the chromate; by adding distilled water the nitrate of lead formed by the decomposition of the chromate of lead will be dissolved, and the remaining sulphate of lead, if any, may be dried, and its amount ascertained, from which the quantity of sulphate in the chromate may be calculated.

If it be required to ascertain the quantity of chloride, this may be done by redissolving the chloride of lead by means of heat, and operating on it by any of the soluble salts of silver.

In conclusion, I may add, that I hope these few remarks will prove not unworthy of the consideration of the manufacturer, although they are but the commencement of a subject which I hope shortly to take up again.

ON THE ARSENIC IN SULPHURIC ACID, WITH A MODE OF EXTRACTING IT.

BY REUBEN PHILLIPS.

Large quantities of sulphuric acid are made from iron pyrites: acid so manufactured contains an enormous quantity of arsenic. This arsenic is injurious to the public in many ways: it not only affects those into whose immediate hands the acid goes; the virulent poison finds a vehicle of introduction in almost every article which undergoes a manufacturing process. In how many processes is the sulphuric acid directly employed? and in how many of those processes is it *impossible* that the arsenic can exercise an injurious influence?

Mr. Charles Watt, jun., has shown, that soda manufactured with an arsenical acid contains an abundance of arsenic, the injurious tendency of which he has pointed out. Probably, the manufacturers supposed that the high heat used for effecting the decomposition of the sulphate was a sufficient guarantee that no arsenic could be contained in the resulting soda.

Many other such cases could be mentioned. Sulphuric acid is frequently employed by the electro-metallurgist, and as a source of hydrogen; in either case, when the acid contains arsenic, the hydrogen becomes arseniuretted, and that which should be innocuous becomes converted into an insidious poison capable of producing the most frightful effects. Cases of death from this cause are on record; by it, tyros in chemistry are liable to become victims to their own experiments; and should the new method of soldering by hydrogen be generally adopted, who shall say how often such cases may occur, unless the evil be removed?

To effect this, I proposed the following cheap and easy method; and what say the manufacturers? they say this—that the arsenical compound comes recommended to them by peculiar advantages, for the arsenious acid increases the density of their sulphuric acid, and they can get as much per lb. for the noxious as for the pure; therefore the lightest expense is an unnecessary burthen, and the removal of the arsenic would be a depreciation of the value of their acid. Besides, they say that they know where their acid goes, and where, and how it is consumed; but, can they tell where the products of that acid go, and where the arsenic becomes finally lodged?

PROCESS.

Sulphate of baryta is intimately mixed with charcoal, placed in an iron retort and maintained at a strong red heat for some hours, the time being dependent on the thickness of the mass. By this treatment the sulphate of baryta becomes converted into the sulphuret of barium. The whole must then be allowed to cool, when the sulphuret is removed and powdered.

The acid as it flows from the chambers is received into a vessel in which there is added to it a quantity of the sulphuret of barium sufficient to take down the arsenic in the form of a sulphuret, the proportion being somewhere about 84·8 sulphuret of barium to 99·4 of the arsenious acid, the exact quantity being determined by previous trial on a small scale. The insoluble substances produced are then allowed to subside; the principal part of the clear fluid is drawn off, and the remainder passed through a sand filter; the acid then receives its final concentration.

NEW MODE OF PREPARING LIQUID RENNET.*

BY J. WISLIN.

SOME years ago, I published a formula for liquid rennet, such as was in use for preparing it among the *pharmaciens* of this country; it succeeded well and possessed all the properties which characterise recent rennet, but it has the inconvenience of not keeping long without alteration, especially when the bottles were in use and being gradually emptied. To remedy this inconvenience, I propose the following mode of preparation, which several years' success warrants me in recommending.

Stomachs of calves (the whole)	10 parts.
Chloride of sodium . .	3
Alcohol at 31° Cartier . .	1
Water . . .	16

The membrane of the stomach is cut with scissors, and malaxated with the salt and the rennet which is found in the interior of that organ; the whole is put into an earthen or stoneware pot, covered with paper, and set aside in a cool place. This mixture is left in contact sufficiently long for the disagreeable cheesey odor which it had previously to be replaced by the proper smell of rennet; the time required for this reaction varies from one to two months, according to the temperature; at this period the quantity of water directed is added, and then the alcohol. It is now filtered, and colored with caramel.

If a considerable quantity has to be prepared, the mixture may be placed in a cask with the head knocked out; and it can be made only as fast as the stomachs can be procured, and in proportion to their quantity. Thus salted, this rennet, which may be kept for any length of time, can be used for the preparation of liquid rennet.

DESCRIPTION OF A NEW ELECTRO-METALLURGICAL OPERATION FOR PRODUCING PLATES FROM IMPRESSIONS OF ENGRAVINGS, &c.

BY M. DAMBERGER.

THE method I have adopted for producing plates from printed copies is the following, of my own devising:—First procure a recently printed impression of the engraving, and press very tightly on a clean plate of polished copper, by which operation the greatest part of the printing ink is transferred from the paper to the metal. Very fine filings of copper, or, what is still better, the powder of that metal precipitated from any of its salts by means of a plate of iron or zinc, must now be evenly sprinkled on the plate, and then brushed lightly over with a fine soft brush: the powder attaches itself to the inked, but not to the other parts of the plate, provided the process has been conducted properly, and by that means raises the picture *above* the general (flat) surface.

The plate may be now considered as a mould or matrix, and is intended to receive a deposit of voltaïc copper, for which object it is placed in a decomposition cell by the side of a similar sized plate of sheet copper of one eighth of an inch in thickness. When these are connected by means of wires with the elements of a Daniel's battery, the acidulous solution of sulphate of copper which fills the cell is immediately decomposed; and the copper affixes itself to the matrix, the opposite plate being, at the same time, proportionally corroded. It scarcely needs to be added that the mould is to be attached to the wire that comes from the zinc element of the voltaïc battery.

This method furnishes a ready means of multiplying and copying engravings, with great facility, and is particularly applicable to the production of the common varieties of prints. The whole of the impressions from plates thus formed are reversed in respect to right and left, but the shades, &c. remain as in the originals. Drawings might be by a similar manner transformed into plates capable of having impressions taken therefrom. This can be accomplished by painting with an oleaginous mixture, either on the plate or on paper, (the latter requiring, of course, to be transferred,) and then sprinkling the plate with a metallic powder in the manner before described. If the painting be executed on the plate, oil varnish may be used; it should be laid on very thin and even, beginning with the lightest shades, and going on progressively to the darkest ones: it will also be an improvement, in this case, to use fine copper filings, carefully sifted through sieves of various degrees of mesh, the finest kind being required for the lightest tints, while the roughest are for the darkest and boldest touches of the picture: by manipulating in this manner a very superior effect will result.

If the impression of which it is desired to have a copperplate, should not have been quite recently printed, but perhaps done a few months before, the best way is first to thoroughly cleanse the matrix-plate with dilute nitric acid, and when dry, to transfer thereto the print, which should be previously exposed to the vapor of heated oil of turpentine, or a mixture of ether and oil of lavender, in order to soften the ink; it must now be sponged over with gum-water and a lithographic roller

* *Journal de Chimie Médicale*, Sept. 1842.

passed over it, so as to cause the lines of the picture to take up an additional quantity of ink: when the gum has been washed off and the plate dried, the application of the copper powder will produce the desired effect.

Although I have mentioned that the decomposition cell and voltaïc battery are to be employed, yet if such things be not at hand, the single cell apparatus will answer very nearly as well; but I must say that I would give a preference to the former arrangement, as the action goes on in a far more regular manner, the deposit must consequently be much firmer, and likewise the energy of the action can be much more easily and correctly regulated.

PRECIPITATION OF COPPER FROM ITS SULPHATE, ON IRON AND ZINC BY GALVANIC AGENCY.

BY F. ELPHINSTONE.

It is well known that this solution of the metal does not answer at all for depositing copper on either tin, ziuc, or iron, as these metals are themselves acted on by the solution. My method is to first precipitate a thin film of copper on the article from the solution of its cyanide in ferrocyanate of potassa, and then to deposit on it the remainder from the common solution of the sulphate. It has occurred to me that some of the more deliquescent salts of copper, as the nitrate, muriate, &c., might be employed to quicken the time required for effecting a thick deposit, as the solution could be more concentrated than that of the sulphate. I have found that the hot solutions of the latter always produce a copy of the object in a far shorter time than those of the same cold, and in my opinion, on account of the greater strength of the cupreous solution. As I have not at present time to prosecute experiments of the kind, probably some one else may consider it worth a trial. I am very confident of its succeeding, and shall be very glad to hear the result.

RAPID METHOD OF ETCHING.

BY MR. A. J. COOLEY.

A most rapid method of etching on iron or steel, capable of very general application, is as follows:—

Warm the metal until it is capable of melting a piece of bees'-wax,* which must then be carefully rubbed over it, so as to form a thin and even coating; allow the whole to cool, and scratch out the design in the common way, with a needle or point; then sprinkle on a little powdered iodine, and at the same time add a few drops of water, with a camel-hair pencil, and work them into a *liquid paste*, which must be moved about over the intended engraving, for a period varying from one to five minutes, according to the depth of lines required to be produced. Afterwards wash the whole in clean water. Persons acquainted with the properties of iodine will readily perceive that the same etching-paste, by being kept for a few days, will again acquire the property of dissolving iron. I have thus successfully employed the same materials three or four times. Iodine will, doubtless at no very distant period, supersede the use of acids for the above purpose, on account of its portability and convenience. To travellers and amateurs who amuse themselves with the delightful art of etching, it will, I think, prove invaluable.

* Etching varnish may be employed instead of wax.

REVIEW:—

AN ESSAY ON THE NATURE AND APPLICATION OF STEAM; *with an Historical Notice of the Rise and Progressive Improvement of the Steam Engine. With Plates.* By M. A. Anderson, Civil Engineer. pp. 124. London: Sherwood, Gilbert, & Piper.

Previous to entering into the consideration of this work, we feel it in some measure necessary to apologise to our readers for bringing before their notice a book so long after its publication; bnt the vast and rapidly increasing interest of the subject of which it treats, will, we think, be sufficient excuse.

In England, a country which owes so much of the prosperity with which it has been blessed, and which, it is to be hoped, it will, ere long, again enjoy—a country which is indebted for the superiority it has acquired in its manufactures and commerce over the other nations of the earth to the discoveries of the men to which it has given birth and nurtured, and who have turned the abilities which Nature has with so free a hand bestowed on them,—the subject of which this book treats must be one which will excite in the bosom of every one who feels concern in his country's welfare, the deepest feelings of interest.

The continual increase of the application of steam in the various arts renders it necessary for every man, however menial be the duties he has to perform connected with those arts, to become acquainted with the nature and properties of steam.

The immense revolutions which this extraordinary power has produced in the modes

of travelling, both by land and by water, excite the surprise and admiration of mankind: there seems to be scarcely any thing that steam is not capable of accomplishing; and if it continues to make the rapid strides which it has done during the last half century, what feats will it not perform some few years hence?

A journey to Liverpool, which, but some five years ago, occupied about twenty-six hours, is now, by the agency of steam, performed in about nine, and might be done in even less time. It is not so very many years before that time, that a person going such a journey would have "made his will," but now it is merely considered as a "pleasant trip!!" No longer are we dependent (thanks to steam!) upon the fickle wind; despite it, by steam, the vessel is enabled to pursue its onward course, and, if we may be allowed the expression, to deride its opposition; even should it be favorable, it receives but little more respect, for the vessel outstrips it in speed. We quote the following from page 2 of the work which has given rise to these few remarks, :—

"Steam, then, may be defined the offspring arising from the union of the two elements, Fire and Water; and as fire and water are proverbially known to be 'good servants but bad masters,' so, too, we may say of their amazingly expansive and terrific offspring, *Steam*, which, when kept in due subjection, is the most useful and powerful servant of man, but when once it gets the upper hand, is the most dangerous and destructive thing known, gunpowder itself not excepted.

"Water, in the process of conversion into vapor or steam, combines with more than *five times* the quantity of caloric, or fire, that is required to bring ice-cold water to a boiling heat, and occupies a space eighteen hundred times greater than it does when in its original form of water; one cubic inch of water expanding into about a cubic foot of steam.

"From practical experience, the best of all teachers, the explosive force of steam is found to be much greater than that of gunpowder. Some volcanic eruptions and earthquakes, it is supposed, owe their terrible effects to the powerful agency of steam, by the collision of the waters of the ocean with subterraneous fires."

There are several names connected with this subject which will be mentioned with respect and admiration wherever steam is known, and science cultivated. Among these that of James Watt stands pre-eminent; respecting him, we take the following from page 32.

"Mr. James Watt was a maker of mathematical instruments in Glasgow, and was employed by the University of that city, in the year 1763, to repair a working model of a steam engine, which was used by the professor in his lectures to his pupils. During the experiments that he made with his engine, he observed the great loss of steam which was occasioned by its coming in contact with the cold surface of the cylinder. It was not, however, till two years afterwards that he brought forward the two most material of his improvements, which were, first, that of making the steam itself depress the piston instead of the pressure of the atmosphere; and, second, that of condensing the steam in a separate vessel. By the first he considerably augmented the power of the engine, because the force with which the piston descended in Newcomen's was limited to the weight of the atmosphere; but by increasing the elasticity of the steam which he used, he could make it equal to twice the atmospheric pressure,—thus making an engine of the same size raise a column of water of twice the weight. His second improvement effected a great saving of heat, because, by surrounding the cylinder with a case, it was constantly kept at nearly the same temperature as the steam,—consequently there was no loss by condensation of the acting vapor."

We have now said almost all that we have space for; but we must detain our readers while we pass a few well-deserved encomiums on the author of the valuable work before us. We consider it a most excellent treatise on the subject, and deserving of public approbation; it is written in a most agreeable and easy style; the youth may read and understand it; the adult, even if he be acquainted with the subject of which it treats, will not rise from its perusal without gaining something.

We earnestly recommend all who have the management of steam engines to read it with attention, and, as it is accompanied with plates, it will assuredly be serviceable to them.

GLYPHOGRAPHY, OR ENGRAVED DRAWING.

Mr. Palmer, of Newgate Street, has favored us with some very beautiful specimens of his patent Glyphography. In accuracy and delicacy of shading they far surpass the ordinary woodcuts, and we have no doubt that ere long they will be universally substituted for them, as, indeed, from their superiority, they ought to be. We defer further noticing them until we have availed ourselves of Mr. P.'s invitation to inspect his collection of specimens.

III. PHARMACY, &c.

ABUSES OF THE DRUG TRADE.

LETTER V.

To the Editors of The Chemist.

GENTLEMEN,—

BELIEVING it to be high time that the attention of the Council of the Pharmaceutical Society should be drawn to the depressed (I had almost written *debased*) state of the trade, of which it is the *professed* representative, and knowing no medium through which it may be more effectually attracted than that of your influential and widely circulated Journal, I beg to crave a small space in the concluding number of your present volume for the few observations I am about to make.

In the first place, I would observe, that it is a great reflection on the executive that no steps have been taken up to this period to mitigate the several abuses which formed the subject of my former communications; at the same time, what an opprobrium does it not present to the Professors of Pharmacy in this country that such evils should continue to exist. Had circumstances permitted, I might have multiplied the list of existing abuses tenfold; but pressing engagements render it necessary that I should limit my *exposé* for the present to this letter, which I beg to hand to you as the conclusion of the series on the same subject.

In illustration of the gross delinquency prevalent amongst the profession, I transcribe the following from the columns of the "Times," with no other comment (its evil tendency being immediately apparent) than the simple question—Should such deception be tolerated either by the Society or the Public?

"To Physicians, &c.—The proprietor of an old established retail business and private connexion at the West end of town, is desirous of treating with an M.D. in a manner that would prove advantageous to both parties. The *strictest confidence* may be relied upon. Address (prepaid) to C. D., Post Office, Thayer Street, Manchester Square."* I think, Gentlemen, you will agree with me, that the public should be made acquainted, through the medium of the press, with these disgraccful connivances, and that Medical Journalists, especially, should unite in the exposure of such *mal*-practices.

For evidence of the fallen state of the trade, which forms the subject of this epistle, I need not travel beyond the said column from which the above is copied, where I find the following:—"To Medical Assistants.—Wanted, in a retail shop in the country, an *Assistant;* one who would consider board and lodging an equivalent for his services would be preferred. Address free, with reference, to A. B., care of Messrs. Hewlett and Goddard, 68, Hatton Garden." So much, Gentlemen, for the services of a member of what is generally, though *erroneously*, called a lucrative and respectable profession. Gracious Heaven! have matters come to such a pass as this, that mere board and lodging are considered equivalent to the services of an Assistant!! Ay, verily, it is so; *par exemple*, I extract one other from the identical column. "Medical Assistant.—To Surgeons, Druggists, &c.—A *gentleman*, aged 26, accustomed to visiting, dispensing, &c. is desirous of obtaining a situation with a general practitioner or druggist, to the promotion of whose interest he would devote his *whole time and attention:* no salary required. Address to D. S., 42, London Street, Fitzroy Square."

I think I may tacitly pass by these instances, as they speak volumes of themselves; but surely something might be done by those in authority towards ameliorating the present degraded condition of the trade; or is it the intention of the Council to turn its attention solely to the better instruction of the rising generation? I would likewise inquire what inducement is there, both for young and old, to set about obtaining a thoroughly scientific acquaintance with the art which they profess, in the present heterogeneous and misgoverned state in which they find themselves involved—surrounded, as they are, by a low, sordid, and disgraceful competition in price. That such is the case, we have convincing proof in the advertisements with which the daily papers teem, and in the ticketing of cheap drugs which meets our view in the shop windows at every corner of the street. How frequently do we see drugs and medicamcnts ticketed at a price far below that which it would be possible to purchase even a second rate article *wholesale!* I need not encroach either upon the space allotted to correspondents, or the time of your numcrous readers, by enumerating the various articles alluded to, as, doubtless, they will at once present themselves to the minds of the latter; though I cannot refrain from mentioning one or two, such as Camomile flowers, 1½*d.* per oz.; Senna leaves, 3*d.* and 4*d.* per oz.; Irish

* "Times," of Nov. 3d, 1842, p. 1, col. 5.

moss, 6*d.* per lb.; Seidlitz powder, 8*d.* per doz.; leeches 2*d.* and 3*d.* each, *cum multis aliis*, and not unfrequently, to crown all, as the climax of Pharmacy in England, brown Windsor soap, 1*s.* per lb.; and ginger beer, 2*d.* per bottle! Against this, amongst other abuses, your pen has been at various times directed, though with less success, I fear, than the earnestness of your remarks, and the justness of your cause merited; and the failure of which (if a failure) is to be attributed more to the want of coadjutors in the good work, and to the apathy evinced by those intrusted with power to control the evil, than to any lack of force in the arguments you employed. If the Council of the Pharmaceutical Society imitated the *Pharmaciens* of France and Germany, in the construction of its School of Pharmacy, so likewise should they emulate them in the protection they afford to their disciples, by the restrictions they enforce relative to these matters. It would be well if the Council would follow the example set by the French Scientific Congress which lately assembled at Strasburgh, and institute an inquiry into the organization and present state of Pharmacy in England; with a view to taking into its immediate consideration the utility of limiting the number of shops, and the expediency of making the possessors give security; also the necessity of devising a law relative to the present unrestricted sale of poisons, and the means of attaining the suppression of secret remedies.

Before I conclude this letter, I am desirous of drawing attention to the *avidity* with which certain accidents or mistakes, said to be caused by *ignorant* druggists, are both reported and read by certain individuals, interested in calling that much belied class by such opprobrious epithets as *manslaughterers*, *murderers*, and the like; as if those immaculates who thunder forth their anathemas, were never known to commit an error or send out *Liquor Potassæ*, even in a *wine-bottle*, with nought but a *verbal direction*, which culpable negligence led to the poisoning of a young woman, the same having been taken by her in mistake for a solution of Epsom salts, which happened to be placed in the same closet. This case, which never came before the public, but which was witnessed by the writer hereof, emanated from one of our metropolitan hospitals; but as such was the case, and as the patient recovered, having been attended by those interested in suppressing it, nothing was said or known of the matter. Talk of murders hidden from the world by the absence of circumstances, indeed! In a former letter, I have alluded in strong terms to the disgraceful manner with which medicines are eked out to the out-door patients at these places.

I am, Gentlemen,
Respectfully yours,
PHARMACOPOLA.

In forming expectations as to the good which is likely to be done by the Pharmaceutical Society, we can take for our guide only that which has already been effected; and it has done no good, nor will any ever emanate from it; it is totally bad, and would have been worse, but for the check we have kept on it.

We intend shortly to lay before our readers and the public a full review of this Society and its deeds.

ADULTERATION OF DRUGS.

To the Editors of The Chemist.

GENTLEMEN,—

Your exertions in inquiring into and exposing the gross adulterations practised in the articles of consumption by the unprincipled in trade, deserve the highest encomiums; and, as you court information on this important subject, I feel assured that you will give publicity to the following hints, embracing as they do some of those things that concern every one, and of which, as yet, I have never seen a word in print, but which are, nevertheless, practised every day almost with impunity.

What I refer to is the many adulterations in drugs and chemicals, practised by the *manufacturers* and vendors thereof in the *wholesale* way. Those, in the retail trade, who have had some years' experience, will bear me out in these remarks, I feel sure, stamped as they are with the force of truth; the enumeration of all of them would occupy too much of your valuable space, but the following are a few of them, viz.—some of the Essential Oils, Lemon, Bergamot, Origanum, &c.; many of the Extracts, particularly the Ext. Coloc. Comp.—differing very widely from that of the *Pharm. Lond.* Elect. Sennæ; Oxym. Scillæ: many of the powders, such as the Pulv. Rhei; Pulv. Jalapæ; Ipecac. Glycyrrh.; Sennæ, &c.; Musk, Otto of Roses, &c. &c.

I have, Gentlemen, in my time, seen many specimens of these adulterations, emanating, too, from houses (it would be invidious to name any one in particular) deemed *highly respectable*, but, nevertheless, guilty of these dishonorable actions: the practice still exists, but not to the extent it formerly did, owing to the better knowledge

of the druggists in general, but there are still many articles sophisticated.

One thing has struck me, and that is, that the wholesale druggists, (I believe all) have joined that compound of jobbing and imposition, which you have handled so severely, and exposed so felicitously, and whose "grand journal" will, probably, on account of that circumstance, abstain from noticing anything so *innocent*,* the exposure of which would only tend to the *injury* of their patrons!

I am, Gentlemen,
Yours, respectfully,
PATRIOTICUS.

Nov. 15, 1842.

P.S.—The retail druggists having hitherto been assailed as being the only persons guilty of adulterating drugs and chemicals, it is only fair that the wholesale vendors should take their share of the public abuse as well, which is the object of this communication.

PILLS OF PROTIODIDE OF MERCURY.

BY M. RICORD.

℞ Protiodide of Mercury	30	centigs.
Lactucarium . .	30	,,
Extract of Thebaia .	6	,,
Extract of Cicuta .	60	,,

To make 6 or 12 pills.

M. Ricord sometimes gives 4 or 5 centigrammes of protiodide of mercury in one day, but it must be borne in mind that one patient may bear a quantity of it, while another cannot; it is necessary, therefore, to study the patient and the doses to be administered.

* Our Correspondent is here somewhat in error. The Pharmaceutical Journal is now engaged in exposing adulterations committed by those who are not members of the Society: this is intended, we suppose, as a means of showing them the advantage of belonging to it, thereby securing themselves in this disreputable practice. The object of the Journal is, we suppose, to induce these druggists to become members. This reminds us of the plan adopted by a paper published some years ago in London, called the *Paul Pry*, which aspersed and belied the characters of all persons the more foully and falsely as those persons were more respectable, and granted immunity to them on their paying a sum of money.

Our object in exposing adulterations, is to report frauds for the benefit of the public, and without any sordid motives.

We are on the alert; but shall not confine ourselves either to the members of the Pharmaceutical Fungus, or to those who are not.—EDS. CHEM.

CASE OF TIC-DOULOUREUX CURED BY CHLORIDE OF BARIUM.*

BY DR. LUTTEROTTI.

A WORKING man, advanced in years and of a scrofulous constitution, was affected for eight years with a prosopalgia, which, for two years, continued, night and morning, without leaving a quarter of an hour's repose; the fits were so violent that it was impossible to interrogate the patient.

The author first employed acetate of morphia by the cudernaic method, and prescribed purgatives internally, but without the least advantage. Then, on account of the strumous constitution of the patient, Dr. Lutterotti resolved to administer to him the chloride of barium, which he did according to the following formula.

℞ Chloride of barium . 12 decigr.
Distilled camomile water 125 grammes.
Mix and dissolve it.

Ten drops to be taken every 2 hours.

At the end of ten days' treatment, he was cured.

The treatment produced at first a strong heat in the stomach, inclination to vomit and colic; but next day the prosopalzic fits became more rare and weaker, until they finally disappeared.

EMMENAGOGUE MIXTURES.†

BY DR. TOTT, RIBNITZ.

℞. Roots of valerian . .	10	grammes.
Roots of artemisia . .	10	,,
Roots of black helebore .	10	,,
Leaves of Chenopodium Ambrosiale	10	,,
Tops of mugwort . .	10	,,

Cut and pound finely, then mix very accurately.

These mixtures are employed with much success by Dr. Tott, in cases of amenorrhœa which are not accompanied with inflammation or irritation of the abdominal viscera.

Dr. Tott prescribes them in infusion which is prepared with from 4 to 8 grammes of the mixture to a quart of boiling water. After standing sufficiently long in a close vessel, it is strained through a sieve, or poured off clear, then the infusion is sweetened with a sufficient quantity of sugar or an appropriate syrup, and it is taken by small cupfuls in the course of the day.

* *Oesterr. Medicinische Wochen-Schrift.*
† *Neue Zeitschrift für Geburtskunde.*

SIX CASES OF DIARRHŒA TREATED WITH MONESIA.*

BY A. GIBBON.

CASE 1ST.—MRS. K. —— MARRIED, had labored under chronic diarrhœa, accompanied with ulceration of the intestines, for two years. Three or four months ago she was put upon small doses of acet. of morphia, which, after several weeks' continuance, arrested the disease, and the patient remained for two months apparently perfectly well. At the expiration of this time the disease suddenly returned, and soon regained all its former activity. The extract of monesia was now commenced in doses of 5 grs. three times a-day, and afterwards increased to 7 grs. four times per day, and continued for ten days, but without any other benefit than a slight decrease of the discharge for the first two days. The medicine was then discontinued, and the morphia substituted with its former success.

CASE 2ND.—A child of W. H., affected with diarrhœa of 18 months' standing, had used anodynes, alteratives, and astringents freely, but without permanent benefit. The ext. monesia, given in doses of two grs., three times per day, assisted by the warm bath and frictions with flannel, checked the discharges in two days, and removed all traces of the disease in two weeks.

CASE 3RD.—I. M., aged 70—health delicate, had a diarrhœa, with short intermissions, for six months—had been treated with creta cum hydrarg., with but temporary relief. Took six grs. of monesia four times per day, from the 2nd to the 10th of November, assisted by a few grs. of pulv. Doveri occasionally at bed time. The discharge ceased in two days, and upon the tenth he expressed himself perfectly well.

CASE 4TH.—MRS. T., diarrhœa of four or five days' continuance—slight fever, tormina, stools bloody and slimy. Had resorted to no previous treatment. Took 8 grs. of ext. mones. three times the first day, which checked the discharge, and allayed all unpleasant symptoms.

CASE 5TH.—A child of four years of age, affected with a bowel complaint, which had commenced in the summer, and continued unchecked until November, though treated with a variety of astringents, and subjected to counter-irritation upon the abdomen. The monesia was given in doses of 2 grs. five times daily, for six days, with the effect of gradually diminishing the discharges, changing their color and consistence, and creating a vigorous appetite, which the child had not previously enjoyed. Having no more of the remedy, I was compelled to relinquish it. Dover's powder and flannel frictions were substituted, and I had the satisfaction of seeing my patient perfectly relieved in another week.

CASE 6TH.—In this case the remedy entirely failed, as in the first case. It was one of diarrhœa supervening upon phthisis pulmonalis, and had been previously treated by creta cum opio, acetate of lead, acetate of morphia, and several of the vegetable astringents, without benefit. The monesia was prescribed at first to the amount of 15 grs., and afterwards increased to 40 grs. per day, and persevered in for three weeks, when, as no abatement of the symptoms was perceived, the remedy was discontinued at the request of the patient.

Remarks.—That the above cases may be entitled to their just weight in the evidence now accumulating, both for and against this new remedy, it may be well to state, that the few last doses taken by MRS. K. in the first case, occasioned slight nausea, accompanied by a slight burning in the stomach. With this exception, I have not witnessed any of the irritating effects detailed by ST. ANGE. My doses, however, were not so large as he prescribed. The above cases lead me to infer that monesia is more astringent than tonic in its operation.

* *American Intelligencer. Salem*, Jan. 6th, 1842.

CERATE OF MEZEREON.*

BY DR. PLEISCHL.

FRESH Mezereon bark, taken at the time of flowering, is digested in alcohol at 0·850, and, after two or three days of contact, it is strained and fresh spirit is added to the bark, and this is repeated until the substance is completely exhausted of its soluble principles.

Then, all the liquors are combined, and submitted to the action of hydrate of lime prepared with three parts of water to one of quick lime; this hydrate should be employed in the proportion of one part and a half, by weight, to three of mezereon. It is digested *de novo* until the mixture becomes of a yellowish green color, then it is distilled to collect most of the alcohol. Water is added to the residue, which separates from it a matter of a green color and soft consistence, one part of which is mixed with four parts of wax and eight parts of olive oil.

* *Buchner's Repertorium für die Pharmacie.*

THERAPEUTICAL EMPLOYMENT OF OXIDE OF MANGANESE.*

BY DR. ERIGELER.

Dr. E. having remarked, in an establishment for bleaching by means of chlorine disengaged with oxide of manganese, that the workmen occupied in this work, and daily under the influence of the latter agent, were in a short time freed from the swelled glands and cutaneous eruptions with which almost all of them were affected on their entering into the manufacture, thought that this result must be attributed to the oxide of manganese. Consequently, he prescribed this substance, in the dose of 1 to 5 centigrammes, for several scrofulous children, and, under the influence of this medicine, he saw the symptoms first amend, and finally completely disappear.

EMPLOYMENT OF IODIDE OF POTASSIUM AGAINST BLEMISHES OF THE CORNEA.†

BY DR. EVERMANN.

Dr. Evermann, of Dussceldorff, has prescribed with the greatest success against films of the cornea, following neglected ophthalmia, and always when the lesion extended no further than the external lamella, the topical application of iodide of potassium in solution.

The dose in which this physician has employed this medicine, is that of 25 to 40 centigrammes in 24 grammes of distilled water.

PASTILLES OF THE CHOCOLATE OF PROTOCARBONATE OF IRON AND POTASSA.‡

BY M. MOUCHON, JUN.

℞ Bicarbonate of potassa . . .	8 gram.
Pure sulphate of iron . . .	8 ,,
Chocolate	500 ,,

The two salts are dried on a stove, then separately reduced to an impalpable powder, and, after having accurately mixed them, they are very intimately incorporated with the 500 grammes of chocolate, which must previously be softened by means of heat.

When the mass is very homogeneous, it is formed into small cylinders which are divided into fractions of eight decigrammes, of which pastilles are formed according to the rules of art.

Each of these pastilles or *tablettes* contains two centigrammes and a half of base,—a quantity sufficient for the medical effect, and such that the taste of the salts is covered by that of the chocolate.

* *Oesterr. Medicinisches Jahrbuch*, Vol. XVI. No., 1.

† *Medicinische Zeitung*, No. 26, 1842.

‡ *Bulletin de la Société de Médecine de Toulouse*, 1841.

TREATMENT OF ASPHYXIA.

The treatment of asphyxia, says Dr. Marshall Hall, involves an attention both to the functions of respiration, and to that of the true spinal marrow. The object, doubtless, is to effect a restoration of the respiratory and circulatory functions, the former of which has been arrested by the external conditions of the patient; the latter, by the contact of morbidly carbonised blood with the capillary vessels of the lungs. The first thing to be attempted is the restoration of warmth by active friction with warm hands, &c.; the second, the imitation of artificial respiration, by any means at hand, of which none is better, usually, than the action of alternate pressure and its relaxation, applied to the thorax and abdomen, so as to induce expiration first, and inspiration immediately by the play of the elasticity of the ribs. The third effort is made by suddenly dashing cold water on the face and general surface, previously warmed by the frictions, in the hope of inducing a more decided inspiration. Artificial respiration must be attended to, if these measures, very promptly enforced, fail; and unless the proper apparatus be present, the mouth of another person, of robust make, is to be applied to that of the asphyxiated person, covered with a handkerchief, the nostrils being closed.

A NEW METHOD OF TREATING RETENTION OF URINE.*

BY G. D. DERMOTT, ESQ.

In a case of retention of urine, occasioned by stricture of twelve years standing, combined with spasmodic and irritable condition of the membranous portion of the urethra, where the attempt to introduce both the bougie and catheter proved unavailing, I succeeded in obtaining a relaxation of the stricture and a passage by the following plan:—took a hydrocele India rubber bottle, furnished with an ivory tube, about two inches in length, filled it with warm water, about blood heat, precisely in the same manner as it would have been filled in performing the operation for the radical cure for hydrocele, then introduced the whole length of the tube of syringe into the urethra, firmly grasping the glans against the tube with one hand, to prevent the escape of the water, whilst, with the hollow of the other, I embraced the hy-

* From the *Medical Times*.

drocele bottle, forced the injection through the urethra against the stricture, and continued an equally directed and uninterrupted pressure against the stricture, through the medium of the warm water, until the stricture yielded, which was in the course of three or four minutes, and which was known by the loss of resistance felt on the part of myself, and by the patient experiencing a slight gurgling sensation, as the water was passing the stricture into the bladder. Both water and urine were immediately afterwards evacuated in a tolerable stream. It is well to follow the evacuation of urine with the ejection of a couple, or even three syringefuls of warm water, keeping the urethra grasped by the left hand after the injection of the first, until the second syringeful is introduced; and so with regard to the third, if had recourse to, in order to prevent the escape of the water, so as to keep up a due distension of the urethra, by the pressure of the non-stimulating but soothing fluid. If there be much irritation in the mucous membrane of the bladder, I have seen good done by injecting an India-rubber bottle full of warm water, with the addition of a little liq. plumbis diacetatis (in proportion of a drachm to a pint), and allowing it to remain a few hours in the bladder; hyoscyamus and belladonna used in the same manner also prove of service. Indeed, I am sure much more influence can be gained, than has been yet obtained, over morbid conditions of the mucous membrane of the bladder, by the free introduction of medical substances, in form of injections into it, taking at the same time due cognizance of the chemical constitution of the urine, in order to avoid precipitation of any of its constituents.

The impulse of the warm water has a mechanical effect in dilating the stricture, similar to the pressure of a bougie, but without the irritation of the latter: it locally soothes the irritable urethra and neck of the bladder, operating as a warm bath internally applied, and far more efficiently in relaxing urethral spasm than warm water applied externally to the perineum. The injection should not be introduced too hot, nor too cold, because either one or the other would have a tendency to increase spasm instead of allaying it; the injection when introduced should be just warm enough to communicate a pleasant warmth to the finger.

These injections should be repeated every night and morning, and better still if also at mid-day, until the stricture is so dilated, and irritation so allayed, that the cure can be proceeded with, by means of the bougie and catheter.

The syringe should be well compressed by the hand until the pipe is fairly immersed in the water for filling, in order that as little air as possible may be injected into the bladder with the water, inasmuch as the former may, perhaps, prove injurious, should there be much vesical irritation with muco-purulent secretion, by making the latter putrescent; but in no other way, I conceive, could the injection of a small quantity of air do harm.

REVIEW:—

MEDICAL GUIDE FOR MOTHERS, *in Pregnancy, Accouchement, Suckling, Weaning, &c. &c., and in most of the Important Diseases of Children.* By J. R. Hancorn, M.R.C.S. Second Edition. London: Smith, Elder and Co.

We are glad to see a second edition of a work so entitled to popularity, and it is our sincere hope that many a mother has obtained so valuable a counsellor,—one to which the most timid may resort for aid without being abashed,—and that those who have not procured the first will not delay to possess themselves of the second.

Having noticed the first edition of this work at some length, and seeing no reason to do aught but reiterate the highest opinion of it, we must now commend this edition to the public. Here is a lesson for those ladies who suckle by proxy:—

"Suckling by the Mother.

"To enumerate the inconveniences to be dreaded by the female who does not nourish her own child, will sufficiently indicate the advantages reserved for the good and tender mother who fulfils that sacred duty. The accidents which follow accouchements when the mother is not the wet nurse, may be referred to two principal heads, accordingly as the breasts, or some other part of the body, is the seat of them. On the third or fourth day after accouchement, the breasts secrete a sweet and abundant liquor: if the infant, for which this was designed, does not make use of it, it may remain in these organs, and distend them, if it continues to flow to them, or it may return to the general mass. In the first case, the milk, accumulating in the breasts, distends them so forcibly as to produce sharp pains, which deprive the female of sleep, and may have still more disastrous consequences; or the excitement necessary to the secretion of milk may, by the prolonged stay of that liquid in the organ, aid in producing an inflammation, which, if it is ultimately resolved, will not only cause intolerable pains, but also it may produce numerous abscesses of long duration, and always accompanied with intense

suffering. There is also another mode of termination of the swellings of the breasts, no less injurious than the preceding. It sometimes happens that they leave after them a small, hard, indolent tumour, which cannot be dispersed, and which becomes, at the period when she ceases to bear children, the germ of schirrhus or cancers, of which a painful operation does not always prevent a fatal return.

"When the milk flows back into the general mass, it produces a plethora, which it is sometimes difficult to remove, and very often gives birth to those numerous milk-diseases, against which the most varied means of art are too often inefficacious. Of this number is milk-fever, which in general is more moderate when the woman gives suck, and less liable to be combined with acute affections more or less serious. The inflammations, which are sometimes observed at the time of lying-in, are moderated, and even prevented, by the organic labor which the action of sucking keeps up in the breasts. The uterus, already fatigued by the labour of accouchement, and the pregnancy which has preceded it, must also, in the female who does not suckle, give a passage to humors which would have flowed out by the breasts; it does not reject them with the same facility as they flow to it; hence overcharging of the vessels, schirrus and leucorrhœa, abscesses, rheumatism, and the numerous series of chronic affections which have long been attributed to checked milk, are sufficiently accounted for by the increased action of the cutaneous system of the woman, who in general perspires abundantly, and thereby becomes more sensible to the action of cold.

"Several accoucheurs have thought that there existed, between the constitution of the child and that of its mother, analogies which can be found only by chance in a nurse, however well chosen she may be. Others oppose this opinion, but they cannot dissemble that the *consistence* of the milk, not essential to the nourishment of the infant, increases according to its strength and its wants, which would be found only with a nurse brought to bed at the same time as the mother. The difficulties in choosing a nurse diminish still more the chances favorable to the existence of the new-born infant. It sometimes also happens that the latter is attacked with diseases, for the cure of which it is necessary to impregnate the milk with principles which alone can effectually resist them. Who but a mother would then consent to confine herself to a regimen, which would impose on her the sacrifice of her taste and inclinations? Who, besides, will have for the infant those minute cares, that tender solicitude, that attachment, of which it has so much need, from its weakness and its inexperience of life?"

The author has, among many other things, added a very good chapter on "Going out of Town;" but our readers must read for themselves.

BOOKS RECEIVED.

Essay de Statique Chimique des Etres Organisés. Par J. M. Dumas. Paris: Fortin, Masson et C^ie^. London: H. Baillière.

Handwörterbuch des Chemischen Thiele der Mineralogie. By H. Rammelsberg. London: H. Baillière.

Histoire de la Chimie depuis les temps les plus reculés jusqu'à notre époque. By M. Hoefer. Vol. I.

Elements of Electrometallurgy. By Alfred Smee. Part VII. Palmer, 103, Newgate Street.

Traité Complet de Pharmacie. 1^er^ Partie, Pharmacie Galénique. 2^de^ Partie, Pharmacie Chimique. H. Baillière. 2 vols.

The American Journal of Science and Arts. Conducted by Professor Silliman and B. Silliman, jun. New Haven: B. and W. Noyes. London: Wiley and Putnam, Paternoster Row.

TO CORRESPONDENTS.

Z.—Dissolve some chloride of calcium in distilled water, and add crystallised carbonate of soda until it ceases to give a precipitate; then wash several times, and dry at a moderate heat.

A Student is referred to Mr. Parnell's work on Analysis, page 282, *et seq.*

The person who writes complaining, enters on a subject for which his education does not render him fit.

ERRATA IN No. XXXV.

Page 356, col. 2, line 15 from top, *for* "For ourselves," *read* For our services; line 22 from top, *for* "virtue," *read* value; 357, col. 2, line 16, *for* "Secretary," *read* Vice-President.

Nota Bene.—*All Communications and Books for Review must be addressed, "To the Editors of the Chemist, Care of Mr. Hastings, Publisher, 13, Carey Street, Lincoln's-inn Fields, London." Communications must be prepaid, and sent before the 15th of each Month; Books for Review before the 10th.*

INDEX

TO THE THIRD VOLUME.

N. B.—*For Names of Contributors, &c., see separate* INDEX *at the end.*

PAGE

Abuses of—
the alkali trade 116, 145, 175
the drug trade, 93, 122, 151, 240, 392
Acetum opii, Houlton's . 159
Acid,
arsenic, new compound of bitartrate of potassa and . . 332
arsenious presence of antimony in 268
——— treatment of intermittent fevers by . . 153
benzoïc, conversion of, into hippuric acid 379
camphoric, anhydrous, action of anhydrous phosphoric acid on 113
chromic, new method of preparing 266
elaïdic, investigations concerning 73
fat, of cocoa nut oil . . 42
ferric, formation of by the galvanic way 193
formic, formation of, in oil of turpentine . . 268
——— new method of producing 233
——— new instance of the production of . . 349
hippuric, conversion of benzoïc acid into . . 379
hydrocyanic, case of poisoning by, medico-legal report concerning 152
iodic, new means of obtaining . 43
lithocholic acid . . . 140
lithofellic, a new principle of biliary calculi . . . 45
——— and the products of the action of nitric acid on the substance . . . 381
margaric, investigations concerning . . . 40
nitric, investigations concerning . 228
——— presence of iodine in . 178
——— products of the action of, on lithofellic acid . . 381
oleïc, investigations concerning . 70
perchloric, on the extraction of all the, from the perchlorate of potassa . . . 234

Acid, PAGE
permanganic, new instance of the formation of . . 381
phosphoric, anhydrous, action of an anhydrous camphoric acid . 113
——— and the phosphates, composition of . . 264
phosphoric, medicinal . . 254
phosphorous, tests for . . 15
sulphuric, on the arsenic in, method of extracting it . 388
——— with one equivalent of water, purification of, for accurate analyses and medico-legal investigations . . 199
——— of Saxony or Nordhausen, on some inconveniences attending . . . 172
sulphurous, as a re-agent . 46
valerianic, and the valerianates . 369
Acids,
chromic and sulphuric, compound of 144
of maniganium, oxidation of the constituents of ammonium by 232
metallic investigations concerning 134
organic, salts of, with baryta, potassa and soda, on a process of analysis applicable to . 198
Aconitum Napellus, extract of, twelve persons poisoned by . . 62
Adulteration,
of alum, powdered . . 396
of cane and beet-root sugar, with sugar of fecula; means of detection . . . 238
of drugs . . . 393
and manufacture of chromate of potassa . . . 387
of nitrate of soda of commerce . 237
of tobacco . . 304, 353
Advertisement . . . 1
Ægirine and titanate of iron, two new minerals from Scandinavia . 114
Air,
on the composition of . . 168
description of some processes for analysing . . . 65

PAGE

Albumen, coagulated, and fibrin, solubility of, in water . . 235
See Fibrin.
Alkali trade, abuses of the 116, 145, 175
Alkaline earth. *See* Earths.
Allantoïn 332
Alum, employment of, in the gangrenous sore throat which complicates scarlatina . . 364
Ammonia,
chloro-aurate of, emmenagogue solution of . . . 288
new instance of the formation of 348
on the compounds of the metallic bromides with . . 201
on the action of metals and some of their combinations on, at a high temperature . . 202
Ammonium,
and sulphur, new crystallised combinations of . . . 136
oxide of, on the oxidation of the constituents of, by the acids of manganium . . 232
Animal
chemistry . . . 321
electricity . . . 379
oil, pyrogenous, efficacy of in consumption . . . 254
Animals,
poisoned, flesh of, danger of making use of the . . 192
effects of quinine on . . 361
Anthrakokali and fuligokali, preparation and therapeutical employment of 367
Anti-epileptic medicine . . 223
Antimony,
and camphor, powder of . 287
presence of, in arsenious acid . 268
toxicological investigations concerning . . . 361
Arquebusade . . . 94
Arsenic,
absence of, from the zinc of commerce . . . 349
acid, and bitartrate of potassa, memoir on a new compound oï urea and allantoïn . 332
in the soda of commerce . 177
mercury and double iodide of (iod-arsenite of mercury) . 43
on the, in sulphuric acid, with a method of extracting it . 388
poisoning by: suspension of the symptoms for eighteen days. Relapse and cure . . 127
Arsenious acid,
presence of antimony in . 268
treatment of intermittent fevers by 153
Arseniuretted hydrogen, poisoning by 98
Asphyxia, treatment of . 396

PAGE

Assistants,
druggists and their . 242, 282, 310
of medical men and chemists, treatment of; proposed remedy 211
Atmospheric,
air, description of some processes for analysing . . . 65
electric discharges . . 170
Atomic weight
of carbon . . . 109
of zinc 233
Atoms, on the constitution of . 297

Barium, chloride of, case of tic douloureux cured by . . . 394
Baryta, potassa and soda, salts of, with organic acids, on a process of analysis applicable to . . 198
Batteries, constant, on the substances employed as diaphragms in . 236
Battery,
galvanic, new and very powerful . 15
Grove's voltaic, improvement in . 233
Beer, ferruginous . . . 189
Belladonna in epilepsy . . 314
Benzoïc acid, conversion of hippuric acid into . . . 379
Bleeding from the nose, simple method of arresting . . . 364
Books received, 96, 192, 224, 256, 288, 320, 368, 398
Bromides, metallic, on the compounds of, with ammonia . . 201
Bromine, decomposition of, water by, 40
Bronze for electrotype medals . 49
Burning glasses . . . 50

Calotype process, The . . 122
Camphor,
and antimony, powder of . 287
and tar ointment of, its therapeutical employment . . 365
Camphoric acid, nhydrous, action of anhydrous phosphoric acid on . 113
Cantharides, symptoms caused by . 286
Carbon, on the atomic weight of . 109
Carbon, sulphuret of, and ethal, on some products of the reciprocal action of . . . 349
Caseum. *See* Fibrin. . . 257
Caustic, Vienna, on the treatment of varix by . . . 187
Caution to soap-makers. *See* Alkali trade.
Cements. *See* Mortars.
Cerate of mezereon . . . 395
Chemical jurisprudence. Case of Cooper the footpad. Impropriety of admitting, in Courts of Law, the analysis of medical men, in cases affecting life and property . 181

PAGE
Cheese, poisoning by . . 363
Chemistry, animal . . . 321
Chemists and druggists, qualification of 123
Chilblains in children, treatment of . 320
Chlorine
on the combination of, with bases 294, 336, 374
on the combination of, with sulphur . . 15, 174
on a new compound of, with oxygen 347
Cholera, treatment of . .
Chrome, on the sulphates of . . 144
Chromic acid,
new method of preparing . 266
and sulphuric acid, compound of . 144
Citron, turpentine and, crystals of the essences of . . . 115
Clinkers, a new remedial agent . 182
Cobalt, peroxide of; new instance of its formation . . . 381
Cocoa nut oil, on the fat acid of . 42
Colwell's trusses . . . 128
Copaiba, resin extracted from, investigations concerning . . 144
Copper,
iodide of, method of obtaining . 114
plates (metallic), formation of, from common impressions of prints, by the aid of the electrotype . . . 23
plates, expeditious method of multiplying, by means of the electrotype . . . 23
poisoning by . . . 253
precipitation of, from its sulphate, on tin, iron, and zinc, by galvanic agency . .
sulphite of, means of obtaining in fine crystals . . 47
Corrosion, preservation of metallic goods from . . . 278
Creosote,
in phthysis pulmonalis . . 364
styptic effects of . . . 188
Croton oil,
external application of . . 364
plaster of . . . 224
Croup, repeated vomiting as a therapeutic agent in . . 154
Crystallisation, on the light which appears during . . . 76
Cubebs, injections of infusion of, in vaginitis . . . 318
Curcumine, memoir on . . 275
Cyder containing a salt of lead, poisoning by 159

Diamond, on the ash which remains after the combustion of the . 202
Diaphragms, on the substances employed as, in constant batteries 236
Digitalis, poisoning of the East India Company's camels by, in Cabul 254
Discharges, atmospheric electric . 170
Drinks, cold, effects of, on the body during perspiration . . 155
Drug Club, Junior . . . 358
meeting convened by (report) . 307
Druggists,
and their assistants 242, 282, 310
qualification of . . . 123
on certain attempts which have been recently made with the professed view of improving the position of, as a body . 280
Drug trade, abuses of, 93, 122, 151, 240, 392
Drugs, adulteration of . . 393

Earths, alkaline, action of water on the combinations of sulphur with 341
Eau de Cologne . . . 94
Education, government and practice, medical, on a new system of . 50
Efflorescences, on the walls of stables . 6
Elaïdic acid, investigations concerning 73
Electric discharges, atmospheric . 170
Electric moulds . . . 239
Electricity,
animal 379
frictional, sources of . . 336
of mineral veins . . 250
tendency of, to promote the growth of plants, and some of the causes of its accumulation in the atmosphere during the winter and spring seasons . 37
voltaïc, improved method of multiplying engraved plates by . 122
Electro-casting, new method of preparing moulds for . . 120
Electro-lace 81
Electro-metallurgical operation, description of a, for producing plates from impressions of engravings, &c. . . . 389
Electrotype,
on the, and on certain phenomena connected with it . 354
expeditious method of multiplying copper plates by . . 23
medals, bronze for . . 49
medals, method of casting in one piece . . . 85
new and useful application of . 85
process for engraving in the wood-cut style . . . 23
Emmenagogue,
mixture . . . 394
solution of chloro-aurate of ammonia . . . 288
pills, Dr. Sichels' . . 365

PAGE

Epilepsy,
belladonna in . . . 314
indigo in . . . 153
Equivalents,
chemical symbols and thermometers, necessity of establishing an universal system of . 11
See also Symbols and Thermometers
on the necessity of establishing an universal system of . 13
Ergot of rye, on the use of . . 125
Erysipelas,
nitrate of mercury ointment in . 363
Etching, rapid method of . . 390
Ethal and sulphuret of carbon, on some products of the reciprocal action of 349
Ether,
hydro-telluric, preparation of . 113
new mode of obtaining . . 349

Ferri potassio-tartras, on the, of the London Pharmacopœia . 248
Ferric acid, formation of, by the galvanic way . . . 193
Ferruginous,
beer, formula for a . . 189
compounds, on certain . . 214
See Iron.
Fibrin and coagulated albumen, solubility of, in water . . 235
proximate composition of; gluten, albumen and caseum . 257
Fires on board ships . . . 278
Fly, bite of a, death from . . 365
Foils, or Tinsel,
preparation of a liquid for communicating a beautiful crimson color to . . 49
preparation for coloring of a fine yellow . . . 278
olympian green mixture for communicating that color to . 238
Formic acid,
new method of preparing . 233
formation of in oil of turpentine . 268
new instance of the production of 349
Frauds. *See* Adulterations.
Fuligokali. *See* Anthrakokali.
Fusing points, observations on the, of certain bodies in the crystallised and amorphous state . . 142

Galbanum 155
Galvanic battery, new and very powerful 15
Gases, repulsive energy of . . 108
disengaged by marine plants . 13
Gestation, impressions made on the mother during, on the influence of, on the conformation of the child with which she is pregnant 358
Gilding,
metals by the humid way and voltaic current, notice of a new mode of . 82, 118
processes of Mr. Elkington and M. de Ruolz, report on . 146
Gluten 257
Glyphography . . . 391
Gold,
iodide of, process for the preparation of . . . 187
iodides of, with remarks on the proportional number of that metal, and on the medicinal employment of the protiodide . . . 25
Gonorrhœa, on a mode of treating . 315
Gouty person, analysis of a substance secreted on the hand of . 115
Guaiacum, resin of . . . 115

Hats, beaver and silk manufacture of; ill effects produced by wearing silk hats . . . 350
Hippuric acid, conversion of benzoïc acid into . . . 379
Hœmostatic water . . . 188
Homœopathy, union of the regular practice with . . 31
Horse radish, spasms of the stomach cured by the application of the last dorsal vertebræ . 128
Hydragogue pills in ascitis . . 287
Hydrargyrum cum creta, state of mercury in . . . 213
Hydrocyanic acid, supposed case of poisoning by, medico-legal report concerning . . 152
Hydrogen
and lead, bichlorides of . . 203
salts of 267
Hydrotelluric ether, preparation of . 113

Indigo,
extraction of from the polygonum tinctorium . . . 23
investigations cencerning . 97
in epilepsy . . . 153
Indigotine, new process for obtaining . 380
Iodic acid, new means of obtaining . 43
Iodine,
action of fluoride of potassium on 330
inhalation of phthysis, injurious effects of . . . 255
in the nitric acid of commerce . 178
schirrus of the uterus cured by the external and internal exhibition of . . . 286

PAGE

Iron,
decomposition of water by . 140
hydrate of peroxide of . . 160
magnetic oxide of . 31, 94
sesquichloride of, tincture of in gleet . . . 255
substitution of, for zinc, as the electro-positive metal for experiments with voltaïc electricity . . . 122
sulphuret of, on the therapeutical employment of . . 253
syrup of subcarbonate of, formula for 189

Jalap,
resin of, new process for obtaining in a state of purity, and perfectly white . . . 156
scammony and, identity of their active principles . . 62
Junior Drug Club . . 307, 358

Kusic powder (poudre kusique) sold as a cure for the diseases of dogs, examination of . . 184

Labels, insoluble . . . 158
Lactucarium, new mode of collecting . 359
Laudanum, poisoning by . . 286
Lead,
combinations of . . . 33
hydrogen and, bichlorides of . 203
poisoning by cyder containing . 159
presence of in water, owing to the use of leaden pumps . 273
Light, on the, which appears during crystallisation . . 76
Lithocholic acid . . . 140
Lithofellic acid, a new principle of biliary calculi . . 45
Lithofellic acid and the products of the action of nitric acid on this substance . . 381
London Electrical Society, 22, 80, 81, 145, 174, 236, 386

Madia Sativa, oil of, investigations concerning . . . 299
Magnesia, preparation of medicinal waters with . 30, 95, 125
Magnet, employment of . . 224
Malaria at the mouth of tropical rivers 80
Manganese,
means of distinguishing, from zinc in solutions which contain ammoniacal salts . . 346
new mode of testing . . 149
oxide of, therapeutical employment of . . . 396
Manganium, acids of, oxidation of the constituents of oxide of ammonium by . . . 232

PAGE

Manna, substitution of a saccharine product for . . . 157
Margaric acid, investigations concerning 40
Marine plants, gases disengaged by . 13
Mattico as a styptic . . . 315
Meat, spoiled, a large company poisoned by 317
Medical education, government and practice, on a new system of . 50
Medical reform . . . 86
Medical Reform Bill, Sir J. Graham's 179
Medicine, anti-epileptic . . 223
Melting bag 286
Mercury,
ammoniacal nitrate of, employment of in photographic experiments . . . 121
and arsenic, double iodide of . 43
nitrate of, ointment of . . 185
protiodide of, pills of . . 394
state of in hydrargynum cum creta, 213
Metals, gilding, new mode of, by the humid way and the voltaic current . . 82, 118
Mezereon, cerate of . . . 395
Microscope, oxyhydrogen, portable . 85
Milk, in nursing women, means of remedying certain alterations of 366
Mineral veins, electricity of . . 250
Molecular changes which sugar undergoes under the influence of water and heat . 129, 161
Monesia, six cases of diarrhœa treated with 395
Mortars and cements, on the employment of in building . . 351
Moulds for electro-casting . . 120
Mucus, composition of . . 302
Musk, artificial, and a new oil (eupione of amber) discovered in rectified oil of amber, chemical examination of . . . 298

Nicotine, on . . . 167
on the composition of, and of some of its combinations . 143
Nitric acid,
investigations concerning . 228
presence of iodine in . . 178
products of the action of on lithofellic acid . . . 381

Ointment of nitrate of mercury . 185
Oleïc acid, investigations concerning . 70
Opii, acetum, Houlton's . . 159
Opium, treatment of strangulated hernia by . . . 188
Oxygen and chlorine, on a new compound of . . . 347

PAGE

Ozone, note on 202

PASTILLES of chocolate of protocarbonate of iron and potassa . 396
Perchloric acid, extraction of all the, from the perchlorate of potassa 234
Permanganic acid, and peroxide of cobalt, and new instance of the formation of . . . 381
Pharmaceutical,
  jobbing . . . 25
  nomenclature . . 283, 312
  Society . 150, 240, 356
  ——— and qualification of chemists and druggists . 123
  ——— refusal of, to receive the subscription of a chemist and druggist (case of Mr. Kay) . 180
Phloridzine, a new medicine . . 288
Phosphates, phosphoric acid and, composition of . . . 264
Phosphoric acid,
  anhydrous, action of, on anhydrous camphoric acid . 113
  and the phosphates, composition of . . . 264
  medicinal . . . 254
Phosphorous acid, tests for . . 15
Photographic,
  drawings, tinting of . . 178
  experiments, employment of ammoniacal nitrate of mercury in 121
  images, new method of fixing . 210
Pills of protiodide of mercury . 394
Plants,
  instinct and irritability of, 103, 170, 195, 259
  marine, gases disengaged by . 13
Plaster of croton oil . . . 224
Plates,
  copper, metallic, formation of, from common impressions of prints, by the electrotype . . 23
  ——— expeditious method of multiplying by means of the electrotype . . . 23
  stereotype 121
Platinum, oxide of, preparation of . 144
Poisoned animals, danger of making use of the flesh of . . 192
Poisoning,
  by arsenic. Suspension of the symptoms for eighteen days. Relapse and cure . . 127
  by arseniuretted hydrogen . 89
  by binoxalate of potassa . . 285
  by cheese . . . 363
  of a large company by spoiled meat . . . 317
  of twelve persons by aconitum napellus . . . 62
  by copper . . . 253

PAGE

Poisoning—*continued.*
  by cyder containing a salt of lead in solution . . . 159
  by digitalis, of the East India Company's camels in Cabul 254
  by hydrocyanic acid, case of, medico-legal report concerning 152
  by laudanum . . . 286
  by quinine, its effects on animals 361
  by squills . . . 318
Pommade for atonic ulcers of the legs 319
Polygonum tinctorium, extraction of indigo from . . . 23
Potassa, binoxalate of, poisoning by . 285
  bitartrate of, new compound of, and arsenic . . . 332
  chromate of, manufacture and adulteration of . . 387
  salts of, with organic acids. *See* Baryta.
Potassium,
  cyanuret of, preparation and employment of . . 225
  fluoride of, action of on iodine . 330
  iodide of, employment against blemishes of the cornea . 396
  ——— in secondary syphilitic symptoms . . . 154
Potatoe starch . . . 273
Purgatives, use of, in the typhus fever of Paris . . . 316

Quinine,
  dose of required for curing intermittent fever, means of accurately determining . 223
  lactate and valerianate of . 287
  sulphate of, as a cure for angina pectoris . . . 127
  ——— effects of on animals, poisoning by . . . 361
  ——— new method of administering . . . 223
  ——— tartarised, draught of . 318

RACEMATES, composition and properties of . . . 142
Rennet, new mode of preparing . 389
Resin,
  extracted from balsam of copaiha, investigations concerning . 144
  of guaiacum . . . 115
  jalap, new process for obtaining, in a state of purity, and perfectly white . . . 156
REVIEWS:—
  Elements of Chemistry; including the most recent Discoveries, and applications of the Science to Medicine and Pharmacy, and to the Arts. By Robert Kane, M.D., M.R.I.A., &c. 16

PAGE

Reviews—*continued.*

Elements of Chemistry; including the Applications of the Science in the Arts. By Thomas Graham, F.R.S.L. & E., &c. &c. Part VI. . . . 20

Pathology, founded on the natural System of Anatomy; a philosophical Sketch, in which the natural Classification of Diseases, and the Distinction between Morbid and Curative Symptoms, afforded by pain or its absence, are pointed out; as well as the Errors of Homœopathy and other Hypotheses. By Alex. Walker . 31

Lectures on Chemistry; including its Applications in the Arts. By Henry M. Noad. Nos. VI. and VII. . . . 47

Proceedings of the London Electrical Society.—Session 1841-2. Part IV. V. & VI. . 47, 145

The Inventors' Advocate. . 50

Elements of Materia Medica and Pharmacy. By O'Brien Bellingham, M.D. Edited by Arthur Mitchell, M.D. Part I. 55

Medical Guide for Mothers, in Pregnancy, Accouchement, Suckling, Weaning, &c. &c., and on some of the more important Diseases of Infancy. With an Appendix, on the successful Treatment of Consumption, by a new Remedy, suggested by Dr. Ulric Palmedo, of Berlin. By J. R. Hancorn, M.R.C.S. . 57

An Essay on Diabetes. By H. Bell, D.M.P., Paris. Translated by Alfred Markwick . 64

Letter to the Right Honourable Geo. Earl of Aberdeen, K.T., &c. &c. Principal Secretary of State for Foreign Affairs, Chancellor of the University and King's College, Aberdeen, on the State of the Schools of Chemistry in the United Kingdom. By Wm. Gregory, M.D., F.R.S.E., M.R.I.A. 81

The Pocket Formulary. By Henry Beasley, 2d edit. . . 96

Medical Students' Guide . . 96

What to Teach, and how to Teach it. By Henry Mayhew . 124

Elements of Electro-Metallurgy; or the Art of Working in Metals by the Galvanic Fluid. By Alfred Smee. 2d edition . 150

PAGE

Reviews—*continued.*

An Investigation of the present Unsatisfactory and Defective State of Vaccination, &c. &c. &c. In a series of Letters to Dr. George Gregory, Physician to the Small-Pox and Vaccination Hospital, London. By Thomas Brown . 155

Structure and Distribution of Coral Reefs. Being the First Part of the Geology of the Beagle, under the command of Capt. Fitzroy, R.N., during the years 1832 to 1836. By Chas. Darwin, M.A., F.R.S., F.G.S., Naturalist to the Expedition . . . 203

Chemistry of the four Ancient Elements—Fire, Air, Earth, and Water. An Essay, founded upon Lectures delivered before Her Most Gracious Majesty the Queen, and dedicated by special permission to Her Majesty. By Thomas Griffiths . . . 208

The Effects of a Lightning Flash on the Steeple of Brixton Church; and Observations on Lightning Conductors generally. By Charles V. Walker, Esq., Hon. Sec. London Electrical Society . . 208

Memoir on the Absorption of Metallic Poisons by Plants. By P. Lonyet, Brussels . 210

The Elements of Materia Medica and Therapeutics. By Jonathan Pereira, M.D., F.R.S., and L.S., &c. &c. &c. Second Edition, enlarged and improved . . 217, 243

Animal Chemistry; or Organic Chemistry in its applications to Physiology and Pathology. By Justus Liebig, M.D., &c. &c. &c. Edited from the author's manuscript, by William Gregory, M.D. . . 269

Elements of Chemical Analysis, Inorganic and Organic. By Edward Andrew Parnell . 299

On Diseases of the Bladder and Prostate Gland: with plates. By William Coulson. Third edition, revised and corrected 311

Pathological Chemistry; or Chemical Investigations concerning the Solids and Fluids of the Human Body, in their Relations to Physiology and

PAGE

Reviews—*continued.*
Pathology. By S. D. L'Héritier . . . 381
The Second Supplement, completing the seventh edition of Dr. Turner's Chemistry. By Justus Liebig, M.D., &c., and William Gregory, M.D. . 382
Lectures on Electricity, delivered in the Royal Victoria Gallery, Manchester, during the Session of 1841-2. By William Sturgeon . . . 385
An Essay on the Nature and Application of Steam; with an historical notice of the rise and progressive improvement of the steam engine. By M. A. Alderson, C.E. . . 390
Medical Guide for Mothers. By J. R. Hancorn, M.R.C.S. Second edition . . 400
Rhubarb,
as an application to ulcerated surfaces . . . 314
extract of, alteration of . . 319
Rockets,
mariners' signal, an improved construction of . . . 121
sky, improved . . . 239
Rot, dry, Indian prevention of . 355
Rumex obstusifolius, analysis of the root of . . . 254
Rye, ergot of, on the use of . . 125

Salt, new 115
Salts of hydrogen . . . 267
ammoniacal. *See* Zinc and Manganese.
Saponaria, (soap wort,) preparation of syrup of . . . 187
Scammony and Jalap; identity of their active principles . . 62
School, Chemical . . . 63
Silver,
certain combinations of . . 199
nitrate of, preservation of . 288
——— internal employment of 315
——— ointment of, in erysipelas 363
Soapmakers, caution to . . 116
Soap wort. *See* Saponaria.
Soda of commerce,
arsenic in the . . . 177
cyanide of sodium in . 306, 355
nitrate of, adulteration of. . 287
salts of, with organic acids. *See* Baryta.
Sodium, cyanide of, in the soda of commerce . . 306, 355
Solutions against inflammation of the back of the mouth and the digestive tube . . . 187

PAGE

Spirit trade, frauds in . . 352
Squills, case of poisoning by . . 318
Stables, walls of, efflorescences on . 6
Starch, potato . . . 273
Stearic acid, composition and products of the distillation of . . 8
Stereotype plates, improved method of producing through the agency of voltaic electricity . . 121
Stone, on the selection of, for the purposes of building . . 208
Storacine, the crystallisable resin of storax . . . 367
Storax, purification of, for internal use 365
Strychnia, tannin as an antidote for . 362
Sugar,
cane or beetroot, adulteration of, with sugar of fecula, means of detection . . 238
on the fermentable properties of various kinds of . . 46
on the molecular changes which sugar undergoes, under the influence of water and heat, 129, 161
various kinds of, dextrine and gums, means of distinguishing from one another, (Frommer's method) . . 139
on the selection for the preparation of syrup of violets . 249
Sulphur,
combination of chlorine with, 15, 174
new crystallised combinations of ammonium and . . 136
Sulphuric acid,
arsenic in, method of extracting it 388
of Saxony or Nordhausen, on some inconveniences attending the use of, as a test . . 172
with one atom of water, purification of, for accurate analyses and medico-legal investigations . . . 199
Sulphurous acid as a re-agent . 46
Symbols, Chemical. *See* Equivalents.
Symptoms, peculiar, affecting an entire family and terminating in death 190
Syrup,
of balsam of tolu, preparation of . 360
laxative . . . 364
of violets, selection of sugar for the preparation of . . 249

Tannin, as an antidote for strychnia 362
Tanning, improvements in the process of 47
Tar and camphor, ointment of . . 365
Thermometers. *See* Equivalents.
Thunder, observation of a clap of, accompanied by hissing . 78
Tin, chloride of, employment of, in cancerous affections . . 127

PAGE

Tinsels. *See* Foils.
Titanate of iron and Ægirine, two new minerals from Scandinavia . 114
To Correspondents, 32, 64, 96, 128, 160, 192, 224, 256, 288, 320, 365, 398
Tobacco, adulteration of . 304, 353
Tooth-ache, remedy for . . 287
Tolu, balsam of, preparation of syrup of 360
Turpentine
and citron, crystals of the essences of 115
oil of, or a substance isomeric with it, on the slow alteration of, in the turf pits of Denmark . . . 138
——— formation of formic acid in 268

Urea 332
Urine, retention of, a new method of treating . . . 396

Valerianic acid, the Valerianates, &c. 369
Varnish, on the use of, in the employment of the starch bandage in the fractures of children . 159
Veratrine in facial neuralgia . . 319
Violets, syrup, on the selection of sugar for the preparation of . 249

PAGE

Vomiting, repeated, as a therapeutical agent in croup . . 154
Wasps, death from the sting of . 365
Water,
action of, on the combinations of sulpbur with the metals of the alkaline earths . . 341
composition of, investigations concerning . . . 289
decompositon of, by bromine . 40
decomposition of, by iron . 140
hœmostatic . . . 188
presence of lead in, owing to the use of leaden pumps . . 273
Waters, medicinal, on the preparation of, with magnesia . 30, 95, 125
Wolfram, composition of . . 111
Zinc,
atomic weight of . . 233
chloride of, preparation of . 236
of commerce, absence of arsenic from . . . 349
means of distinguishing manganese from, in solutions which contain ammoniacal salts . . 346
substitution of iron for, as the electro-positive metal for experiments with voltaïc electricity . . . 122

LIST OF CONTRIBUTORS.

Adeline, 239
Aimé, 13
Arnand, 359
Arnoldi, 128
Barnett, P., 15
Barral, 167
Berard, 187
Berton, Dr., 315
Bisson, 178
Bolardini, 62
Bonaparte, L.-L., Prince, 369
Boquillon, 354
Bouchardat, 224, 257, 288
Boudin, 153
Bourson, 40, 43, 47
Boyer, 115
Brazendale, 85, 122, 239
Bromeis, 42
Brooks, 239
Brunner, 65
Brunton, 50, 121
Buchner, sen., 159
Burdach, 188
Bush, 15
Cazenave, 253
Chabrely, 366
Charbounier, 121
Cooley, 185, 214, 390
Cooper, 188
Cousserau, 187
Damberger, 389
Danger, 361
Debreyne, 314
Dermott, 396
Desains, 349
Deschamps, 188
Deville, 115
Dubourges, 287
Duflos, 46
Dumas, 146, 168, 289
Duméril, 288
Dupasquier, 172
Edwards, 182
Elphinstone, 390
Elsner, 298
Erdmann, 97, 109
Erigeler, 396
Evermann, 396
Flandin, 361
Fehling, 144
Fife, Dr., 125
Forchammer, 138
Fordos, 25
Franz, 364
Frémy, 134
Fresenius, 142
Fritzsche, 136, 380
Gaultier de Claubry, 349

Gay-Lussac, 336, 374
Giacomini, 361
Gibbon, 395
Gill, 355
Göbel, 45
Guastamacchia, 223
Guérard, 155
Guislain, 358
Habert, 184
Henry, 115
Hiley, 6, 103, 170, 195, 259
Hille, 223
Holt, 283, 312
Hunt, 250
Jacquelain, 199, 233, 349
Jobert, 363
Jones, 255
Kay, Hildreth, 180
Keller, 379
Kemp, 302
Krantz, 238
Labordette, 249
Lafargue, 154
Lander, 278
Laurent, 349
Le Calvé, 317
Legripe, 160
Leistner, 189
Lembert, 178
Lepage, 365
Levol, 149
Liebig, 225
Lonchamps, 264
Lonyet, 82, 118
Lutterotti, 394
Ludwig, 155
Lüdicke, 362
Marchand, 109, 174, 360
Markwick, 314
Malaguti, 384
Matteucci, 379
Marotte, 154
Meillet, 187
Meissner, 396
Ménier, 157
Meyer, 73
Millon, 203, 228, 347
Mitscherlich, 139
Mouchon, jun., 396
Mowbray, 248
Munro, 315
Nardo, 187
Nativelle, 156, 234
Nauche, 127
Naylor, 23, 49, 121
Negrier, 364
Newnum, 23
O'Reilly, 89
Orfila, 152
Ortigosa, 143
Osborn, 62, 273
Otto, 346
Payen, 273
Pelouze, 33, 332
Petzhold, 202
Phillips, Reuben, 108, 114, 170, 199, 210, 232, 267, 297, 330, 336, 381, 388
Pine, 37
Pleischl, 395
Prony, 318
Plantamour, 114
Poggendorff, 193
Pollins, 363
Polya, 367
Provostaye, 349
Rammelsberg, 201
Redtenbacher, 8
Reinsch, 319
Reizet, 348
Ricord, 394
Riegel, 299
Righini, 236
Rockline, 85, 120, 122
Rose, H., 46, 76, 341
Rowe, 236, 238
Sallamea, 127
Sarzeau, 381
Schaffgotsch, 111
Schiotter, 144
Schrœtter, 202
Seidel, 223
Sigg, 317
Simonim, 153
Simpson, 278
Smith, 355
Soubeiran, 43, 129, 161
Stöber, 158
Tavignot, 159
Tessain, 78
Tott, 394
Trorel, 23
Varrentrapp, 40, 70
Vauquelin, 47
Viper, 140
Vogel, 235
Vogel, jun., 275
Wackenroder, 254
Walter, 113
Watt, Charles, sen., 50 (also editorial articles, &c. &c.)
Watt, John, 181 (also editorial articles, &c. &c.)
Watt, Charles, jun., 11, 177, 208, 213, 233, 237, 266, 306, 351, 387
Weppen, 268
Wiggers, 268
Wilson, Dr., 190
Wislin, 389
Wittstein, 144
Winzheimer, 364
Witting, 144
Wöhler, 15, 113, 140, 142
Wolfring, 318
Zimmermann, 286

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